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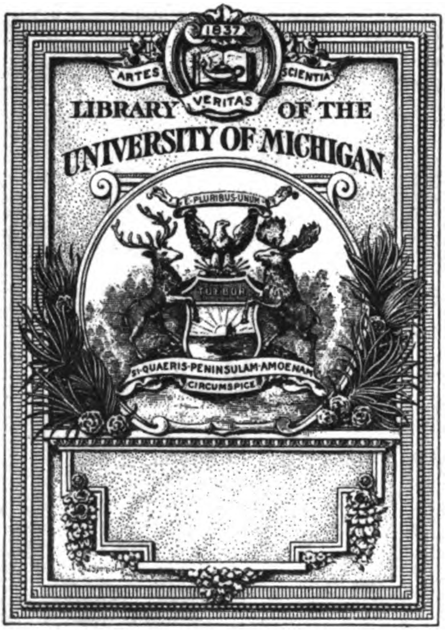

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THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL

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OF

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THE

BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW.

JANUARY, 1859.

PART FIRST.

Analytical and Critical Reviews.

REVIEW I.

1. *Papers relating to the Sanitary State of the People of England: being the Results of an Inquiry into the different Proportions of Death produced by certain Diseases in different Districts in England.* Communicated to the General Board of Health by EDWARD HEADLAM GREENHOW, M.D., L.R.C.P., Lecturer on Public Health at St. Thomas's Hospital, and Physician to the Western General Dispensary; with an Introductory Report by the Medical Officer of the Board, on the Preventability of certain kinds of Premature Death.—*London*, 1858. pp. 164.
2. *Report to the Honourable Commissioners of Sewers of the City of London on Sewage and Sewer Gases, and on the Ventilation of Sewers.* By HENRY LETHEBY, M.B., F.L.S., &c., Medical Officer of Health for the City of London, and Professor of Chemistry and Medical Jurisprudence in the College of the London Hospital.—*London*, 1858. pp. 84.
3. *The Influence of Sewer Emanations.* By J. HERBERT BARKER, M.D. London, F.R.C.S., Fellow and Fothergillian Gold Medallist of the Medical Society of London, &c. &c.—*London*, 1858. pp. 19.
4. *Fever in Agricultural Districts: being a Report on Cases of Fever occurring in the Parish of Great Horwood, in the County of Buckingham.* By HENRY W. ACLAND, M.D., F.R.S., Regius Professor of Medicine in the University of Oxford.—*Oxford* and *London*, 1858. pp. 23.
5. *Sanitary Science.* By WM. TINDAL ROBERTSON, M.D. Edinburgh; Ext. L.R.C.P. London; Physician to the General Hospital, Consulting Physician to the Dispensary and the Union Hospital, and Honorary Physician to the Midland Counties' Blind Asylum, Nottingham.—*London*, 1858. pp. 32.
6. *Second Report of the Commissioners of Her Majesty's Customs on the Customs.—London*, 1858. pp. 60.
7. *Reports of the Medical Officers of Health for the Metropolitan Districts.*—1856–57.

Notwithstanding all that has been said and written in this country upon sanitary subjects during the last thirty years, it must be confessed, more especially since the publication of the important work which heads our list, that the knowledge of our sanitary condition as a people is still most indefinite, and that still less defined is our knowledge of the measures best adapted to correct the causes of disease and death which are daily and hourly diminishing the efficiency, as well as curtailing the lives, of English men, women, and children, spreading among them not only physical pain and want, but moral perversion also. It is impossible to go over the publications to which we call attention, and not to feel how much remains to be done both as regards knowledge and practice in the matter of public health. Hitherto, sanitary efforts have been for the most part temporary and spasmodic; an alarm of cholera, or an outbreak of fever in some town or village, has roused the people or the authorities into a sanitary activity, often too great an activity, for the time being; but the cause of alarm disappears, and with it the reforms to which it had given momentary impulse; the old nuisances accumulate, and the people return to their old habits, little thinking, that though cholera or fever with their striking effects have gone, the same predisposing causes are still working, with as certain if less palpable power to sap the constitutions of themselves and of their children, and that convulsions, with the whole train of nervous diseases, pulmonary affections, with consumption at their head, are doing far more deadly work among them. That every casual malady, every epidemic of influenza,* every child-bed ailment, every accident, is liable to be rendered more severe, perhaps fatal, by these influences. Just so did our forefathers five centuries ago. They well knew that filth, impure air and water, were productive of disease, and when the black death of the fourteenth, the sweating sickness of the fifteenth, and the plague of the seventeenth centuries had done their fearful work, then, as now, were sanitary precautions taken, and many if not all the measures of this our nineteenth century resorted to, to be, like them, abandoned, or allowed to slumber, as soon as the present fear had passed away, and the remembrance of the horrors become dimmed.

We speak now of the country at large, for the lessons of the past have not been entirely thrown away, and thinking men, chiefly, justice compels us to say, belonging to the medical profession, have ever kept urging the adoption of a proper sanitary system, extended to every city and town, to every village and district of the kingdom. That their urging has availed somewhat, the list of works which heads this article testifies.

We learn much from the 'Reports' of the recently appointed "Medical Officers of Health;" but by far the most comprehensive and valuable exposition of the "sanitary condition of the people of England," is that of Dr. Headlam Greenhow, most valuable in itself, and rendered still more so by the introduction added to it by Mr. Simon, the well-known medical officer of the General Board of Health.

Dr. Greenhow's 'Papers,' and Mr. Simon's 'Report,' offer a stand-point from which we may regard the present state of sanitary science; they direct our attention, in the first place, to what we know of the causes of disease and mortality, and in the second, to how far and by what means that disease and mortality are to be prevented. The first of these questions will fully occupy the present article, the latter we reserve for future consideration.

We find that we are as yet but on the threshold of the science of hygiène, and Mr. Simon points out that special attention is required to some of the conclusions of Dr. Greenhow's 'Papers'

> "Because in the new light which they afford, the sanitary state of the people of England almost imperatively claims to be reconsidered as a whole, and because of the valuable evidence presented of how very much remains to be done in great part of England before the limits of practical preventability will be even distantly approached." (p. 3.)

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* Dr. Greenhow's Papers, p. 108.

Further on, Dr. Greenhow himself tells us—

> "That before sanitary science can make any further progress, it would be necessary to investigate the causes of excessive disease and mortality in a more analytical manner than has heretofore been done, for without a more precise and accurate acquaintance with their cause, it would be impossible to employ the most certain means of prevention against the diseases which so largely aggravate the death-rates of certain districts." (p. 132.)

Indeed, nothing strikes the mind more on the perusal of these papers, than the vast amount of work to be done before we can arrive at an accurate opinion respecting causes, and how very unstable are many of the foundations on which rest present opinions and theories. One in point: It has almost been received as an established fact, that the immense mortality of soldiers stationed in this country, and especially of the Foot Guards in London, from pulmonary disease, was due to overcrowding and other causes, which occasioned them to breathe all night and almost all day an impure air. "The soldier sleeps in a fetid and unwholesome atmosphere, the habitual breathing of which, though producing, for the most part, no direct immediate effects, probably lays the seeds of that pulmonary disease which is so fatal in the British army."* At the recent meeting of the British Association, Mr. Neison read a paper,† tending to disprove the fact assumed, that pulmonary disease does arise from such a cause, and that we must look for its great prevalence and mortality among the troops elsewhere. How far Mr. Neison sustains his point remains to be seen, but the fact of its being mooted by so high an authority is strongly illustrative of the present unsettled state of sanitary science. Moreover, the point is one of special interest here, Dr. Greenhow attaching the greatest importance to the pulmonary death-rates of the various districts as indicative of their general sanitary state. It may be remarked that the term "death-rate" belongs peculiarly to the science of sanitation, having taken its origin from reports on health; and now, the death-rate of a town or district is the dial-plate of its sanitary working. The entire death-rate, however, gives but a rough idea of the causes which are at work to cut short human life; it requires to be analysed, and the results classified and tabulated, not in one, but in various combinations, as we see done in Dr. Greenhow's 'Papers,' before it yields the novel and valuable information which can be extracted from it. Certain diseases, certain localities, certain occupations, difference in sex or age, all have their own special death-rates, which must be compared one with another, and must be brought into contrast and comparison with contingent circumstances and influences. For convenience, throughout Mr. Simon's 'Report,' and Dr. Greenhow's 'Papers,' the number 100,000 has been adopted as the standard of comparison for the various death-rates; this standard being adhered to even when the population of a town or district does not nearly reach it, the ratios being calculated proportionally.

Mr. Simon opens his Report by distinguishing from the 628 registration districts into which England is divided, sixty-four districts which, compared with the others, present the best sanitary condition. In these sixty-four districts, with a combined population of one million, the annual death-rate runs from 1500 to 1700 per 100,000; the average death-rate for England at large being 2226, rising in some notorious districts to 3100, 3300, or even 3600. Disregarding for the present the latter high rates, we may well ask the question why, if in some districts but from 15 to 17 persons die per 1000 annually, in others the mortality rises to 22 or more per 1000?

Moreover, although a mortality of from 1500 to 1700 is taken as a sanitary standard, it is so simply for the reason that it is, *for the present*, with one small exception, the best we can get; we have amongst us no model community to furnish us with, if we may so call it, a model death-rate. The exception alluded to is that of the distant and isolated Faroe Islands. In this small community the largest proportion of deaths occurs in the decenniad between eighty and ninety years of age, the death-rate being as low as 1250. Of this rate, which is the lowest we can find, Mr. Simon makes great use, constituting it, as it were, the key-note of our entire subject. Albeit, it is more than doubtful whether, even in this case, anything like sanitary order prevails. Mr. Robert Chambers, in his 'Tracings of Iceland and the Faroe Islands,' tells us that "round nearly every house is a black and fetid sewer," the houses themselves being "small and stifling," and the adjacent rill defiled with "washings of clothes and eviscerations of fish." Well may it be asked, if we have this low mortality amid all these fosterers of disease, what might we not have under a better system? Verily we have not yet found our lowest death-rate. Nevertheless, what we have will serve our purpose for the time.

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* Health of the English Soldier, Sanitary Review, April, 1858.
† Evening Mail, September 28th, 1858.

As above stated, a mortality of 1500 per 100,000 is the highest sanitary status to be found in the English registration districts; for an isolated community, with certain conditions favourable to health and longevity, the mortality falls as low as 1250, leaving between the two lowest death-rates a margin of 250. This mortality of 250 Mr. Simon assumes is due to non-preventable causes of disease and death—that is to say, causes which cannot, *at present, at least*, be prevented from operating upon us as a large community, but which are escaped by people so limited in number and so isolated as the Faroe Islanders. The non-preventable causes of premature death are enumerated as congenital and hereditary influence; contagions of small-pox, hooping-cough, measles, and scarlatina; privation, accidental injuries and violence, vice and intemperance. Passing the lowest English death-rate of 1500, or at most 1700, all mortality in excess of these numbers we are to consider as preventable, and as due to

> "Diseases of which the very essence is filth; diseases which have no local habitation except where putrefiable air or putrefiable water furnishes means for their rise and propagation; diseases against which there may be found a complete security in the cultivation of public and private cleanliness." (p. 9.)

Dr. Greenhow commences his inquiries by the selection of one hundred and five registration districts, which should "comprise a variety of healthy and of unhealthy places, each of them distinguished by its positive character, or some peculiarity in the industrial employment of its inhabitants." (p. 19.) Next he selects the diseases to be investigated, and in this our author appears to have taken a sufficiently extensive field for an initiatory inquiry; so extensive, indeed, that he considers "the entire subject as barely opened" by the present investigation. The period selected includes the seven years 1848–54, the census of 1851 occurring midway; this length of time being deemed sufficient "to obviate the fluctuations that are liable to occur from year to year."

The diseases investigated are arranged in ten groups:—

> "A. Pulmonary affections, including phthisis.
> B. Contagious diseases, including small-pox, measles, scarlatina, and hooping-cough.
> C. Alvine flux, including diarrhœa, dysentery, and cholera.
> D. Typhus and erysipelas.
> E. Croup, influenza, and ague.
> F. Strumous diseases.
> G. Nervous diseases of children, including convulsions, hydrocephalus, and teething.
> H. Apoplexy and paralysis.
> I. Rheumatic fever and rheumatism.
> K. Carbuncle and phlegmon." (p. 24.)

> "Pulmonary diseases, alvine flux, and the nervous diseases of children, are the classes of disease which are, both absolutely and relatively, the chief causes of high death-rates . . . . It is to the investigation of their origin that sanitary inquiries may most advantageously be directed. It is from devising and adopting measures for the removal of their

> causes, that we may most confidently hope for an amelioration in the public health. Any measures that should be successfully adopted for diminishing the mortality produced by these diseases, would undoubtedly diminish that from other diseases likewise. Certain of the contagious diseases, although their amount might be undiminished, would at least fall with diminished intensity upon a healthier population; and the same would probably hold true of other diseases likewise." (p. 131.)

If these opinions are correct—and we see no reason to doubt them—they must have a very important bearing upon the question of sanitary reform, and the measures to be adopted.

The class of pulmonary diseases is first brought under notice; and the most prominent fact which appears is the absence of "uniform relation between the male and female death-rates,"—the male-rate exceeding the female in the proportion of 100 to 94 in the country generally. In three of the registration districts, however, this is reversed. In these three divisions, the Eastern Counties, the South Midland Counties, and the North Midland Counties, few males are engaged in manufactures, but the number engaged in agriculture considerably exceeds the average. On the other hand, a good proportion of the adult females are engaged in industrial manufacturing pursuits, these being chiefly conducted at their own homes. Albeit, the three districts in question contrast favourably in the matter of health generally with the other districts of the kingdom, we have now got the fact that, in three healthy districts, with a general pulmonary death-rate not excessive, the female-rate, reversing the usual order, exceeds the male, the exception occurring coincident with the difference in industrial pursuit above alluded to. Mark, however, the following:—Buckinghamshire, Hertfordshire, and Bedfordshire, with almost the same number of adult males engaged in agriculture as Lincolnshire, have a pulmonary death-rate one fourth larger than the latter county. Cambridgeshire, though with an agricultural element second only, and that in very slight degree, to Lincolnshire, and with scarcely any manufacturing element at all, has a pulmonary death-rate proportionally larger, not only than Lincolnshire, but larger than that of any of the counties mentioned.

Again, in Nottinghamshire the female pulmonary death loss exceeds the male almost as much as it does in Bedfordshire; whilst the proportion of women employed in manufactures is only half as many, and materially less than the number in Buckinghamshire, which county Nottinghamshire also approximates in the proportion of its male and female pulmonary death-rates. Dr. Greenhow's remarks upon the foregoing are:—

> "Here, then, is a case which seems at variance with the conclusions that appeared to spring so evidently from the facts previously recorded. No explanation of the fact is to be gathered from the evidence, so far as it has hitherto been considered. Probably the discrepancy will be explained when the subject shall have been more fully investigated." (p. 29.)

And a little further on:—

> "Thus, although there does appear to be some general relation between the comparative death loss of different counties from pulmonary affections, and the closer or more diffused aggregation of their inhabitants, the per-centage of urban population, and the nature of the prevailing industrial employment, this relation is neither constant nor exact." (p. 29.)

Well may it be elsewhere observed, that "the causes which modify public health are of a complicated nature, and still require much investigation." "When large districts and more numerous populations are taken as subjects of comparison, their very extent and diversity offer various sources of fallacy and confusion; and we are thrown back upon more circumscribed inquiries for more accurate deductions."

In Staffordshire the male occupations are of a very mixed character, including the manufacture of earthenware, the female employments also taking up three and a half per cent. for this branch of industry, and nine per cent. in manufacturing pursuits generally. The general pulmonary death-rate is above the average of England and Wales; moreover, the difference between the male and female death-rate is small. In a subsequent observation it is shown that the chief pulmonary (phthisical) mortality among the earthenware workers appears to arise from the fabrication of a particular kind of fine pottery, attended with evolution of fine dust.

Illustrative of the difficulty of forming definite conclusions from the inquiries already made, we come at a more advanced stage of the Papers, upon a curious anomaly connected with the pottery district death-rate. Wolstanton and Stoke-upon-Trent are closely adjacent to one another, the former being much the most thinly populated, and having fewer adults, either male or female, engaged in the earthenware manufactures; yet Wolstanton has a higher pulmonary death-rate than Stoke: "the question could only be satisfactorily solved by a careful investigation of this subject in the pottery district." (p. 73.)

Again, the North-Western Registration Division includes the counties of Lancashire and Cheshire, with the West Riding of Yorkshire, in fact, "the great manufacturing centre of England." Comparing the male pulmonary death-rate of the above with that of England and Wales, and with that of the three agricultural counties of Lincoln, Hereford, and Cambridge, we find it considerably in excess, as may be seen in the following selection from Table VII. of the work before us:—

Name of County.	Death-rate from pulmonary affections per 100,000 males.
Lincoln	367
Hereford	444
Cambridge	495
Cheshire	558
West Riding	577
Lancashire	722
England and Wales	569

In the above table Cheshire contrasts rather favourably with England and Wales, but the contrast is only favourable as regards the male sex, for the mean pulmonary mortality of both sexes is higher in Cheshire than in the country generally.

Lancashire shows the highest pulmonary mortality, higher than the West Riding, and yet in the latter a larger proportion of males are engaged in manufacturing industry than in the former,—a sufficient proof, perhaps, that some other cause, in addition to manufacturing employment, is at work to produce the mortality. Lancashire contains Liverpool, and

> "The mortality from pulmonary disease is higher in Liverpool than in any other district to which this investigation has extended. The pulmonary mortality of Lancashire is therefore in some measure augmented by the high pulmonary death loss of Liverpool; and the high pulmonary death loss of Liverpool is not at all attributable to the employment of its inhabitants in manufactures, but must be ascribed to some other cause not apparent at present." (p. 32.)

If we look at the density of the population of Liverpool as compared with that of other towns and cities in the kingdom—for example, Liverpool having 34,000 persons to the square mile, while London has little more than 19,000—we can scarcely wonder that Liverpool has not only a very high general death-rate, but we are also ready to connect its high pulmonary death loss with the same cause; justly, perhaps, in some degree, but we cannot altogether, when we find a few pages further on that Hull, with a population of 13,750 per square mile, contrasts favourably as regards pulmonary death-rate with Ipswich, which has but 2493 persons per square mile; and with Gravesend, which has but 6908 in the same space, both sea-port towns; and that the same favourable comparison holds good with inland places, even such as Macclesfield, with only 493 persons per square mile, and indeed with many other places.

We might here pass on to review what we learn from Dr. Greenhow of the influence of density of population generally on pulmonary death-rates, but we cannot omit an interesting and valuable comparison between the manufacturing districts of Cheshire, Lancashire, and the West Riding, and the midland manufacturing counties of Leicester and Nottingham. In the first three the factory system prevails, in the last two more work is done by the operatives at their own homes. In the first three the pulmonary death-rate is larger than in the last two, in which, however, as in Cheshire, the female death loss is the larger; it should be remarked, however, that in Cheshire, which offers fair ground of comparison, the rate is higher for both sexes. It might be fair to conclude that the factory system, as it works at present, is more prejudicial to life, as far as pulmonary disease is concerned, than where work is done at home. It may be so, but many contingent circumstances, only to be brought out by minute inquiry, must be taken into consideration.

The fact seems to come out strikingly in these investigations, that one marked indubitable cause of pulmonary disease, and especially of phthisis, is the inhalation of fine *hard* dust. This seems to be the case in Warwickshire, especially where the metallic manufactures are of a kind to give rise to such dust, more than where the work is of a coarser description; we have observed the same fact where fine pottery is made; it is well known in the hardware manufactures of Yorkshire, and the mortality of miners where the ore lies in dry sandstone is said to be from the same cause. On the other hand, where the dust inhaled is of a soft character, as in woollen, flax, and cotton factories, asthma and chronic bronchitis are more prevalent. Pulmonary consumption, it is true, is also prevalent, but it must be a question whether this may not be the result of the deterioration of the constitution generally under the influence of the manufacturing system, as much as the consequence of the direct irritation of the lungs; at least the increased pulmonary death-rates in those places where females are engaged in industrial employments—such as lace-making and straw-bonnet making—where no dust is evolved, tend to support the proposition. Thus, whilst investigating the effect of manufactures as productive of pulmonary disease, it should not be lost sight of, that if in some cases the disease undoubtedly commences in the lungs; possibly, probably in others, as in the factory operatives, it has its origin in the constitution. There can be no doubt that, as expressed by Mr. Simon,

> "In proportion as the male and female populations are severally attracted to indoor branches of industry, in such proportion, other things being equal, their respective death-rates by phthisis are increased." (p. 28.)

In the lace-making districts just alluded to, the female death-loss seems always to exceed the male.

> "The pulmonary death-rate is usually excessive in towns where both males and females are largely employed in the manufacture of textile fabrics, but the difference in the mortality of the sexes is rarely great." (p. 34.)

The woollen districts have a somewhat lower mortality than the cotton and silk, the disease excited seems rather of the form of chronic bronchitis and asthma.

The pulmonary mortality of the cotton districts is decidedly high, Manchester, as we might expect, having the greatest loss. The difference in the death-rates, however, of males and females, is slight, both being largely engaged in the industrial occupations of the place. This is remarkable in comparison with Liverpool, which, with a higher mortality still, does not show it so much in its female population, who are not engaged in any special employment. Preston stands next to Manchester in pulmonary insalubrity, higher than Chorlton and Salford, and yet the people are much less thick upon the ground than in the latter places, and the proportion of agricultural labourers is higher, but the paupers, as in Saffron Waldon, are more numerous.

It is difficult to pass over a page of the elaborate and industriously compiled work before us, without finding matter for comment or extract. Space confines us to a selection of the most striking observations and marked exceptional facts. Of the latter, Saffron Waldon, just mentioned, is an instance. Of the hundred and five districts under investigation, it has the highest pulmonary death-rate; and the only probable reason to be assigned is, that it has the greatest amount of pauperism. Another exception we find in Hendon, near London, a purely agricultural district, yet presenting an exceptionally high pulmonary death loss. The most probable reason for this adduced by Dr. Greenhow, is, that it is a great hay growing district, the labourers being for the most part employed as hay cutters, carters, &c., and exposed continually to the fine grit or dust thrown off from the hay. It may here be added, that one probable cause of high phthisical death-rate, especially in hamlets, is the intermarriage of the people. In our own experience, at least, it is not uncommon to find villages in England composed of families of which a large proportion bear not only the same name, but the same stamp of family resemblance, and the same tendency to disease, especially of a scrofulous character.

The influence of the occupation of mining upon the health and life is full of interest, as well as most important to this country of coal and iron. The facts connected with it are significant. The employment is generally carried on in salubrious country districts. In lead mining, which seems unusually pernicious, the differences in the pulmonary death-rate are well marked, and correspond pretty nearly with the proportion of men engaged in the pursuit. The district of Alston, the most exclusively lead mining district in England, is specially noticed. Situated in a most salubrious country district, the pulmonary death-rate exceeds that of Liverpool; and this excess is almost entirely due to the adult male mortality, the mortality of the infant population of Alston being immensely below that of Liverpool. Alston has more widows, proportionally, than any other place in the kingdom. Moreover, the female death-rate from phthisis is also high, although none of the sex are engaged in mining. This may be due to acquired hereditary tendency, presenting us with another of those influences which complicate the calculations. Tin and copper mining both seem to increase the pulmonary death loss among those engaged; the latter, as in the case of Redruth, gives a male mortality considerably in excess of the average. Coal mining, on the other hand, does not appear to affect its workers in the same way. Easington and Houghton-le-Spring, both purely coal districts, have comparatively low pulmonary death loss. An exception, nevertheless, meets us in the Welsh coal mining districts, which, as well as the slate-quarrying district of Carnarvon, have a high pulmonary death loss. So little explicable, too, is the cause, that Dr. Greenhow is fain to fall back upon the question of race as a predisponent, especially as this mortality prevails likewise among the females, who do not engage in the staple occupations.

Passing from the consideration of the effect of industrial employment upon the general pulmonary death-rate, to the effect of density of population, we find very indefinite results. The discrepant example of Hull in comparison with other towns, has already been noticed. As a rule, the greatest mortality appears to prevail in urban, and consequently crowded, districts, but to this there are many exceptions, which Dr. Greenhow thus endeavours to explain:—

> "It is indeed certain that the mode usually adopted, of measuring the aggregate distribution of the population by the number of persons on a square mile or acre, fails to afford a true estimate of density in a sanitary sense. Some town districts have a considerable portion of outlying country district attached to them, while the density having been calculated for the entire district, of course diminishes the average number of persons on a given space, and yet the urban portion may be very densely populated. Other places, where the labouring classes chiefly dwell in separate cottages, may present a smaller superficial density of population, and

yet, from deficiency of space within the dwellings, their inhabitants may suffer from all the worst evils of an over-crowded population." (p. 42.)

Contrasting the discrepant pulmonary death-rates and population-densities of Hull, Macclesfield, and Huddersfield, we meet the following most important remarks:—

> "For these three towns there is an entire absence of definite relations between the proportion of deaths from this particular class of diseases, and the more or less urban character of the several places. Let it not be supposed that I desire altogether to ignore the urban element in the causation of pulmonary affections. I am fully sensible its influence is considerable, and have already said it is always evident; but we must look to other causes as at least largely auxiliary to town influences in the production of the very remarkable diversities which have just been enumerated." (p. 45.)

In the Report on the 'Sanitary State of Clerkenwell,' by Dr Griffiths, one of the Medical Officers of Health in the metropolis, we find some observations which bear upon the above. After giving a "mortality table," showing the number of persons living, to one death in the districts of the metropolis, he adds:

> "Here we find that Hampstead, with its pure atmosphere, its beautiful trees and fields, its open gravelly soil, its great elevation, and its population of only five persons an acre, has the same mortality as the city of London, with its atmosphere of smoke, its population of 128 persons to the acre," &c., &c. (p. 24.)

The investigations before us entirely disprove any supposed immunity of the southern districts of England from pulmonary disease, at least as compared with the northern parts of the kingdom. The fact, however, that chest affections are less frequent and fatal in the keen air of Northumberland and Durham, than in the more relaxing atmosphere of Devon or Hampshire, would have been thought more remarkable a few years ago. The hygienics of consumption, especially, are not what they were.

Contagious diseases, however important, do not occupy as prominent a place in the present inquiries as they do in most death reports, for although they claim a not inconsiderable proportion of the mortality as their own—97 in every thousand deaths in England and Wales—they "are not the principal causes of high death-rates." (p. 130.) Small-pox is chiefly remarkable for the irregularity in its prevalence, and, consequently, in the mortality from the disease; this is just what we might expect as the result of the uncertain and ill-regulated system of vaccination as practised at present in England. Measles and scarlet fever are marked by the absence of definite relations between their mortality and the general death-rate. Measles tend to greater fatality in places where the pulmonary death-rate is high, and even joined with scarlet fever and hooping cough, have nearly double the mortality in the crowded north-western districts that they have in the comparatively thinly-peopled south and south-east of England. Doubtless the facile transmission of contagious disease amid a thickly-planted population contributes much to its prevalence and mortality, but there can be little doubt, as suggested by Mr. Simon,* that a general weakness of constitution, conjoined with defective sanitary arrangements, "greatly aggravates the fatality of the infectious diseases in question."

Alvine flux includes diarrhœa, dysentery, and cholera. With respect to the second of these it may be asked, whether the "dysentery" we see so frequently reported, is true dysentery with ulcerative inflammation of the colon, or whether it is not, in many cases, simply aggravated diarrhœa? There is perhaps a little vagueness in the use of the term. The connexion between alvine fluxes and impurities of every kind, impure air, water, and food, is too fully established to require much comment. Specific influence is often, doubtless, it may be always a *co-originator*; but filth, if not the sole parent, is at least the foster-mother of these diseases. Apart from the direct influence of impurities, the mortality from alvine flux in various districts and towns, seems little amenable to any rule we at present know. Generally speaking, where diarrhœa has been prevalent, there cholera has likewise prevailed; but to this there are many exceptions. Birmingham, Nottingham, Leicester, have a high diarrhœal death-rate, but have been remarkably exempt from cholera. Gateshead and Abergavenny reverse the fact.

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* p. 29.

Cholera was of course most fatal at the epidemic periods of 1849, 1853–54. In a few districts, as London, Newcastle-on-Tyne, and Gateshead, it prevailed severely at both periods, but was much less fatal at the latter than the former visitation. Plymouth, Bradford, and Wolverhampton, are examples of this fact. A few districts in which it had been severe at the former visitation, entirely escaped at the latter period; this was remarkably the case with Tynemouth. In the cholera visitation of 1848–49, that town suffered severely, losing 463 out of its population of 64,248; warned by this, active sanitary measures were adopted, and when, in 1852, cholera was again epidemic, those exertions were redoubled. 1853 saw Newcastle and Gateshead suffering "from the most terrible outbreak of cholera yet experienced in England;" whilst Tynemouth, eight miles lower down the river, was exempt, although numerous cases of diarrhœa plainly showed that over it the choleraic influence extended, but found no congenial soil.

Dr. Barclay, Medical Officer of Health for Chelsea, tells us that "the average level of this extensive parish is but a few feet above high-water mark, but that the district of Kensal, with the advantage of at least fifty feet higher elevation than the rest of the parish, and an 'open airy situation,' has a death-rate from epidemic disease, principally diarrhœa, nearly double that of any other district in the parish; whilst, excluding epidemic disease, it is actually the healthiest of the Chelsea districts. Inefficient drainage, fæcal fermentation, and the impregnation of the atmosphere with unwholesome emanations from foul drains, ditches, and cesspools," are the words in which Dr. Barclay describes the Kensal sanitary arrangements. After such a description we cease to wonder at the death-rate, and accept it as the natural consequence of the evils described. The people breathe an atmosphere laden with impurity, and die of epidemic disease. Not less certainly do they die if they drink a water saturated with sewage.

We are indebted to Dr. Lankester, Officer of Health to the Westminster district,* for the full exposition he gives in his Report, and for the abundant testimony he adduces to support his exposition, of the injurious effect arising from the use of the "surface-well waters" of London, containing as they do organic matters "of precisely the same nature as those found in rivers which are the receptacles of house sewage" (p. 16), and saline matters, common salt, ammonia, the phosphates, nitric acid, &c., all indicative of animal excretion. Carbonic acid is largely present in these surface waters, and from the pleasant drinking qualities it imparts to them, actually makes the most impure waters the most popular, and the most dangerous. Did space permit, we would gladly cite some of Dr. Lankester's proof-cases, but for these we must refer to the pamphlet itself. We must, too, content ourselves with the simple acknowledgment of the labours of the late Dr. Snow in connexion with this very subject, the propagation, if not the origination of disease, by polluted drinking water. He may at times have pushed his facts and his deductions too far, but he was, unquestionably, the prime mover of inquiry in this direction. Dr. Liddle, Officer of Health to the Whitechapel district, has the following (p. 13): "In a street at Salford containing ninety houses, 25 deaths from cholera occurred in thirty of these houses, the inhabitants of which drank water from a well into which a sewer had leaked; in the remaining sixty houses, where pure water was drunk, there were 11 cases of diarrhœa only, and no deaths."

We have almost proved enough; but an instance quoted by Mr. Simon is so strongly marked, partakes so much, as he expresses it, of the nature of a gigantic experiment, that we cannot avoid quoting it in his own words. It occurred during two epidemics in the southern districts of London:—

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* Second Annual Report.

> "These districts (comprising nearly a fifth of the population of the Metropolis) have been notorious for the great severity with which cholera has visited them . . . . Throughout these districts, during the epidemics of 1853–4, there were distributed two different qualities of water; so that one large population was drinking a tolerably good water, another large population an exceedingly foul water; while in all other respects these two populations (being intermixed in the same districts, and even in the same streets of these districts) were living under precisely similar social and sanitary circumstances. And when, at the end of the epidemic period, the death-rates of these populations were compared, it was found that the cholera mortality in the houses supplied by the bad water had been three and a half times as great as in the houses supplied by the better water. This proof of the fatal influence of foul water was rendered still stronger by reference to what had occurred in the epidemic of 1848–9. For on that occasion the circumstances of the two populations were to some extent reversed. That company which, during the later epidemic, gave the better water, had given during the earlier epidemic even a worse water than its rival's; and the population supplied by it had at that time suffered a proportionate cholera mortality. So that the consequence of an improvement made by this water company in the interval between the two epidemics was, that whereas, in the epidemic of 1848–9 there had died 1925 of their tenants, there died in the epidemic of 1853–4 only 611; while among the tenants of the rival company (whose supply between the two epidemics had been worse instead of better) the deaths, which in 1848–9 were 2880, had in 1853–4 increased to 3476. And when these numbers are made proportionate to the populations or tenantries concerned in the two periods respectively, it is found that the cholera death-rates per 10,000 tenants of the companies were about as follows:—for those who in 1848–9 drank the worse water, 125; for their neighbours, who in the same epidemic drank a water somewhat less impure, 118; for those who in 1853–4 drank the worst water which had been supplied, 130; for those who in this epidemic drank a comparatively clear water, 37. The quality of water which (as is illustrated in the first three of these numbers) has produced such fatal results in the metropolis, causing two-thirds of the cholera deaths in those parts of London which have most severely suffered from the disease, has been river-water polluted by town-drainage—water pumped from the Thames within range of the sewage of London—water which, according to the concurrent testimony of chemical and microscopical observers, was abundantly charged with matters in course of putrefactive change." (Mr. Simon's "Report," p. 14.)

From diarrhœal to febrile disease the transition is natural as short; at least, typhoid fever, with its diarrhœal tendency, gives us the link which connects two species—we might almost, botanically speaking, call them "varieties"—of disease, having the same origin, filth, polluted water, privation; the last, perhaps, more a predisponent than an actual cause. It is remarkable how, in "sanitary reports," we find the above causes and their effects, fever (typhoid) and diarrhœa, associated. Dr. Letheby, in his report on "Sewage and Sewer Gases," gives the somewhat notorious case of Croydon, where, in consequence of a new but badly constructed system of drainage, very shortly there was produced "an alarming outbreak of fever, diarrhœa and dysentery;" adding, that Dr. Carpenter, of Croydon, informs him, "that even now he can tell where the pipes are stopped by the occurrence of diarrhœa or fever in the houses through which the foul gases are forced." In one respect, as justly remarked by Dr. Barnes in his report on St. Leonard's, Shoreditch, typhoid fever "is a better test of the sanitary state of a town than diarrhœa;" it is "more strictly dependent upon local conditions," and "is less influenced by season and atmospheric changes." The distinctions between typhoid and typhus fevers are not generally, that is popularly, known, and for this reason Mr. Simon has felt compelled to treat them as "a single form of disease," as they appear in the registration returns. This necessity is unfortunate, as tending to confound a contagious disease with one almost* non-contagious, and in many respects different both as to origin and diffusion, circumstances which can scarcely fail to introduce an element of fallacy into deductions drawn from the returns of disease under the head of "fever."

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* Not long since the "almost" would have been omitted by us; but circumstances which have occurred under our notice in a serious outbreak of typhoid fever, have led us to doubt at least the absolute non-contagiousness of the disease. Dr. Barnes seems to hold it as "no doubt true that typhoid fever is occasionally communicated by contagion" (Report, p. 8); and Dr. Acland, in his pamphlet on Fever in Agricultural Districts, considers the fact proved.

So notoriously are outbreaks of fever connected with deficient sanitary arrangement,—typhoid with impure water and air, typhus more generally with overcrowding and putrefying emanations from the living body,—that it is unnecessary to enlarge upon the subject further than to state the significant fact that typhus, the old genuine typhus of Edinburgh and other large towns, will die out, in cases which may succeed, if any do, a case imported into a perfectly healthy situation.

The best defined special fact which Dr. Greenhow seems to havé elicited in his fever investigations is, that although generally

> "The differences in fever death-rates in males and females are very considerable and very irregular, the places in which the female death loss is proportionally highest, and most largely exceeds the male death loss, are counties in which the proportion of females engaged in manufactures is very much greater than that of the males." (Dr. Greenhow's 'Papers,' p. 100.)

The same fact we find elicited with respect to erysipelas, the female death loss from that disease being

> "Proportionally higher than the male in Bedfordshire, where the men are chiefly employed in agriculture, but thirty-three per cent. of the women are employed in manufactures; and likewise in Leicestershire and Nottinghamshire, where the women are largely employed in the manufacture of lace." ('Papers,' p. 105.)

Although fever causes a larger proportion of deaths in unhealthy than in healthy places, it yet holds a subordinate position to several other causes of death in the production of a high death-rate; moreover, the death-rate seems to have no definite relation either to the density or the amount of population. Thus:—

> "The mortality from fever in the large and densely-peopled cities of Manchester and Liverpool is rather less in proportion to their population than the mortality in the much less thickly-peopled towns of Blackburn and Wolverhampton; lower in Blackburn and Wolverhampton than in the smaller and much less dense population of Leighton Buzzard, Merthyr-Tydfil, and Abergavenny." ('Papers,' p. 192.)

Again:—

> "Although the 'general death-rates are lower,' the mortality from fever is larger in proportion to the population, in the South Midland counties and in Monmouthshire and Wales, than in either London or the South-Western counties." (Ibid. p. 100.)

These facts are quite at variance with the commonly received idea that the zymotic diseases, and fever especially, are more prevalent and more fatal in proportion to the density of population. Equally erroneous proves to be the opinion "that fever is especially a disease of mature life." Of 10,995 deaths that occurred "during the Septennial period in twenty of the districts comprised in that investigation, with a population of 1,297,832, the deaths of persons under twenty amounted to 4700." Although fever will doubtless spread more widely amid a crowded population, and find more force for its origin, it does not require large aggregations of impurity for its development; a neglected sewer, ash-pit, or cesspool, an unsound soil-pipe,* it matters not whether in town or country, may be all that is required. The farm-house or labourer's cottage, nay, the mansion of the squire, though situated in the most healthy district, if putrefying animal and vegetable refuse is permitted to taint its *immediate* atmosphere, is as liable to be invaded by fever as the town-dwelling in the close alley. The taint of the atmosphere in the vicinity of fermenting and decaying matter proceeds chiefly from the gases, but partly also from organic matter "in a state of active decomposition." Dr. Letheby tells us that he cannot speak with certainty "as to the properties and effects of the organic vapour which is contained in sewer gases," but both he and

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* See a case quoted by Dr. Letheby, Sewage and Sewer Gases, p. 51.

Dr. Herbert Barker* most thoroughly demonstrate, not only how certainly sewage gas affects both health and life, but how small a proportion of the gases are capable of extinguishing life, or of giving rise to the phenomena of febrile disease according to the intensity and period of their administration. The demonstrations of Dr. Barker are chiefly in the way of experiment. Exposure, by means of a suitable and ingenious contrivance, to the gas of a large cesspool, proved fatal to a mouse, and produced in larger animals, such as dogs, a series of symptoms analogous to those of febrile disease. Sulphuretted hydrogen, carbonic acid, and ammonia, the principal and most deleterious components of sewage gas, were experimented with separately. Of the first, less than two per cent. in the air killed a puppy in two minutes and a half, and so small a proportion as 0·428 killed a second within the hour. A dog exposed to an atmosphere containing 0·206 per cent. of the gas died in nine hours and a half, but another, in the same description of atmosphere, suffered at first, but soon recovered; others were more violently affected in a less contaminated air. Ammonia and its salts produce "what may be unhesitatingly considered typhoid symptoms,"† and prostration and diarrhœa followed the inhalation of carbonic acid in small proportions. In short, as Dr. Barker concludes:—

> "The symptoms which have thus been noticed as resulting from the inhalation of sulphuretted hydrogen, sulphate of ammonia, and carbonic acid, are sufficient to account for the effects arising from cesspool effluvia, without seeking for any further product from such emanations." (p. 19.)

The most important result of these experiments is the demonstration of the minute quantity of the deleterious gas which is sufficient to produce disease, a fact, moreover, evinced practically by the researches of Dr. Letheby.‡ In the cutting of the Thames Tunnel the men "suffered severely" from the gas, "although the proportion of it in the air was hardly to be discovered by lead paper, and could not therefore have exceeded one part in 100,000." (p. 33.) Emaciation, low fever, and death were the results in several cases.

> "Another remarkable instance of the same kind of poisoning occurred last summer at Clayton Moor, near Whitehaven, where there is a row of small cottages built on the refuse slag of some neighbouring iron-furnaces. The houses are occupied by the workmen and their families, who for some time had been annoyed by a bad odour, which pervaded the lower rooms. Suddenly, however, in the month of June of last year, the smell became unusually offensive; and in the course of two days thirty of the inhabitants were made seriously ill by it. The attack was remarkably sudden, as if a poison had been at work. In one of the houses there was a family of seven persons, consisting of the husband and wife, and five children. They retired to rest in their usual health, but in the morning two of them were dead, and the others were in a state of profound insensibility. Before the day was over another of them died, and in the course of a week a fourth. In a second case, a strong, healthy man came home from his night-work and went to bed; but an hour had hardly elapsed, when he also was found dead. And in a sixth instance, a child was taken ill in the morning and was a corpse at night. An inquiry was instituted for the purpose of discovering the cause of the mischief, and Dr. Taylor came to the conclusion that it was sulphuretted hydrogen, generated by the action of water on the refuse slag upon which the houses were built. If so, it is a remarkable instance of the poisonous action of this gas; for those who examined the air of the rooms declare that the test of lead paper failed to show the presence of the poison, except in mere traces—that is, in quantities which could not have been greater than one part in 100,000 of air, for such a proportion would have been easily recognised if it had been present."§

Clear as may be the connexion between impurity and zymotic disease, small as may be the amount of impurity requisite to work the mischief, the question yet remains to be solved, why the outbreaks of disease are only occasional in most instances, often few and far between, but yet the pollutions are constant? As Dr. Ackland says of Horwood,* "there are doubtless many sanitary errors, but scarce any that may not be found in hundreds of other rural parishes." The same might be said of a village which, under our observation, has suffered from a severe visitation of typhoid fever during the last two months—a village, moreover, in which fever cannot be remembered to have occurred before, and in which the sanitary conditions, or perhaps deficiencies, have been the same throughout. The materies morbi, the miasma of fever, if such exists, has as yet eluded the test of the chemist, and the microscope of the physician; but whether it be discoverable or not, of this we may be certain, that it cannot act, perhaps cannot be developed, in the absence of impurity. This is our fact—a fact so important, so simple, so well ascertained, that we can only marvel how little has yet been its practical application. Death-rates may show where that application is needed most; they cannot show more plainly than we know it, the necessity for its immediate and universal application throughout the length and breadth of Britain.

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* The Influence of Sewer Emanations.
† Essay on the Coagulation of the Blood, by B. R. Richardson, M.D., p. 345.
‡ Sewage and Sewer Gases, p. 33.
§ Sewage and Sewer Gases, p. 35.

It has usually been thought, not only popularly, but medically, that croup is more prevalent and fatal "in damp places, by the sides of rivers and marshes, than in drier and more elevated situations." The tables in Dr. Greenhow's work disprove the idea. The Eastern and South-eastern counties, with their fens and abundant moisture, have a lower death-rate from croup than Monmouthshire and Wales, the North-western, or the West-midland counties. Cambridgeshire, Lincolnshire, and Durham, suffer less than Staffordshire, Cheshire, and Lancashire. It must, however, be remembered, when considering the mortality from croup, especially in connexion with such districts as those last named, that probably many cases of spasmodic croup are recorded under the present head, and that we have then one of the nervous diseases of children in which "the chief manufacturing towns sustain the highest mortality, and the more purely agricultural counties the lowest." (p. 115.) The tables confirm the generally received opinion that males suffer more from croup than females.

Influenza, which is included by Dr. Greenhow in the same division as croup, is not remarkable for a high death-rate; but where the death-loss from the epidemic is heaviest, there do we find the most unhealthy conditions of a town or district, people debilitated from any other cause most readily succumbing to the attack of a disease not characterized by any necessary fatality. In this we have an unique example. A purely epidemic malady, mild in character, affects the whole community, but only severely and fatally where the causes and fosterers of zymotic diseases generally exist in strength.

A singular fact with regard to scrofula becomes apparent from the investigations. "It is found to be, upon the whole, more fatal in healthy than in unhealthy districts." . . . "The agricultural counties of Cambridge and Buckingham sustain a higher proportionate mortality from scrofula than the counties of Lancashire, Staffordshire, and the West Riding of Yorkshire." ("Papers," p. 111.) Why this should be, scarcely appears. It may be, as Dr. Greenhow suggests, that deaths from scrofula take place in early life, while children are yet under their parents' roof, and before the age when they depart to the crowded industrial districts; we would, however, suggest two causes which may account still more readily for the apparent anomaly. Some of the diseases of childhood, such as tabes, hydrocephalus, and some forms of convulsion, are of undoubted scrofulous origin, and it is not at all improbable that scrofulous children die, and are registered as dying, of these diseases in crowded situations, who in more healthy districts would have survived till a later period, and succumbed to their scrofulous taint in another form, their deaths being registered under the head of scrofula. A second cause we would suggest, is the intermarriage which frequently takes place in country places, where the same name occurring over and over again among the present inhabitants, can be traced back perhaps for centuries.

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* Fever in Agricultural Districts.

The mortality caused in childhood by disease of the nervous system, forms one of the most important heads of the present inquiry, approaching in this respect pulmonary disease and alvine flux. There may be some doubt, however, whether convulsions, hydrocephalus, and teething, the division of the subject adopted, is likely to lead to perfectly sound conclusions, considering how loosely certificates of death are often filled up. A large proportion of deaths from hydrocephalus and teething are accompanied with convulsions, and often, probably, the death return is made in accordance with the most prominent symptom, thus leaving a large margin for indefinite conclusions.

The anomalies exhibited by the death-rates of the nervous diseases of children are numerous, and Dr. Greenhow tells us that "the mortuary statistics of the infantine population" seem marked by an absence of any uniform relation "between the proportion of deaths from all causes and from particular diseases." (p. 118.)

Males die in a much larger proportion than females from these diseases, but why we cannot tell. The male rate for England being considered as 100, the female is but 75; in some places, such as Nottingham, Leicester, Derby, falling as low as 67 or 68; in others, such as Newcastle-upon-Tyne, approximating the 100 as nearly as 91. Carnarvon, which has almost acquired notoriety from the circumstance, has greatly the largest death-rate from nervous disease in children, and yet it is by no means an unhealthy place, being very far in advance of such a place as Liverpool in general salubrity; as much indeed as in the proportion of two to one in the value of young life generally, and "yet for every hundred of the males of Liverpool that die from these children's diseases, more than one hundred and forty perish in Carnarvon." (p. 117.) This fact, and others of a similar kind, is at present inexplicable. As a general rule, however, "the mortality is lowest among the thinly scattered population of rural districts, highest in the large towns" (p. 116); highest of all where female labour is most in request, and especially, as in factories, away from their own homes. In other words, the young children die off most rapidly where, as we have already seen, the lives of the mothers are shortened by their industrial employments, where the temptation, if not often the necessity, exists for early weaning; where infants are left in the hands of strangers; where irregular and improper feeding causes fretfulness and disquiet; and where last, not least, drugging, chiefly with opiates, given to subdue the effects of the mismanagement, completes the catalogue of evils by which the infant constitution is irretrievably ruined, and those diseases of the nervous system developed which form so large an item in the death-rate of the country.

Taking a retrospective view of our subject, as we find it brought out in the researches and reports we have passed in review, the results seem to arrange themselves under the following heads:—

The confirmation of facts which have either been tolerably well established already, or which, though received, have been so on indefinite grounds, or which, if not entirely new, are nearly so;—

The reversal of facts which have hitherto been accepted as established;—

The eduction of anomalous facts which are perfectly inexplicable in the present state of our sanitary knowledge.

Of the first of these, we would cite the excess of the female death-rate from pulmonary disease where much in-door female industry is required; the greater liability to cholera, diarrhœa, and fever where impure water is drunk, and especially the valuable fact connected with the surface wells of London; the difference in the female pulmonary death-loss traceable apparently to work at home and work in the factory, the probable connexion of pauperism with high death-rates, both pulmonary and zymotic, and the important results of the experiments of Drs. Barker and Letheby.

Under the second of these heads, we would call to mind the Paper of Mr. Neison respecting the causes of pulmonary disease in the army, the disproof of fever being either, in large proportion, a disease of mature life, or of very crowded localities, as compared with others; and the fact that croup is not found to have the highest death-rate in damp and cold situations. Moreover, that consumption is more fatal in the comparatively mild climate of the southern English counties, than in the northern.

Lastly, as regards anomalies, we have them abundantly enough. Cambridgeshire has a high pulmonary death-rate without palpable reason. Stock and Walstanton show no cause for giving different mortalities from lung disease, nor can we tell why the Welsh coal-mining districts should differ in this respect from their English neighbours. Birmingham, Nottingham, and Leicester, if liable to diarrhœal outbreaks, have an exemption from cholera, apparently from no merit of their own ; and when we come to the differences in the male and female death-rate from the nervous diseases of children, as well as the differences in the general death-rate itself, we are completely at fault; neither is our perplexity lightened by finding that scrofula is a more fatal disease in the generally healthy than in the unhealthy districts.

To these anomalies we may add another not yet alluded to, the fact that the "waterguard" and the waterside officers of Her Majesty's Customs, although exposed to much vicissitude of weather, and *to the constant influence of Thames emanations*, do not suffer from a high rate of mortality.*

Perhaps sufficient has been said to show that although many and valuable are the facts which we have acquired, especially were they practically used, we are as yet but on the threshold of the science of hygiène, and that, as Dr. Greenhow remarks, " the entire subject is barely opened by the present inquiry."

In addition to investigations respecting crowding, occupation, drainage, &c., climate, domestic habits, the structure of the houses, the food, the topography and geology of the districts, must be examined, as well as every circumstance which can affect health, before we shall be in a position to ascertain correctly the causes and the practical bearings of the death-rates of England.

Such minute and local investigations are evidently beyond the power of one man, and the necessity is at once apparent that local "medical officers of health" should be appointed throughout the entire kingdom, from whose reports, rendered in a systematic, and above all in *an independent form*, deductions might be drawn, not simply of the broad features of the sanitary question, but of the varied local circumstances which not only occasion preventable mortality, but which tend to impair health and constitution. Till this is done, there will be no real sanitary science. Individual labours may bring out new facts, and confirm old ones, but they will want much of that minuteness which would make them truly valuable in practical application.

REVIEW II.

1. *A Manual of Obstetrics, Theoretical and Practical.* By W. TYLER SMITH, M.D., Member of the College of Physicians, Physician-Accoucheur to, and Lecturer on Midwifery at, St. Mary's Hospital.—*London*, 1858. pp. 628.
2. *Lehrbuch der Geburtshülfe.* Von Dr. OTTO SPIEGELBERG, Privat-docenten an der Universität zu Göttingen.—*Lahr*, 1858. pp. 372.

Manual of Midwifery. By Dr. OTTO SPIEGELBERG.

3. *Illustrations of Difficult Parturition.* By JOHN HALL DAVIS, M.D., &c. &c.—*London*, 1858. pp. 284.

SOME one has called this the "Age of Manuals," and there is so much truth in it, that we have manuals, and good ones too, upon almost every subject. But in the contemptuous sense in which the term was applied we cannot agree, for the manuals of the present day are in fact treatises of considerable length, retaining only the name of the former brief abstracts to which it was applied. Look, for instance, at the series published by Mr. Churchill; they are certainly small in bulk, but that is the result of small, close print, and thin paper: any one of them, if printed in the old manner, would make a respectable octavo volume. But the most marked difference between them and their predecessors of the same name is, that they are the productions of the ablest men in the different departments of the profession. Almost every section has its volume, and their sale is the best proof of their popularity with the profession. Hitherto, midwifery has been the solitary exception, but at length this want is worthily supplied.

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* Second Report on the Customs, by J. O. McWilliams, M.D.

In Germany, more than one series of manuals, we believe, is in the course of publication. Dr. Spiegelberg's volume is one of a series publishing under the editorship of Dr. Schauenburg, and if the other volumes are of as high a character as the present, the students of Germany will have little cause of complaint.

As most of our readers are probably acquainted with Dr. Tyler Smith's former work 'On Parturition,' in which he so successfully applied Dr. Marshall Hall's discoveries to the elucidation of the physiological problem of pregnancy and parturition, they will no doubt be prepared to welcome another work from the same pen. Nor will this feeling be lessened by the practical character of his recent volume 'On Leucorrhœa.' He has shown that he possesses not only reasoning powers of a high order, but acute and accurate observation of facts.

It will be remembered that the course of lectures delivered by Dr. Tyler Smith appeared in print in the 'Lancet' for 1856, and so favourably were they received, that Mr. Churchill applied to the author to recast them for one of his manuals, in which popular form they are now presented to the profession.

We are inclined to begin our notice by finding a little fault, if Dr. Tyler Smith will excuse us. No doubt classification and division may be carried too far, but a certain amount is a great assistance to the student, and we cannot but think that the studied avoidance of such helps on the part of the author is a defect. To those who look closely, there is certainly an undercurrent of connexion, if we may so speak, between the different subjects, but superficially this does not appear. Of course we have the two great divisions of anatomy and physiology, and practical obstetrics. The execution of the first part is very good, perhaps clearer and more correct than in any of the ordinary works on the subject. The second part is good also, but it is by no means superior, perhaps hardly equal, to some of the other manuals. In order to give the reader a bird's-eye view of the ground occupied, we shall quote the titles of the successive chapters. First, we have the subject of generation treated sufficiently fully, followed by a description of the organs of generation, ovulation, menstruation, conception, and the development of the ovum and its appendages, the signs of pregnancy, disorders of pregnancy, the causes and treatment of abortion, molar pregnancy, super-fœtation, extra-uterine gestation, the nervimotor functions of the uterus, the fœtus in utero, and the duration of pregnancy. We next come to the obstetrical portion of the volume, strictly so called, although no classification is made by the author beyond the separation into chapters. Here we find chapters on the pelvis, the anatomy of the fœtal head, the mechanism of labour, the stages of labour, the management of natural labour, and of the puerperal state, face presentations, pelvic presentations, transverse presentations, funis presentations, placenta presentations, deformities of the pelvis, obstructed labour, difficult labour, tardy and precipitate labour, post-partum hæmorrhage, rupture of the uterus, puerperal mania, puerperal convulsions, puerperal fever, phlegmasia dolens, and lastly, the various obstetrical operations, induction of premature labour, version, the forceps, embryotomy, the cæsarian section, and a chapter on chloroform.

This enumeration will sufficiently prove the want of an adequate classification, at the same time that it will enable the reader to estimate the wide range of subjects included in the work.

As the volume is merely an improved reprint of Dr. Tyler Smith's lectures, we do not think it necessary to enter upon an elaborate analysis of the volume, but shall content ourselves with culling an extract here and there which may appear to us interesting, original, or practically important.

On the subject of the "behaviour" of the uterine mucous membrane during menstruation, the author has carried still further the views of those who regard that membrane as excrementitious under certain circumstances. After describing accurately and carefully the phenomena of menstruation, the character and quantity of the discharge, and the condition of the ovaries and uterus, he observes:—

> "It appears to me in accordance with what I have observed on uteri examined during a menstrual period, the facts connected with membraneous dysmenorrhœa, and the detachment of the decidua in abortion and parturition, to suppose that the mucous membrane is in great part or entirely broken up, and its *débris* discharged during each menstruation. The blood is probably exuded during the breaking up of the mucous structure, and the duration of the menstrual period represents the time occupied in the periodical decadence and renewal of the mucous membrane of the body of the uterus. The new membrane becomes converted into decidua in the impregnated female." . . . "According to the view I have stated, a new mucous membrane is formed as a part of the process of preparation for the reception of a fœcundated ovum; not that the aptitude for the reception and implantation of the ovum belongs only to the newly-formed mucous membrane, though it is probably greater at this time than at others. The mucous membrane may become the seat of the change consequent upon impregnation just before a menstrual period, and in cases where menstruation is suspended. According to the view now stated, the mucous membrane of the uterus becomes excrementitious every month, and is discharged from the cavity of the uterus in a state of disintegration. The uterus appears to gain a new mucous membrane by a similar process to the reproduction of lost parts." (p. 62.)

This is ingenious, no doubt, but we think the facts hitherto observed are far from sufficient to establish it. Nor is it at present reconcileable with known facts, for if the exfoliation is an essential part of the regular monthly process, and necessary for the reception of the ovum, what proof have we that it occurs in those cases when conception takes place before a menstrual epoch, or how explain the occurrence of impregnation during the menstrual flow, as in the cases related by Raciborski? In truth, the condition and changes of the interior of the uterus during menstruation, gestation, and after parturition, require more careful and detailed observation.

The chapters on conception, and the development of the ovum and its appendages, are clearly and carefully written, but without any original observations. The different signs of pregnancy are detailed in a condensed and able manner. The description of the sounds of the fœtal heart is somewhat imperfect: nothing is said of the peculiar rhythm which differs so much from the healthy adult heart, but which is occasionally simulated in typhus fever.

The variations in the actions of the kidneys are very well told, and the treatment of albuminuria very sound. A variety of the renal secretion is noticed by Dr. Tyler Smith, as he believes, for the first time, and we shall quote his own words:—

> "I am not aware that the matter has been observed by obstetrical authors, but in some pregnant women, the urine, without being albuminous, contains habitually a large quantity of triple phosphates, is of a high specific gravity, and has an alkaline reaction during the greater part of pregnancy. The nervous and vascular erethism attendant upon, or produced by, the state of pregnancy, is followed by the same results as other and more marked causes of exhaustion. I have known this phosphatic diathesis to exist in cases in which fatty degeneration of the placenta has occurred in successive pregnancies." . . . . "The treatment in such cases should be that employed in the phosphatic diathesis occurring under other circumstances than pregnancy, namely, the mineral acids, opiates, rest, and a nutritious regimen. Such patients also require either during or after the completion of pregnancy, preparations of steel, as a marked degree of anemia is produced by the persistence of the disorder." (p. 121.)

When treating of retroversion of the impregnated uterus, under the head of disorders of pregnancy, Dr. Tyler Smith mentions M. Gariel's proposal to introduce his india rubber pessary, and then inflate it as a means of raising the fundus uteri, but he adds, that he is not aware of its having been tried. We had an opportunity of testing its value recently, and we are happy to say that it succeeded perfectly, and in a few minutes, without the least pain; but then it is only fair to state that .the patient was only about three months pregnant, the uterus by no means tightly filling the pelvis. The idea of thus replacing the uterus, however, is due to Dr. Halpin, of Cavan, who many years ago reported a case thus treated successfully to the Dublin Obstetrical Society.

The chapter on abortion and its treatment is very good as far as it goes, but scanty information is given upon one or two points which are most puzzling to beginners, and even sometimes to those of riper age. Suppose an abortion of two or three months, the fœtus being expelled, but the shell of the ovum retained. We are to restrain hæmorrhage of course, and to procure the expulsion of the retained portion by ergot, &c., if we can. But suppose we fail, and that we can neither expel nor extract it, what is to be done? Dr. Tyler Smith has not provided for such cases, and yet they are very common.

The cause of the ordinary position of the fœtus in uterus, *i. e.*, with its head downwards, whether it be the result of gravitation or instinctive and voluntary motions or reflex movements, is yet a subject of controversy. Probably no one of these theories affords an adequate explanation, and this seems to be Dr. Tyler Smith's opinion. His own views are thus given :—

> "In the early development of the embryo the limbs are deficient in muscular power, and do not assume any definite form. The nervous system has hardly commenced its control over the, as yet, feeble muscles. The quantity of liquor amnii is very large in proportion to the size of the uterus, and the uterus is circular rather than ovoid in shape. We have to consider these elements as slowly altering from day to day in an almost inappreciable manner, during the middle and later months of pregnancy, and while the fœtus is gradually taking up its ultimate position. The limbs of the fœtus enlarge, becoming subject to the vis nervosa, under the influence of tone; the arms and legs, particularly the latter, become contracted so as to form the fœtal ovoid. During this time, the relative quantity of the liquor amnii diminishes, so that at the full time the liquor amnii scarcely does more than fill up the interstices left between the fœtus and the uterus. Synchronously with these events, the uterus itself, by the development of the cervix, changes from the circular to the pyriform or ovoid shape. With this change of shape the uterus acquires more power of muscular contraction, and becomes the subject of reflex and peristaltic actions. The contractions of the uterus necessarily exert a moulding or adaptive influence upon the fœtus, poised lightly as it is in the liquor amnii, and moved within the limits of its prison by the slightest impetus. These causes are aided by the reflex movements of the fœtus itself. Under irritation, the limbs of the fœtus strike out, but only to return it more closely to the ovoid shape, and to accommodate it as accurately and easily as possible to the uterine cavity. All these influences, combined with the effects of gravitation and of the inclined planes upon which the fœtus rests in the upright and recumbent positions of the mother, arrange and preserve the fœtus in the normal position with the head at the os uteri. No single power, however, gives its attitude or position to the fœtus, and it is difficult, amidst such a number of adaptations, all contributing to the same end, to single out the most important. If we give the predominance to any single one of them, I think the spinal principle of Tone must be considered as the most influential, and it is to the absence of this, more than of any quality, that we must attribute the irregular presentations of dead children." (p. 217.)

Passing on to the consideration of labour, we find that Dr. Tyler Smith divides it practically into a preliminary and supplemental stage, and three principal ones—dilatation, propulsion, and expulsion. There is no great harm in making more stages than usual, provided the limits are steadily borne in mind; at the same time, we contend that there is a practical advantage in marking the distinction between what has been generally called the first and second stage, or the interval before and after the passage of the head through the os uteri. The entire history of prolonged labour turns upon the question of the stage in which the delay occurs, and if we add the question of relative proportion between the pelvis and that which has to be transmitted through it, we shall include all the problems of uncomplicated midwifery. A prolonged first stage involves no danger to the child, and none to the mother, except so far as loss of sleep may produce exhaustion, and is a bad preparation for a possibly long second stage. Whereas the second stage very slightly prolonged beyond its normal duration will give rise to constitutional symptoms, and if much prolonged, will involve both mother and child in imminent peril. We strongly recommend the study of the chapters on the management of natural labour and the puerperal state to the junior members of the profession.

When describing the mechanism of face presentations, Dr. Tyler Smith states that the emergence of the head from the pelvis with the chin on the perineum, as described by Smellie and Hamilton, is impossible without assistance; and in this we are satisfied that he is right. He also very properly objects to the different modes of management which have been laid down, and truly states that the treatment is very simple:

> "We may assist the chin in making its rotation forwards and downwards by introducing the finger into the child's mouth, making traction upon the lower jaw, and bringing it under the arch of the pubis. Meigs lays it down as a great rule of practice in face cases, that the chin should be brought towards the pubis as the face emerges from the pelvis. This is an analogous procedure to that of bringing the occiput down in vertex cases, either directly, by the fingers applied to the back of the head, or indirectly, by pressure exerted upon the forehead. The principle is the same in both cases—viz., to favour the birth of that part which tends to be born first. Should the head fail to rotate in the pelvis, the forceps will generally be necessary or the head must be dislodged, and the child delivered by turning." (p. 335.)

The directions for delivery in breech or footling cases are clear and concise, based upon a due appreciation of the source of danger to the child. Pressure upon the funis is to be watched and guarded against if possible, but as it is important to allow the body of the child to be expelled slowly for the more perfect dilatation of the passages, we are unwilling to afford assistance if it can be avoided; but after the arms are expelled and the head is in the cavity of the pelvis, we must interfere, or the child will be lost.

> "The necessity of interference being clear, there is no great difficulty in rendering it. A finger or two of the left hand should be introduced into the child's mouth, or laid one on each superior maxilla, and the face drawn steadily down towards the fourchette of the perineum: at the same time, the occiput should be pushed up by a finger or two of the right hand, introduced behind the pubis. This manœuvre will bring the shortest diameter of the fœtal head into relation with the antero-posterior diameter of the outlet of the pelvis. The head will emerge now with the aid of gentle traction towards the knees of the mother, and the birth is complete. Whether the position of the fœtus is a dorso-anterior one or an abdomino-anterior, the management varies hardly at all. The occiput rotates forwards from its posterior position just as in third and fourth vertex cases; and nature will generally adapt the head to the pelvis much better than the accoucheur. No force should be used in the extraction of the head. Above all, traction should never be exerted upon the shoulders, as very slight extension of the neck is sufficient to destroy the child." (p. 350.)

With multiparous women, the timely extraction of the head is easy enough, and Dr. Tyler Smith's rules may be safely followed, but with a first case we are pretty sure that a rigid observance of them would entail the loss of the child. The truth is, that in such cases a certain amount of traction force must be used, but it is our duty to take care not to exceed what the child can safely bear; and that force must be exerted upon the shoulders, simply because there is no other part on which it can be effectively exerted. If the body of the child be carried very much forward, not towards the knees, but between the thighs, the face will generally sweep over the perineum in time, and without requiring an injurious amount of traction.

Dr. Tyler Smith differs from Dr. Barnes as to the cause of placenta prævia. His opinion is that it is caused by the impregnation of the ovule, or attends the

> "Arrest of the impregnated ovule after it has descended through the Fallopian tube and

uterus, so as to reach the upper part of the cervix uteri, this being the last point at which the ovule retains its capability of impregnation and attachment to the uterine surface . . . There is no more difficulty in supposing that, instead of being arrested or impregnated in the tube or in the fundus, it may in some cases pass on to the lower part of the cavity of the uterus, the point at which it joins the cervical cavity, in which case we have placenta prævia." (p. 376.)

This does not appear to us very explanatory; it is at most another way of expressing the same fact.

We are rather surprised at the omission of the stethoscope as a means of diagnosis in these cases. In several cases we have been able to decide between accidental and unavoidable hæmorrhage by its aid alone.

We quite agree with the author in the use and value of the plug, and in the opinion he expresses of the usefulness of astringents. Puncturing the membranes we think objectionable unless good pains be present, or the hæmorrhage have ceased, as emptying the uterus would not control the flooding, but might divert it, and render the case one of internal hæmorrhage. Dr. Tyler Smith observes—

"In my opinion, turning is the great operation in placenta prævia, when the child is living and viable—that which, if performed at the proper time, affords the greatest chances of safety both to the mother and child. But there are circumstances in which turning is the best practice, when the safety of the mother alone is concerned, the child being already dead. The conditions favourable to turning are, a dilated or dilatable condition of the os uteri, the retention of the liquor amnii, or a moderately relaxed uterus, a pelvis of average capacity, the absence of dangerous exhaustion, or a temporary cessation of the hæmorrhage. If the placenta be attached to one side of the uterus the hand should be introduced on the side opposite to the placental site; or if it extends over the whole os, the hand should be passed in the direction in which the attachment is least considerable, or where the separation has already taken place. The advantages of turning are, that without materially increasing the danger of the patient, and in a very short space of time, the feet and body of the child may be brought down so as to act as a tolerably efficient plug to the os and cervix uteri. During the early part of the operation the hand and arm of the accoucheur form a tampon. Turning is generally easy in placenta prævia at the full term, as compared with other cases in which it is required, because the contractions of the uterus are commonly less powerful than usual. The flooding itself tends to produce dilatation (qu. relaxation) of the os uteri, and to weaken uterine action. It is, therefore, a less severe operation to the mother than in many other cases in which it is called for. This is particularly the case in multiparous women. As regards the state of the uterus, primiparous women, as in other cases requiring turning, offer greater difficulties than women who have borne children. When the operation of turning is performed early in placenta prævia, the proportion of mothers saved is large, and a considerable number of children are born alive. Turning should always be performed in placenta prævia, when it is considered advisable, the instant the operation is rendered practicable by the condition of the os and cervix uteri. In cases where the os uteri has been dilated for many hours sufficiently to admit of turning, and blood or strength has been lost in the interim, we should blame not the operation, but the delay, for a great proportion of the fatality to the mother and child." (p. 382.)

It does not appear that Dr. Tyler Smith is convinced of the advantages of completely detaching and extracting the placenta according to Dr. Simpson's plan, nor of detaching the lower portion, as proposed by Dr. Barnes.

The chapter on convulsions is very complete. Under the heads of centric and excentric causes the author enumerates all the physical excitants of the disease; and he adds, under the former head, a valuable paragraph upon the psychical causes. Moreover, the subject of albuminuria, and its connexion with convulsions, receives special and careful consideration, the author availing himself of Dr. Braun's valuable treatise.

As regards bloodletting, we quite agree with the following remarks of the author:—

"The action of bloodletting on the spinal marrow is greatly modified by the condition of the circulation. In fulness of the vascular system, it is a powerful sedative of spinal action. Hence venesection is a great remedy in the simpler form of puerperal convulsion, where the disease chiefly depends on stimulation of the spinal marrow by excess of blood, or the mechanical pressure exerted by the blood on that organ, together with the counter-pressure of the distended brain on the medulla oblongata. In such cases bleeding should be performed with a view to its sedative action on the spinal marrow, and to avert the mechanical effects of vascular pressure from this organ. Alone, it will frequently be sufficient to subdue the disease particularly when the fits come on before the beginning of labour or after delivery. But the second important intention of bloodletting should never be lost sight of—namely, that of preserving the brain from injury during the convulsion. Besides the primary congestion, which may have been the cause of the attack by its counter-pressure on the medulla, the convulsive actions themselves, exerting great muscular pressure on the whole vascular system, and causing, as they do, great turgidity of the vessels of the head, are frequently dangerous sources of fatal cerebral congestion, or of serous or sanguineous effusion. As in the case of epileptics, women in puerperal convulsion frequently die of apoplexy, produced by the immense pressure exerted on the cerebral column of blood during the fits. It is, I believe, in great measure from the effects of bloodletting in warding off accident from the brain that bleeding is so general in this disease. The due recognition of the distinct operation of bloodletting on the cerebral and spinal systems is of the utmost consequence. In plethoric states of the circulation it is in this disease *curative* in its action on the spinal marrow, *preventive* in its action on the brain." (p. 505.)

One of the most important points in connexion with the management of convulsions is, whether the labour is to be left to nature, or whether we are to interfere, and in what way. There is good sense in the following observations:—

> "The general principle we may deduce is, that wherever artificial delivery can be effected with less irritation than would be produced by the continuance of the child in the parturient canal, and its expulsion by the natural process, it is advisable that it should be performed if the situation of the mother be perilous. It must be with reference to this principle—namely, to the irritation of any particular operation, and the irritation of labour itself—that turning, craniotomy, or the forceps must be decided upon." (p. 511.)

We confess to having a great objection to turn in such cases, and rarely will craniotomy be justifiable unless the child be dead. But here chloroform is invaluable, and deserving, we think, of a fuller notice than Dr. Tyler Smith has given it. It not only arrests the convulsion, but renders the application of the forceps safe and easy; and the delivery without pain, or a recurrence of the paroxysm.

In conclusion, we may congratulate the profession upon having the matured opinions of so able an accoucheur as Dr. Tyler Smith within reach, and the publisher upon having secured a valuable addition to his manuals. Of the style of the author we have enabled our readers to judge for themselves, and we do not think that their judgment will be adverse. The book is not perfect, of course, and we have taken the liberty of pointing out what we consider its chief defect—viz., that the practical part is rather a brief summary of the best current opinions, than a series of detailed instructions for the guidance of practitioners.

The illustrations, which are very numerous, are beautifully executed, and add much to the value of the book.

We are happy in being able to speak very highly of the second book at the head of the article. Dr. Spiegelberg deserves great credit for the care and pains he has taken. The volume has no pretence to originality; it is even more of a manual perhaps than Dr. Tyler Smith's, and its range is more limited, but so far as it goes, the execution is very respectable.

The first part consists of the anatomy of the pelvis and the parts of generation, with ample instructions for both external and internal investigation. Then follows the physiology and dietetics of pregnancy, labour, and childbed, with very minute details as to the changes effected in the organs involved; the development of the ovum, the diagnosis of pregnancy, the duration of gestation, and the diagnosis of the life and death of the child. The sections are carefully written, and contain a large amount of information.

The author next speaks of labour, of the pains, including not merely the uterine contractions, but those of the vagina, as well as the action of the abdominal muscles; after which he describes the ordinary course of labour, the mechanism of parturition, and the management of labour, with a section on the use of chloroform. Then we have a carefully written section on the physiology and dietetics of childbed, both as regards mother and child.

The third division of the work concerns the pathology and treatment, first of pregnancy and then of delivery. Under the latter head we have sub-sections, where the pathological condition is referable, 1 to the pains, 2 to the pelvis, 3 to the soft parts, and 4 to the child, with the subsequent addition of complicated labour. This arrangement is excellent, and we very much prefer it to the absence of all such classification in Dr. Smith's manual.

The fourth division embraces all the obstetrical operations, and is both full and precise.

Our limits will not permit us to enter upon a detailed analysis of this work, but we shall notice one or two points.

Dr. Spiegelberg is an advocate for the use of chloroform, and we think that he has met the usual objections in a fair and satisfactory manner. He lays down the following conditions for its administration: 1, that an inhaler is not necessary, provided the handkerchief or towel be held at a short distance from the face, so as to admit the free mixture of atmospheric air; 2, the chloroform must be pure; 3, the inhalation should commence at the beginning of the second stage; 4, and should be continued only during a pain; 5, in normal labour, an extreme degree of anæsthesia need not be produced; 6, we should commence with a moderate dose; 7, the patient need not be confined to one position, and special attention should be paid to the evacuation of the bladder; 8, chloroform should neither be given after a full meal, nor after long fasting; 9, the greatest quiet is to be observed, especially at the commencement of inhalation; 10, the patient is not to be awoke.

As our object is not so much to exhibit the style of the author, as to show the manner in which he treats his subject, we will turn to the section on narrow pelvis, and to that on the operation by the forceps, for this purpose.

The former commences by the distinctions between general narrowing, or that arising from special deformity of bones or joints. Thus we have the equally undersized pelvis, which may take the female, the juvenile or the male form, the funnel shape, the diminished conjugate diameter of the brim, synostosis of the sacrum and innominata, Naegele's oblique distortion, and synostosis of both sides, as described by Robert. Next, we have carefully described the varieties of deformity caused by rickets, osteomalacia, exostosis, and fractures, with admirable illustrations; then those resulting from hip disease, acute and chronic, and from changes in the vertebral column. After a full account of the distortions, the author enters upon the consideration of their diagnosis, and their influence upon pregnancy and child-birth; and lastly, the treatment, according to the amount of narrowing, divided into four degrees.

Thus it is evident that in brief space we have a complete review of the whole subject, and we do not see a single point of importance omitted. The descriptions are short and to the point, and the practice recommended is in accordance with the best authorities.

Now let us take the forceps operation. The author begins with a description of the long and short forceps, with plates of Levret's, Smellie's, and Naegele's instruments; and proceeds to consider their action, whether mechanical or dynamical. Then follow the conditions requisite for the operation, and the circumstances which indicate its necessity, and the prognosis.

There is sound sense in the rules laid down as to the time of the operation, so as to enable us to save both mother and child if possible; at all events, to injure neither.

After enumerating the preparations necessary, such as emptying the rectum and bladder, placing the patient in the proper position, &c., &c., and recommending the use of chloroform, Dr. Spiegelberg lays down twelve general rules for the operation, as to the position of the forceps, the mode of introduction, the mode of closing and holding the instrument, and the line of extraction and amount of duration of the force employed.

From the way these two subjects are handled, the reader may form an idea of the book itself. They were not selected as being better than the rest, but were taken haphazard. Each subject has been thoroughly investigated, and though we may not agree with all the author's conclusions, we have always been gratified by the care he has bestowed and the amount of information he has brought forward.

The third volume prefixed to this article, is a record of cases of difficult labour, preceded by some general remarks on the subject and followed by a statistical report of 7302 cases delivered under Dr. Hall Davis's superintendence. Details of 144 cases, presenting some difficulty or peculiarity worthy of notice, are given with great care, and will form a valuable body of reference to the practitioner. The preliminary observations are sound as far as they go, but in one respect they too much resemble the operative part of Dr. Tyler Smith's book—they are too general, too short and cursory, so that they do not meet the practical difficulties which beset the accoucheur. A chapter might be profitably written on difficult parturition as *affecting the accoucheur.* The most prominent subjects of the book are the cases requiring the forceps or craniotomy, and to some remarks upon them we shall confine ourselves. We could have wished that Dr. Davis had marked out more distinctly the limits of the two operations, especially as we see from the journals that a controversy on the subject of craniotomy has taken place between an anonymous writer in the Dublin (Literary) Review, and Dr. Churchill in the Dublin Quarterly Journal. When we find craniotomy absolutely proscribed, except in the case of dead children, and the cæsarian section proposed as its ordinary substitute, it becomes quite necessary to define, with as much accuracy as possible, the limits of the former operation, not with reference so much to hysterotomy as the alternative, as to the employment of the forceps.

The practical question which presses upon every one extensively engaged in midwifery is, "What are the limits, on either side, of the operation with the forceps and of craniotomy?" In other words, on what grounds are we to determine that either operation is unnecessary on the one hand and useless on the other? Between these two extremes lies the ground which such operation will occupy, and to which each must be limited.

Let us consider the forceps first. The delay may be the result of dynamical or mechanical causes, or a combination of both. There may be ample space in the pelvis, and no ascertainable physical impediment, and yet the pains, strong at first, become gradually less effective, though to the full as painful. Or the mechanical impediment may be extremely slight, so slight, indeed, that nothing but experience could satisfy us that assistance could be necessary. We have more than once seen labour rendered powerless by the child's nose pressing against the symphysis pubis, the forehead being anterior, and where there was otherwise ample space and good pains; and yet this very slight obstacle protracted the labour until assistance with forceps was necessary. Again, there may be good forcible pains, but the pelvic space may be diminished by some unusual condition of the soft parts, or by the sutures of the child's head being ossified, or by an actual disproportion between the head and the pelvis. Now in each of these cases the important question is, how far we are to trust to the natural powers, and by what are we to be decided in having recourse to art? Is it to be a question of time, symptoms, or measurement? Let us hear what Dr. Davis says: before operating—

> "We should feel satisfied that there is risk to either the mother or child from a longer protracted pressure. But on no account must we withhold our assistance till that extremity of prostration of the vital powers has supervened in which little or no hope remains that art can avail us in rescuing either life. The danger of injury lies in the pressure exerted in labour

being violent and long continued on the same tracts of tissue, the head immovably fixed in one position, so as to interrupt the free circulation in the parts pinched, so to speak, between it and the pelvic walls. The head may sometimes remain stationary in the pelvis after full dilatation of the uterine orifice for a longer period than even twelve hours, and no evil result follow. But for this to occur there must be ample space, with the soft parts moist and relaxed, and little or no labour action present. In due time, often after a refreshing sleep, which it may be judicious to promote by artificial means, nature resumes and completes her work with safety. It is usually admitted that so long as the head advances *pari passu* with the pains of parturition, so long there can be no necessity for interference, even though the above stated limit of time may have been much exceeded. As a general principle this is undoubtedly a fact, but fatal exceptions to its universal truth have occurred." (p. 24.)

Again,

"There must be room in the pelvis for the safe introduction of the forceps at the two opposite points of the circumference of the head. If the child is known to be at or near the full term, and appears to be of the average size, there should, in my opinion, be at the brim of the pelvis in the conjugate diameter, and at the outlet in its transverse diameter, a clear space of three inches and a quarter, to afford us any hope of success in a forceps operation. A labour in which the head is so completely locked and wedged in the pelvis that an ear cannot be reached, and when the examining finger cannot be passed between the head and the pelvic wall at any point, is obviously one in which we could not have recourse to the forceps." (p. 23.)

Lastly, Dr. Davis observes,

"As an aid to our decision in favour of interference, I may refer to a sign which I have for several years past found useful as an intimation of approaching danger from protracted pressure in obstructed labour, namely, an olive-coloured or brownish slimy discharge, a depraved secretion from the mucous membrane, the result of long-continued irritation. The child may often be saved after the occurrence of this discharge, which differs in character from that of meconium."

Thus we find that Dr. Davis takes into consideration time, space, and certain symptoms or results of delay. We cannot agree with him that the whole danger consists in the long-continued pressure. Even when this is in no degree excessive, the febrile symptoms of powerless labour may arise, and the patient die, if not delivered in time. Neither is the olive-coloured discharge a trustworthy guide; when it *does* arise from irritation of the mucous membrane, subjected to long pressure, it is very significant; but how often does a similar discharge occur at all periods of labour where there has been no undue pressure? We saw a case lately in which the liquor amnii resembled green peasoup, and the entire labour was completed in two hours.

In making up our minds, then, we must take into consideration *first*, the character of the pains. If they have been strong and propelling, and are now getting less forcible, producing less impression on the head, and if by degrees instead of forcing the patient to bear down, they occasion an outcry like first-stage pains, we may fear that, irrespective of any obstacle, they alone may not be sufficient to complete the labour.

2. The Symptoms.—If at any period of the second stage we find the pulse higher than usual, and remaining so between the pains, the skin becoming hot, the mouth and tongue dry, with other symptoms of powerless labour, we may rely upon it that we run great risk by delay, as we generally find also, when these symptoms arise, that the pains diminish, in power at least.

3. Time.—We quite agree with Dr. Davis, that it is impossible to fix a precise time for interference, inasmuch as unfavourable symptoms will arise in one patient in five or six hours, and not for twelve or eighteen in another. Of course, in speaking of time, we refer to the second stage only, or after the os uteri is fully dilated, or the head has passed through it. To us the question of time seems rather subordinate to the other conditions generally; but it is very important in calculating on the results of continued pressure, and also upon the life of the child.

It is hard to say exactly after what period the child is in peril; but we know that it becomes so, and this will be an element for our calculation, especially in forceps cases.

4. Space.—We must not forget that, under certain dynamical conditions, a very little obstacle will, so to speak, paralyse uterine action, and ultimately render the labour a "powerless" one; so that we must estimate its importance not *per se*, but by its effect upon the progress of the child's head in its descent, allowing sufficient time. If strong pains, continued for a sufficient time, do not overcome the obstruction, and force through the child's head, unfavourable symptoms will result, and it is our duty to anticipate these. On the other hand, though we cannot easily measure the number of inches in the conjugate or transverse diameters, we can arrive practically at sufficiently correct conclusions by comparing the child's head with the pelvis, and observing the 'dip' or protrusion during each pain. If the finger cannot pass between the head and the pelvis at any part, we quite agree with Dr. Davis that the case is not one for the forceps apparently. But it may pass to a certain extent without reaching the ear; and we confess we should be very unwilling to destroy a child in such a case without trying to introduce the forceps. The kind of instrument preferred by Dr. Davis is not easy to introduce, except laterally; whereas one blade of the single-curved forceps, when properly made, may be passed under the arch of the pubis, where there is generally the most room, and afterwards changed into the oblique position without difficulty or danger, if the operator be only gentle and dexterous; the other blade will pass easily posteriorly. We have ourselves repeatedly succeeded in introducing the blades, and extracting a living child, in cases where at first it seemed impossible. Of course, great care and judgment will be necessary both in the introduction and in the amount and continuance of the force employed in extracting; still, when the alternative is the destruction of life, no means should be left untried to guard against so fearful an operation.

These, then, are the points for our consideration in every difficult or protracted case, and upon them our decision must be founded as to the necessity of the operation, and the time best suited for our interference. There is a singular amount of tact acquired by practice, which is beyond price to the accoucheur and the patient. Those who have seen many cases of this kind will very often be able to predicate that interference will ultimately be necessary long before any urgent symptoms seem, to an ordinary mind, likely to require it. Now if by any accurate means we can arrive at this conclusion, it is evident that many hours of suffering may be spared, and the safety of both mother and child made more secure.

We have said that we do not agree with Dr. Davis in his preference of the forceps with the double curve, but we think the rules he has laid down for their application intelligible and correct. He has hardly dwelt enough upon the *local* care and attention which the nurse should be directed to observe during convalescence.

Now let us say a few words upon craniotomy. No doubt it is a most painful operation, when the child is alive, to any man of feeling, and certainly if it be wantonly and unnecessarily employed, it is nothing short of murder; however, Dr. Davis is quite correct in observing that, "in performing this destructive operation on the child, we are not necessarily sacrificing its life, for it is consolatory to know that in most cases, ere we are compelled to resort to it, the child has already ceased to live." It is generally admitted that when assistance is necessary to deliver, if the child be dead, craniotomy is preferable to the forceps.

But, though few, there are still some cases in which the child is alive, and in which we are called upon to act. Are we to act as soon as the necessity is established, or wait until the child die, notwithstanding the increased risk to the mother? Unhesitatingly, we should choose the former, if the case be one where craniotomy affords the only chance. What, then, will be sufficient ground for this conclusion? Suppose we detect a positive disproportion between the head and the pelvis, and that after so many hours of labour little or no progress has been made beyond a certain point; that the pains produce no depression of the child's head, and that febrile symptoms are setting in: this is a combination which affords but little prospect of relief by any other means. If, in addition, we have made a cautious use of the forceps, and have failed either in introducing them or in extracting the child, it would involve the mother's life if we were to hesitate. "When the impediment to delivery," says Dr. Davis, "is greater than can be overcome by the means already considered, it will be impossible to save both mother and her offspring: both lives would inevitably be lost in the attempt. Under these circumstances it becomes our imperative duty to rescue the more valuable life, which, for obvious reasons, is that of the mother." The latter sentence, we think, involves a most mischievous fallacy. We do not "rescue" the mother's life *because* it is most valuable; there is no choice in the matter: we *may* save the mother, but we *cannot* save the child, and as it is necessarily and inevitably doomed: we merely destroy it a few hours earlier than it would die if left alone.

But whilst unequivocal bony distortion may render the birth of a living child impossible, a minor degree, or disproportion from other causes, may necessitate craniotomy in one labour and not in another. As Dr. Davis remarks:—

> "Let us not, on the other hand, take for granted, that because craniotomy has been the only alternative in one labour, it must of necessity be resorted to on a subsequent occasion. On account of the swelling of the maternal tissues and fœtal scalp, of rigidities by inordinate ossification of the head, or by reason of its being above the average size, perforation may have been perfectly proper in the first confinement; and in a succeeding delivery, a child of equal or of not much smaller size, may pass living without obstruction. Again, though much more rarely, a patient's first or second labour may be easy, and a following one difficult, requiring the forceps, or even craniotomy, on account of the like swelling in the soft tissues, undue hardness or thickness of the fœtal cranium, or from its head being longer than on previous occasions." (p. 62.)

This is quite true, no doubt, but such cases are rare, and would be still less frequent if we had the care of the patient from the beginning. By proper treatment several of these causes might be prevented or removed, so as to bring the case within range of the forceps.

In conclusion, we find that one of the limits of the forceps operation is marked by certain symptoms, the force or feebleness of uterine action, and time; the other limit by mechanical disproportion. Craniotomy is almost always (except in the case of dead children) a question of mechanical disproportion, comprised between the measurements which will not permit the passage of a living child, and those which prohibit the passage of a mutilated one.

Having said so much, we must refer our readers to Dr. Davis's book for his opinions on the induction of premature labour; on face, breech, footling, and transverse presentations. Although we may not always agree with him, yet we feel high respect for his judgment and intelligence.

REVIEW III.

1. *Anatomie Comparée du Système Nerveux, considéré dans ses Rapports avec l'Intelligence.* Par Pr. LEURET et P. GRATIOLET. Accompagnée d'un Atlas de 32 Planches dessinées d'après nature. Tome Second, comprenant *l'Anatomie du Cerveau de l'Homme et des Singes, des recherches nouvelles sur le développement du crâne et du cerveau, et une analyse comparée des fonctions de l'intelligence humaine.*

Par M. Pierre Gratiolet, Aide-Naturaliste-chef des Travaux anatomiques au Museum d'Histoire Naturelle, Membre de la Société Philomathique.—*Paris*, 1857. 8vo, pp. 692.

Comparative Anatomy of the Nervous System, considered in its Relations with Intelligence. By MM. Leuret and Gratiolet. Second Volume, comprising *The Anatomy of the Brain of Man and the Quadrumana, new Researches on the Development of the Cranium and of the Brain, and a Comparative Analysis of the Functions of the Human Intelligence.* By M. Pierre Gratiolet.

2. *An Essay on Physiological Psychology.* By Robert Dunn, F.R.C.S., Fellow of the Royal Medical and Chirurgical Society, &c.—*London*, 1858. 8vo, pp. 94.
3. *The Human Mind in its Relations to the Brain and Nervous System.* By Daniel Noble, M.D., Visiting Physician to the Clifton Hall Retreat, &c.—*London*, 1858. Post 8vo, pp. 157.

After an interval of nearly twenty years, M. Leuret's valuable treatise on the Comparative Anatomy of the Nervous System, the continuation of which by his own hand was prevented by his early and lamented death, has been completed by M. Gratiolet, an anatomist whose position gives him the greatest facility for the prosecution of such researches, and whose ability we believe to be fully adequate to the first portion of his task. The power of observing and analysing psychological phenomena, however, is a very different gift from that which makes a good dissector; and here, as it appears to us, M. Gratiolet by no means equals the *savant* whose work he has undertaken to carry on. We shall not attempt a systematic analysis of his work; but shall extract from it such materials as suit our present purpose, which is that of inquiring into the progress of Cerebral Physiology since the date of an essay which appeared in the pages of one of our predecessors just twelve years ago.* This essay, we believe we may say without vanity, has had an important influence in the subsequent direction of the thoughts and inquiries of our own countrymen in relation to its subject; and it is explicitly referred to as the foundation of the two smaller treatises before us, which proceed from two gentlemen, who, whilst themselves putting forth very different views as to points of detail, closely accord as to all general considerations both with each other and with ourselves. Mr. Dunn's Essay consists of a series of papers which have appeared in the 'Journal of Psychological Medicine,' and which are now issued in a collected form, having for their object to expound the general bearing of Physiology upon Psychology, and at the same time to advance certain new propositions which appear to their author to be deducible from the results of inquiries carried further in the same direction. "Written," as he says of them, "at varying intervals amid the distraction of medical practice," they bear some "marks of needless repetition and of a want of unity in the treatment of the subject;" but of such imperfections it would be hypercritical to take account in a series of papers containing so much that is interesting and valuable. Dr. Noble's little book, which is essentially an expansion of the earlier chapters of his treatise on Psychological Medicine, is more compact and less elaborate; but its object is less to develope any new views, than to present, in a form adapted to the general reader, a summary of what is known, or may be reasonably surmised, as to the dependence of different forms of Psychological activity upon the instrumentality of different parts of the Encephalon. The notice which we took of it in our last number supersedes the necessity of any further allusion to it on the present occasion.

Taking our stand upon the Article just referred to, the great purpose of which was to show in general terms what the Cerebrum *does* and what it does *not do*, we shall first direct the attention of our readers to the most important facts which have been substantiated or rendered probable, by subsequent researches into the comparative anatomy of the cerebrum. In this survey we shall limit ourselves to the Mammalian series; for it is here that the Cerebrum first asserts its predominance over the sensorial centres; and it is not a little remarkable that in this single class we should accomplish the entire ascent from a grade of cerebral development that seems but little elevated above that of the Bird, to one in which the proportions are so completely reversed, that the Cerebrum, both in size and complexity, throws every other part except the Cerebellum literally as well as metaphorically into the shade. The difference in this respect between the Fish and the Marsupial, is not nearly as great as that which exists between the Marsupial and Man. For even the external augmentation of the Cerebrum in the lower of these two series, is not by any means to be taken as an indication of its proportional development; since a very slight examination suffices to show that the hemispheric ganglia are still, in the lowest Mammalia, nothing else than incrustations (so to speak) wrapping round the comparatively large sensorial centres; whilst a more careful examination of their internal structure brings to light a corresponding simplicity in the arrangement of its fibres. But as we ascend through the higher Mammals, we find the size of the Cerebrum to increase beyond all proportion to that of the sensory ganglia; its superficial layer of vesicular matter is vastly augmented not merely in thickness, but in extent of surface; and its fibrous structure progressively increases in complexity, so as almost to defy the efforts of the most skilful anatomist to unravel its multiple interlacements. The Cerebrum is, in our apprehension, the most distinctive portion of the Mammalian organization. The arrangement of the genital apparatus (in which we include the mammary glands) peculiar to this class, is subservient to that prolongation of the period of dependence of the offspring upon the parent, which we find throughout animated nature to stand in relation to a higher grade of ultimate development. And as the various modifications of general structure which its different types present, can be easily shown to be far more intimately related to distinctive peculiarities in their respective habits of life than they are elsewhere, we should anticipate finding in the Mammalian series a diversity of conditions of cerebral organization, corresponding to that diversity in the construction of the sensory and motor apparatus which so remarkably distinguishes its several orders, and yet more closely related to that progressive advance in *intelligence* which is the essential character of elevation in this class. The researches of comparative anatomists, though far from informing us completely as to the Cerebral organization of the several types of Mammalian structure, have brought together a mass of material which admits (to say the least) of provisional arrangement; and we shall now inquire what are the general conclusions to which the facts thus classified appear to point.

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* British and Foreign Medical Review, vol. xxii. p. 488.

Four leading modifications of cerebral structure have been recently shown by Prof. Owen* to be distinguishable in the class Mammalia; and these are concurrent with such modifications in other important systems of organs, as, in the opinion of that distinguished naturalist, to afford the most satisfactory basis for the primary division of the group:—

I. The first and lowest sub-class may be termed from its cerebral character *Lyencephala*, or loose-brained, the cerebral hemispheres being comparatively loose or disconnected with each other, owing to the want of the great transverse commissure or corpus callosum. The size of these hemispheres is such as to leave exposed the olfactory ganglia, the cerebellum, and more or less of the optic lobes; their surface is generally smooth; and the anfractuosities, when any present themselves, are few and simple. This division includes the two orders (*Monotremata* and *Marsupialia*) of "implacental" Mammals, which have been considered by many Zoologists to rank as a distinct sub-class, on account of their very marked points of affinity to the oviparous Vertebrata, but which Prof. Owen now regards as constituting a group equivalent in value to each of the other three, and as having special relations of analogy with the Lissencephala .Although our acquaintance with these animals is comparatively imperfect, yet it can scarcely be questioned that as regards intelligence they hold the lowest rank in the entire Mammalian series; and that their habits of life are almost entirely determined, as in the oviparous Vertebrata, by the instincts peculiar to each species, which are capable of very little modification either by education or by change of external conditions.

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* Journal of the Linnæan Society, vol. ii. 1857, p. 18.

II. The next well-marked stage in the development of the Cerebrum is where the corpus callosum is present, but connects cerebral hemispheres as little advanced in bulk or outward character as in the preceding sub-class; the cerebrum leaving both the olfactory lobes and the cerebellum exposed, and being commonly smooth, or with few and simple convolutions in a very small section composed of the largest members of the group. The Mammals so characterized constitute the sub-class *Lissencephala*, or smooth-brained; under which are ranked the orders Rodentia, Insectivora, Cheiroptera, and Bruta (this last being the equivalent of the Edentata of Cuvier). In each of these orders, many points of affinity to the oviparous Vertebrata are discernible; and notwithstanding that an undue reliance on dental characters has led almost all systematists to assign a much higher position to the Cheiroptera and Insectivora (the former having been generally ranked next to the Quadrumana, and the latter between the Cheiroptera and the Carnivora), we feel satisfied that in thus degrading them, Professor Owen has assigned to them a far more appropriate place than that which they have previously occupied. The aptitude of the Cheiroptera, Insectivora, and certain Rodentia, to fall, like Reptiles, into a state of true torpidity, associated with a corresponding faculty of the heart to circulate imperfectly-aerated blood, is an important physiological character of degradation; as is also the predominance still manifested in all these orders, as among the Bruta, of the instinctive over the intelligential direction of their actions. It is among the Rodentia and Insectivora that we find the most remarkable examples of the constructive and the migratory instincts, which the Mammalian class presents; and there are so many obvious points of parallelism between these two orders, that nothing but a foregone conclusion as to their dissimilarity could have led scientific naturalists to separate what less instructed observers have recognised as closely allied.

III. The third leading modification of the Mammalian cerebrum is marked by such an increase in its relative size, that it extends over more or less of the cerebellum and of the olfactory lobes. Save in a few exceptional cases of the smaller and inferior forms of the Quadrumana, the superficies is folded into more or less numerous gyri or convolutions; whence the name *Gyrencephala*, or convolute-brained, assigned by Professor Owen as the designation of the third sub-class, under which he includes the orders *Cetacea* and *Sirenia* (the former carnivorous, the latter herbivorous pisciform mammals), the Ungulata or hoofed quadrupeds (which he distributes under the four orders *Toxodontia*, *Proboscidia*, *Perissodactyla*, and *Artiodactyla*), and the Unguiculata, now limited to the *Carnivora* and *Quadrumana*. In this sub-class we look in vain for those marks of affinity to the Ovipara which are exhibited by the preceding; and in it "the Mammalian modification of the Vertebrate type attains its highest physical perfections, as manifested by the bulk of some, by the destructive mastery of others, by the address and agility of a third order. And, through the superior psychological faculties,—adaptive intelligence predominating over blind instinct,—which are associated with the higher development of the brain, the *Gyrencephala* afford those species which have ever formed the most cherished companions and servitors, and the most valuable sources of wealth and power, to Mankind."

IV. In Man, the brain presents an ascensive step in development, higher and more strongly marked than that by which the preceding sub-class was distinguished from the one below it. Not only do the cerebral hemispheres overlap the olfactory lobes and cerebellum, but they extend in advance of the one, and further back than the other. Their posterior development is so marked, that anatomists have assigned to that part the character of a third lobe; it is peculiar to the genus *Homo*, and equally peculiar is the posterior horn of the lateral ventricle 'and the hippocampus minor,' which characterize the posterior lobe of each hemisphere. The superficial grey matter of the cerebrum, through the number and depth of the convolutions, attains its maximum of extent in Man. With this highest form of brain are associated peculiar mental powers, the possession of which entitles the genus *Homo* to rank as the representative not merely of a distinct order, but of a distinct sub-class of Mammalia, for which Professor Owen proposes the name *Archencephala*, significant of its dominant superiority.

It is obviously a question of the first importance to determine whether in the ascending series marked out by these subdivisions, anything like a continuous gradation can be traced in the internal structure of the Cerebrum, by which Cuvier's well-known principle can be applied to the determination of the functions of parts successively added; or whether the differences between the several types of Cerebral organization are such as cannot be expressed by mere addition and subtraction, but involve fundamental differences of plan. On this point our information is still very scanty; and even with all the aid that we gain from the study of development, the reply must still be extremely vague. It seems to us most probable, however, from various data which we collect from the researches of MM. Leuret and Gratiolet, as well as from general considerations, that the latter view would be the truer one; so that to maintain that by simply abstracting or suppressing one part after another, we could bring down the brain of Man to the condition of that of a Marsupial, would be to affirm a proposition very far from the truth. As well might it be said that because the skeleton of a Fish is nearest the archetype, the suppression of those parts of the human skeleton which are peculiar to it would make it correspond with that of a fish. The general law of progress from the general to the special *must* apply, if it be the grand truth we hold it to be, to the development of the cerebrum as to that of every other organ; and however like the brain of the embryo Marsupial may be to the brain of the Human embryo, the subsequent difference is occasioned not merely by the advance in the development of the Human brain, but by the advance (though much more speedily checked) in the development of the Marsupial brain *along a different line.*

A single illustration will serve to convey our meaning. If the difference in the connexion of the two hemispheres were merely such as might be inferred from the suppression of the corpus callosum, then the psychical phenomena of the Marsupial (if we could rightly interpret them) would show us what functional deficiencies arise out of the absence of the great transverse commissure. But there is another most important difference, consisting in the very large size of the anterior commissure in the Marsupial; and although this may be partly accounted for by its relation to the olfactive lobes, which seem, when highly developed, to be specially connected through part of its fibres, yet it seems to be partly related to its function as a band of inter-cerebral connexion,—its fibres, as MM. Foville and Gratiolet have demonstrated, passing transversely through the corpora striata even in Man and the Quadrumana, to various parts of the hemispheres, especially (singular as it seems) to their posterior lobes. Thus the functions of the corpus callosum may very probably be performed in the Lyencephala by the anterior commissure, the high development of which seems to be one of the positive peculiarities distinctive of cerebral confirmation in that group. We doubt not that others would be discovered by careful examination, which would show that the cerebrum of the Marsupials could no more correctly be described by negative characters alone, than their genital apparatus could be characterized by its want of the parts that appertain to the Placental Mammals.

We will take another illustration from a different part of the cerebral organization. According to M. Gratiolet there is, in the Quadrumana and in Man, a large tract of fibres which passes at once from the optic nerves to the cerebral hemispheres, quite independently of any connexion with the corpora quadrigemina or thalami optici. The rays of its fan-like expansion may be easily traced, he affirms, into all those parts of the superior border of the hemisphere which are behind the posterior fold of the corpus callosum; but in front of this it is very difficult to follow them, on account of their interlacement with the converging fibres of the corpus callosum. Still he affirms that he has succeeded in tracing these fibres as far as the frontal extremity of the hemisphere; and that they have their special termination in that band of convolutions which extends along its superior border, and of which the high development is peculiarly characteristic of the human brain. Now in the Monkeys, the optic nerve is very large; but the portion of it which proceeds to the cerebrum is relatively small, a much more considerable part of its fibres proceeding to the corpora quadrigemina. In the inferior Mammalia, it is very difficult to discover the cerebral expansion of the optic nerve at all, owing to the deficiency of the posterior portion of the hemispheres, to which it may most readily be traced; still M. Gratiolet states that he is satisfied himself of its existence in the Carnivora, whilst in the Herbivora, and especially in the Rodentia, its presence is more doubtful. In the Marsupials, however, the roots of the optic nerve pass directly to their large tubercula quadrigemina and thalami optici, the cerebral expansion being altogether deficient. Here again, therefore, we have a case not merely of suppression but of substitution; for as the cerebral roots of the optic nerve diminish, the roots proceeding to the sensory ganglia augment not merely in proportionate but in absolute amount—a fact which (if M. Gratiolet's statement of it should prove correct) is of remarkable physiological import, as we shall presently show. And there is another circumstance to which we may refer in the same connexion; viz., that as we descend the Vertebrate series, the portion of the roots proceeding to the thalami optici diminishes, whilst that which passes to the tubercula bigemina (or proper optic ganglia) increases, and this not merely relatively but positively.

Now let us apply the same method of inquiry to the Cerebral hemisphere taken as a whole; and consider what evidence we have in regard to the homology of its parts in the higher and in the lower Mammalia. In the first place, has the ordinary division of the hemisphere into *lobes*, anterior, middle, and posterior, any real existence? The anterior lobe is, it is true, bounded inferiorly by the fissure of Sylvius, whilst the extension of the anterior cornu of the lateral ventricle into its substance seems a definite mark of its distinctness. But along the inner and outer faces of the hemisphere, and on its superior aspect, there is not the least mark of separation; on the contrary, an absolute continuity in the convolutions between the anterior and the middle lobes exists in every part but that into which the fissure of Sylvius extends; and when the anatomy of the Cerebrum is studied from the developmental point of view, it becomes obvious that the fissure of Sylvius, so far from constituting a division between the anterior and middle lobes, marks the real termination of the anterior lobe which is as it were folded back upon itself. A very simple illustration will render this apparent. Let the reader merely double his fingers down towards the palm of his hand, as if not quite clenching his fist; the palm will then represent the middle lobe, the fingers the anterior lobe, the line where the ends of the digits approximate the palm will be the fissure of Sylvius, and the space between the doubled phalanges will be the anterior cornu of the ventricle. It is obvious, then, that as the real continuity of the fingers and the palm is at the bases of the former, there is no true division between them where the bending of the fingers brings their points into proximity with the palm; and further, that the proper anterior termination of the member is not at the forward-projecting knuckles, but at the doubled-back tips.

Between the middle and the posterior lobes, there is no external semblance of a division; but the presence of the posterior cornu of the ventricle and that of the hippocampus minor are regarded as its characteristics. It may fairly be questioned, however, whether the presence of these parts can be regarded as establishing the fact that the posterior is altogether a superadded organ in Man; since the posterior cornu may be considered, like the anterior, merely as an infolded space; and the hippocampus minor is not a special organ, but merely a part of the ganglionic mass which is peculiarly shaped-out by the inward projection of one of the fissures dividing the convolutions at the inner side of the posterior lobe, where it is covered by the posterior fibres of the longitudinal commissure (Solly).

It seems to us, then, that each Cerebral hemisphere must be looked-on as a single organ, unless a truly natural division can be shown to exist in it, either by the evidence of its developmental history, or by the comparison of its convolutions in different groups of animals. Now if we compare the Cerebrum of a Rabbit (for example) with that of Man, either of three modes of development might be theoretically conceived possible. 1. The organ may enlarge as a whole, every part of it extending itself in equal or nearly equal proportion. In this case it is obvious that, notwithstanding the difference in relative size and configuration, the cerebrum of the Rabbit would be homologous *as a whole* with that of Man. 2. The organ may extend itself chiefly by outgrowth at its two extremities, just as a tree grows at the ends of the ramifications of its branches and of its roots; the anterior lobes budding forth first, and then the posterior, so as really to constitute new and distinct parts. In this case, the cerebrum of the Rabbit would in its enlarged form correspond only with the *middle lobe* of that of Man. 3. The principal growth may really take place in the central portion, new substance being interposed there, so as to separate the anterior and posterior halves, carrying the one forwards, and the other backwards, just as the epiphyses of a long bone are separated by the elongation of the shaft. In this case, the cerebrum of the Rabbit would really represent the *anterior* and the *posterior* lobes of that of Man; and the part peculiar to him would be the middle lobe. Between these possible modes, a more careful study of development than has yet been made may enable us to ascertain the true one. The study, however, is rendered extremely difficult by the softness of the embryonic cerebrum, which makes it difficult if not impossible to track the course of its fibres; and by the absence of any such definite indications, either on the surface or in the substance of the organ, as may afford a guide to the distinctness of its parts. The descriptions that have been given of the development of the posterior lobe, which is the last part to be formed, certainly favour the conclusion that this buds forth from the posterior extremity of the middle lobe; but we do not consider them by any means incompatible with the idea of such a shifting-back of a part of the substance of the middle lobe as our third hypothesis involves. And all that is known of the development of the anterior lobe is quite in harmony with the idea that this is an extended part of the original cerebrum, and not an addition to it.

Now if we inquire into the evidence afforded by the convolutions, we shall find that it rather favours the third hypothesis than others of the preceding. It may be true that the convolution termed by Foville *l'ourlet* or the hem, which is in immediate proximity to the corpus callosum, represents the same part of the cerebral hemisphere in all Mammals in which it can be distinguished. But it seems equally true that the anterior convolutions in the cerebrum of a Ruminant correspond with the anterior convolutions in the cerebrum of Man, and that the posterior convolutions which are continuous with them in the former are separated from them in the latter by a set of interposed convolutions of whose existence we have clear evidence in the elephant. "Suppose," says Leuret, "all the superior convolutions, and the prolongation of the great internal convolution which is united with them, to be obliterated, then the fourth anterior convolution might be united to the fourth posterior, the third to the third, and we should have one of the groups of convolutions of the brain of ruminants and solipedes." Now it is this series of interposed convolutions that especially distinguishes the brain of Quadrumana from the brains of other Gyrencephala excepting the Elephant; and its development is still more remarkable in Man. Hence we cannot help surmising that the principal extension of the Cerebrum in the antero-posterior direction is due rather to the introduction of a sort of wedge-shaped addition into the middle of the hemisphere, which not only separates its two extremities but causes them to bend downwards, than to the outgrowth of a new lobe at either end. At present we can only throw out these ideas as little better than speculations; our object is chiefly to show that they are at any rate as worthy of consideration, and have as much claim to acceptance as the views at present commonly entertained;

the time may perhaps not be far distant, when more certain data for the determination of this question may fall within our reach.

The advance in the development of the Cerebrum, however, which is so obvious in the ascending series of Mammalia, does not consist only in augmentation of size, and in extension of surface by the convoluted arrangement of the vesicular stratum; there is also a vast increase in the complexity of the arrangement of the fibrous tracts connecting the different parts of each hemisphere with each other; and connecting either hemisphere with its fellow as well as with other parts of the encephalon. Much attention has been bestowed by M. Gratiolet on the disposition of these tracts; and he gives an elaborate description of their course. Proceeding from the internal towards the external surface of each hemisphere, he enumerates the following as its constituent parts in the posterior part of the Human cerebrum:—1. The cortical layers of the internal surface. 2. The system of commissures proper to the internal surface, all of which attach themselves to the fibrous riband or *l'ourlet*. 3. The expansions of the corpus callosum which pass to the convolutions of the internal surface. 4. The internal wall of the occipital cornu of the lateral ventricle. 5. The external wall of this cavity. 6. Outside this wall, the expansion of the corpus callosum which Reil has designated the *tapis*. 7. Externally to the *tapis*, the stratum of the cerebral expansion of the optic nerve. 8. Beyond this, the stratum of the fasciculus which springs from the internal geniculate body. 9. Then the stratum of fibres directly proceeding from the peduncular fan. 10. The stratum of fibres expanding from the anterior commissure. 11. The lamina which, from the corpus callosum, descends into the inferior convolutions at the fissure of Sylvius. 12. The system of commissures proper to the external surface of the hemisphere. 13. The cortical layers of the external surface.—In the fronto-parietal region, the interlacements of these different strata are so complicated that their course cannot be traced with the same certainty as in the part of the hemisphere that lies behind the posterior duplicature of the corpus callosum.

As we have already adverted to M. Gratiolet's account of the double connexion of the optic nerve, we shall here only stop to mention the results of his inquiries into the connexions of the corpus callosum; as to which there has been, as many of our readers are aware, an important difference of opinion amongst some of the most able neurologists,—Arnold and Reil, followed by most British authorities, affirming that its fibres pass directly from the convolutions of one hemisphere to those of the other, whilst Foville has adopted the other idea of Willis that it is a commissure between the peduncles and not between the convolutions of the hemispheres, which has also been supported by Tiedemann and Rolando. According to M. Gratiolet it is not difficult to produce, by a particular mode of preparation, each of the appearances on which the above conclusions are based; so that each may be accepted as having a measure of truth, but not as being the whole truth. How, then, are these two opposing views to be reconciled? According to one possible hypothesis, the corpus callosum contains two distinct systems of fibres, one passing between the convolutions of the hemispheres, the others uniting by transverse arches the coronæ of the peduncles. According to another, the corpus callosum is formed of fibres proceeding from the corona of one peduncle to the convolutions of the opposite hemisphere. It is very positively affirmed by M. Gratiolet that the second of these hypotheses must be the correct one; for that in no instance has he been able to trace any fasciculus continuously from the convolutions of one hemisphere to those of the other; whilst he has found every one which he could trace so far, arching downwards into the peduncle of the opposite hemisphere. This solution appears by no means improbable in itself; and it has this great claim upon our admission of its high probability, that it reconciles the apparently contradictory statements of most experienced and trustworthy anatomists, without invalidating the real basis on either side.

In his chapter on the Development of the Encephalon, M. Gratiolet warns us against being led astray by the analogies which unquestionably present themselves between its transitional forms in Man and its permanent conditions in the lower

Mammals; and remarks that Tiedemann and those who have followed him have done wrong in attaching themselves too exclusively to these analogies, to the neglect of the differences. And he lays great stress on the fact that there is no necessary parallelism between the order of the *appearance* and that of the *development* of the different parts, or (as we presume that he means to say) between the *morphological* and the *histological* transformation of the organ. Thus notwithstanding a certain general resemblance in plan between the cerebrum of Man and that of the Quadrumana, there are very marked differences in their respective courses of development. For in the ascending series of Monkeys, the completion of the general form is found to precede the first appearance of convolutions; whilst in Man the convolutions appear and multiply a long time before the general form is complete. The order in which the several convolutions appear is almost opposite in the two cases; the alphabet being recited (so to speak) from alpha to omega in the one case, and from omega to alpha in the other. Thus it happens that at no epoch in its development can the human brain be said to resemble that of any monkey; and that if its development be arrested in any inferior stage, the organ still presents characters which are peculiarly anthropic. It is somewhat remarkable that the two hemispheres do not seem to develope themselves in an absolutely symmetrical manner: the frontal convolutions most frequently appearing earlier on the left side, whilst those of the occipito-sphenoidal lobe are first seen on the right. We do not attach much importance to this observation, except from its concurrence with the well-known fact that there is a less perfect symmetry between the convolutions in the completely-developed hemispheres of Man, than there is in the Mammalia generally.—Without wishing to undervalue the information it affords, we feel constrained to say that we are far from being satisfied with this portion of M. Gratiolet's work: and have no hesitation in affirming that the true developmental history of the Human Encephalon has yet to be worked out. In its earlier stages there will doubtless be found a close conformity to the general Mammalian type; and up to a somewhat advanced condition its development will probably be found to run tolerably parallel with that of the higher Gyrencephala; but those later stages which mark its distinctive characters, require to be worked out in far more minute detail than that of which we find evidence in M. Gratiolet's book; though we meet with sundry hints, that lead us to surmise that he has accumulated a mass of material relating to this subject, which for some reason or other he has not thought it suitable to bring forwards.

We have carefully examined the Second Part, which treats of the Experimental History of the Nervous Centres, and of the Intelligence, for some addition to our knowledge, in the way of facts, inferences, or suggestions; but we are bound to say that we have found it singularly barren of novelty, and so far behind the state of opinion in this country, that we should have to go back a quarter of a century at least to bring ourselves down to its level. It will be sufficient to indicate, by way of example, that the psychology of vision is discussed without any reference to Professor Wheatstone's researches, and that under the head of the effects of imagination the phenomena of Mesmerism are discussed without the least reference to the experiments of Mr. Braid.

We now turn to Mr. Dunn's Essay, which contrasts most favourably with M. Gratiolet's more pretentious treatise in the intelligent appreciation it displays of the fundamental questions of physiological psychology, and in the value of the suggestions it throws out with a view to the elucidation of them. Avowedly taking his stand on the views which we recapitulated at the commencement of this article, the author looks at the Cerebrum as an organ superimposed upon the centres of the sensorial consciousness, to serve as the instrument of the faculties both of the Perceptive and of the Intellectual consciousness; and he considers its type through the whole Vertebrate series to be essentially the same, its evolution taking place from before backwards, in a series of ascending planes of development. The longitudinal convolutions, from their first marking-out, increase in number, volume, and extension backwards as the animal rises in the scale of being, and as the range of its perceptive intuitions or faculties widens. The primitive basement convolutions of the hemispheres,—the great internal, or *l'ourlet* of Foville,—form in Mr. Dunn's opinion the broad boundary lines of demarcation between the sensory and the perceptive ganglia, between the sensational and the ideational apparatus; being, in fact, as the central organs of the perceptive consciousness generally, the common portals to intellectual action and volitional power. Now with these basement convolutions, all the other longitudinal convolutions of the hemispheres are directly connected and associated; and Mr. Dunn is strongly impressed with the conviction that the entire series forms an aggregate or whole, constituting the nervous apparatus of our perceptive consciousness,—the instrument of all our immediate cognition, not merely of external existences, their sensible qualities and physical attributes, but also of those purely ideational activities which constitute so large a part of our mental operations, and which, together with the emotional conditions that are closely associated with them, make up the essential part of the character of each individual.

In its lowest grade of development, the Cerebrum is regarded by Mr. Dunn as consisting essentially of the basement or internal convolution, and of its anterior and basilar connexions; and looking to the faculties of the animals which are characterized by this grade of cerebral development, he thinks that we should justly consider the function of the anterior portion of their hemispheres to consist in the acquirement of a perceptive knowledge of external things, whilst the posterior or basilar minister to those elementary feelings and appetites which may be supposed to be common to all Vertebrata, such, for example, as the love of life, the alimentative propensity, the sexual appetite, and the tender regard for offspring. Mr. Dunn thinks that it can scarcely be denied that the intuitions (whether original or acquired) which are called forth by the activity of the special senses, and with which are allied the feelings, appetites, and instincts that have reference to the demands of our corporeal nature, constitute the chief and prominent part of the mental life of the inferior Vertebrata, while at the same time they constitute the inferior region of the more elevated mind of Man, and enter largely into the complicated web of his conscious existence.

Upon this foundation there is erected in the superior Mammals a higher plane of cerebral development, in which the perceptive apparatus presents itself in a more extended form. For the longitudinal convolutions are carried upwards above the lower perceptive organs, and are prolonged backwards even beyond the median lobe; and with this mark of elevation Mr. Dunn thinks that we may associate that higher exercise of the perceptive faculties, in which there is a recognition not merely of the sensible qualities and physical attributes of external things, but of their differences and relations, of their order, arrangement and number, of the phenomena of their action (or events), and in which the higher individual or personal affections, such as love of self and love of others, are called into play.

In the highest plane of cerebral development, which is exclusively human, the towering longitudinal convolutions receive the completion of their development both anteriorly and posteriorly; and with this are associated in Mr. Dunn's idea that more elaborate and complex action of the perceptive consciousness, and that operation of the moral and religious feelings, which are the distinctive prerogatives of Man.

So far we have been concerned with those forms of cerebral activity alone which minister to the various kinds of perceptive activity that directly result from the contact of the perceptive faculties of the Mind with the external world or Nature. But to *perceive* and to *think* are distinct mental processes, though indissolubly connected, since ideation is the pabulum of thought. All our representative knowledge is the product or creation of the mind's own power of intellection; and of this power Mr. Dunn holds that the transverse convolutions of the Cerebrum serve as the instrument, these being anatomically a distinct series, and having no direct connexion with the basement or great internal convolutions. The appeal must be made for the confirmation or the setting aside of these views, to the brains of the most rational among Mammals; since, if such should exhibit a high development of the transverse convolutions, whilst in species merely allied to these, whose habits appear to be governed by instinct alone, the transverse convolutions are correspondingly small, a very important probability will be afforded in favour of Mr. Dunn's hypothesis.

We believe that no intelligent psychologist will now be inclined to dispute that the true difference between Man and other Mammalia rests specifically and fundamentally on the greater number and the higher nature of his intellectual powers and his emotional states; the less elevated forms of both being common to him with animals much lower in the series. Hence, in Mr. Dunn's opinion, the tripartite division of the Cerebrum into lobes, and the order and succession of their development, point to some such inferences as the following:

1. In animals which only possess the representatives of the *anterior* lobes, these must necessarily be the seat of the mental faculties which such animals manifest.

2. If the posterior lobes be exclusively Human, they must be the seat of exclusively human faculties.

3. The middle lobe may perhaps be regarded as the seat of those affections which are more of a personal or individual character, from the circumstance that it is more directly connected than the others with the sensory apparatus, and especially, by the olfactory peduncles, with the sense of smell, which is the guiding sense to the gratification of the first instinctive want, or craving for the conservation of existence, of the young Mammal. Hence the *love of life* may be regarded as one of the primordial forms of cerebral activity, and may be fairly located in the primordial convolutions.

As we have already expressed our opinion upon several of the points involved in Mr. Dunn's statement of doctrines, we shall limit ourselves to a general statement of what we believe to be the present aspect of the question. We fully agree with him as to the *method* of the inquiry, and believe that, rightly followed out, it will lead to important results. But it appears to us far more difficult than he would seem to suppose, to determine in the first place what are the mental phenomena in the lower animals which are homologous with those of man; and secondly, to determine what are the homologous parts of the cerebral structure. We have pointed out on former occasions what a fundamental difference exists between the instinctive and the intelligential sources of operations that may really be in themselves of the same character; the action in the former case being the direct response to an external impression, which only involves the sensori-motor apparatus, whilst in the latter it is the product of cerebral activity to which external impressions may only afford the remote stimulus. The architecture of the bee as compared with that of man, the migrations of the locust in search of food as compared with man's voyages across the ocean with the like object, the nurture of the eggs and pupæ so remarkable in the ant as compared with the thoughtful care bestowed by the well-trained human mother upon her offspring, are examples of the difference to which we allude. In the case of Invertebrated animals, we have not much difficulty in coming to the conclusion that even the most striking manifestations of adaptiveness do not indicate that the source of this adaptiveness exists in the *mind* of the animal; since it may be fairly set down, whenever it is the constant habit of the species, to the account of its bodily organization, like the equally adaptive actions of coughing, sneezing, &c., in ourselves. But when we have evidence of the dawning of the truly rational powers, corresponding with the progressive evolution of the cerebrum, there comes to be a great difficulty in separating the instinctive or intuitional from the intelligential direction of the actions. We do not say that this difficulty is insurmountable; we believe that it may be in great degree got over by careful observation and judicious analysis of facts; but at present we feel assured that it is premature to base any induction upon the comparison of the psychological manifestations of the lower animals with our own.

We may take a fundamental question of visual perception as an illustration our remarks. We have lately been engaged on a careful analysis of the sources of our appreciation of the solid forms and relative distances of objects; and we have come to the conclusion that even that which we gain through the medium of binocular vision, and which comes to us so directly and instantaneously as to have much of the character of an instinct, is really the result of long experience in the interpretation of visual, tactile, and muscular sensations; the only part which can be regarded as an original intuition being the sense of direction, by which we instinctively refer an impression made upon the retina to an external source having a certain related position in space.* Now, we think there is ample evidence that the appreciation of the exact distance of near objects is an original and not an acquired intuition with many of the lower animals; or rather, we should say, that the movement which results from the visual impression does not acquire any perceptive appreciation at all. And this being the case, we should refer the resultant action, which in man has a cerebral source, to the most direct prompting of the optic ganglia. Now it is not a little confirmatory of this view, that, if M. Gratiolet's statement be correct, the proper cerebral termination of the optic nerve gains rapidly upon its sensorial roots in the ascending series of Mammalia, and is particularly developed in Man. And if he be further justified in the assertion, that the fibres of this cerebral root directly proceed to all parts of the surface of the hemispheres, but primarily and especially to the posterior, it seems to us that one of the chief foundations of Mr. Dunn's theory,—namely, the primordial nature of the great internal convolution,—is knocked away. The anatomical relations of what Mr. Dunn regards as the primitive convolutions to the superadded, do not at all correspond to our notion of their physiological connexion. For we should have expected that if there were any portion of the cerebrum set apart for the perceptive conciousness, it would have been a stratum of grey matter interposed between the sensorial centres and the convolutions, and not any portion of the periphery itself.

Again, it goes very much against our notions of cerebral action to suppose that any particular set of convolutions are the instruments of thought and reflection, as distinguished from simple ideation. Our whole mental life consists in a succession of ideas and of feelings connected with them; and that succession, the order of which is determined *suggestively*, seems to us much more related to the mode in which the different parts of the convoluted surface are connected with each other, than to the independent operation of any particular parts of them. We are by no means opposed to the notion that different parts of the convoluted surface may be instrumental in the formation of different classes of ideas (though we do not as yet see any valid evidence that they are so); but the linking-together of these ideas by associations of various kinds, on which depends the growth of our reasoning powers as well as of the imaginative and poetic faculty, can hardly be the work of a set of convolutions disconnected from the rest. Besides, the transverse convolutions, to which Mr. Dunn assigns this office, are already developed as fully as any other part of the hemispheric ganglia, in the brain of the infant; and yet its mental life for years to come is scarcely elevated to all appearance above that of the brutes. But during those years we believe that a store of perceptions and of simple ideas founded on them is being laid up; and that a development of commissural fibres is taking place,—partly according to an original plan, varying, like the shape of the brain itself, in each individual, and partly in accordance with the habitual mode in which the organ is used,—which fibres co-operate with the vesicular matter of the convolutions they connect, in establishing those peculiar relations between ideas, which determine their succession in the modes which we denominate Memory, Imagination, and the like.

In thus expressing our differences from Mr. Dunn, we must at the same time record our sense of the great value of his Essay, as one calculated to prove a stimulus to thought in others, and to direct intelligent minds to a field of inquiry which can only be really productive when cultivated by men of Mr. Dunn's powers of observation and reflection. The difficulties of the work are great; but that should not be a ground of discouragement, for difficulties were made to be overcome; and the example of Mr. Dunn is sufficient to show that a man who has at the same time the capacity and the inclination to do something for the promotion of scientific inquiry, will find means to do so even in the midst of the distracting cares of an active professional life. The more extensive his practice, indeed, the more sure he is to find something worthy of being noted and recorded; and we would point to the succession of cases which Mr. Dunn has published at various times, as among the most valuable contributions to this department of inquiry with which we are acquainted,—in regard alike to the mode in which their psychological and their pathological phenomena are recorded.

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* See Article, Binocular Vision, in Edinburgh Review, Oct. 1858.

Review IV.

On Chloroform and other Anæsthetics; their Action and Administration. By John Snow, M.D., Licentiate of the Royal College of Physicians. Edited, with a Memoir of the Author, by Benjamin W. Richardson, M.D., Licentiate of the Royal College of Physicians.—*London*, 1858. pp. 443.

A panic has lately seized the public mind in reference to the employment of chloroform, because in rapid succession several deaths have occurred which appeared fairly attributable to this agent. If these deaths have proved anything, they have proved the truth of the observation of the earnest worker, now, alas, no more among us, that chloroform partakes of the character of all powerful agents in requiring to be used with care and with a due regard to the ordinary laws both of pneumatics and of vital dynamics. To judge by the remarks that meet us on all sides, one would suppose that a grand discovery had just been made that anæsthesia was to be purchased only at the risk of life, and that medical men had hitherto been using chloroform, ether, or amylene at haphazard, rather perhaps with a view to saving themselves trouble, than for the purpose of relieving their patients of pain and suffering. But as medical men seized avidly upon the great suggestion of etherization, because they felt it to be one of the greatest boons that could be conferred upon mankind, and a certain means of advancing the science and art of medicine, so they also at once recognised the dangers which surrounded the employment of anæsthetics, and sought to determine the exact limits which should guide us in their administration. No one thinks of decrying railways because of the occasional accidents, however fearful, which are attributable to negligence in some quarter; nor is our steam fleet in danger of being abandoned, and emigration arrested, because terrific conflagrations or explosions prematurely hurry hundreds of men, women, and children into the arms of death, owing to culpable neglect of precautions which we all know to be necessary. Dr. Snow, more than any other physician, has assisted in promoting the use of anæsthetic agents after their first introduction into the practice of medicine; but no one more fully appreciated the dangers which may surround its employment, or sought more earnestly to prevent the occurrence of those lamentable issues which from time to time have startled us from our feeling of security. The numerous experiments so carefully conducted by Dr. Snow amply testify to the sound judgment which he held upon these questions. He was not led away by enthusiasm to overlook the necessity of well weighing all the circumstances that influence the result produced by chloroform and its congeners; and his large experience at the operating-table and at the bed-side only served to stimulate this earnest student to ever renewed efforts, not only to lessen the risks attending the use of known anæsthetics, but also to discover others which might be exempt from the charge of endangering the patient, even if exhibited in a careless manner. He thought at one time that he had in amylene found a hydrocarbon which accomplished all he desired; but two deaths which occurred under his own administration bitterly disappointed these hopes: yet the energy and indomitable perseverance of the man would certainly have carried him on to ultimate and complete success had not the hand of death been laid upon him, and arrested a career which may well serve as an example and as a beacon. The interesting and warm-hearted memoir prefixed to Dr. Snow's work, by one whom we all know, tells us of no thrilling deeds and hairbreadth escapes, but it informs us how, by hard work and determination, Dr. Snow overcame the dragon that opposes the progress of so many a London physician, and had reached the tree from which the Golden Fleece was suspended; it reminds us how, as early as 1848, Dr. Snow saw the principles upon which the action of narcotic inhalations are based, and knew their dangers commensurate with their power of doing good.

> "He demonstrated that these substances modify, and in large quantities arrest, the animal functions, in the same way and by the same power as that by which they modify and arrest combustion; the slow oxidation of phosphorus and other kinds of oxidation unconnected with the living body when they (the narcotics) are mixed with the atmospheric air. . . . . Placing a taper during one of our experiments in a bottle through which chloroform vapour was diffused, and watching the declining flame, he once said, 'This, now, is all that occurs in narcotism; but to submit the candle to the action of the narcotic without extinguishing it altogether, you must neither expose it to much vapour at once, nor subject it to the vapour too long, and this is all you can provide against in subjecting a man to the same influence. I could illustrate all the meaning of this great practical discovery of narcotism on a farthing candle, but I fear the experiment would be thought rather too commonplace.'" (p. xvii.)

To those who have followed the medical literature of the last ten years, Dr. Snow's views will be known from the various papers which he has published in various periodicals, but especially* in the old Medical Gazette during the years 1848–1851. The present volume is, however, by no means a *réchauffé;* new experiments, and a careful summary of the very extensive experience which Dr. Snow has enjoyed in the use of chloroform, in surgery and medicine, since its first introduction into practice, are added, and the whole forms a work which ought to be read by all practitioners. They can nowhere find a more lucid account of very important inquiries into the physiological and therapeutic action of chloroform and some other anæsthetics; and whatever the ultimate fate of chloroform may be, the labours of Dr. Snow will ever serve as a guide and pioneer to those who wish to extend and establish the benefits that accrue to suffering humanity through the removal of pain.

The book was, so to say, completed on the death-bed; the editor has only added the index and the memoir already alluded to; we have, therefore, as every page testifies, the *bonâ fide* production of the lamented Dr. Snow. After an historical introduction, and some general remarks on inhalation, the properties and preparation of chloroform, with its physiological effects, and the circumstances modifying those effects, are examined. Most medical men are familiar, from observation, with the various degrees of narcotism which the inhalation of chloroform is capable of producing; but we fear that even those who habitually employ it, or cause it to be employed, are unacquainted with the valuable experiments and calculations of Dr. Snow, made with a view to determining the exact ratio that exists between the quantity of vapour introduced into the circulation, and its anæsthetizing effects. The experiments were made by introducing into glass jars, birds, guinea-pigs, cats, with definite quantities of chloroform; the cubic contents of the jar being ascertained, it was easy to calculate the exact amount of saturation of the contained air. By noting the time at which the different effects of the vapour became perceptible, and comparing the results thus obtained with one another, and with the data that we possess regarding the respiratory functions of man, Dr. Snow has arrived at a series of conclusions which establish his character as an inductive philosopher. The book before us happens to be open at page 66. We quote the fifteenth experiment as an instance of the author's mode of procedure :—

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* Vol. xli. pp. 850, 893, 1074; vol. xlii. pp. 390, 412, 614, 840, 1031; vol. xliii. pp. 298, 451, 692, 963; vol. xliv. p. 979; vol. xlvi. pp. 321, 749; vol. xlvii. pp. 194, 622.

> "A cat was put into the jar holding 3000 cubic inches, and 82·5 grains of chloroform were introduced, being two grains and three-quarters to each hundred cubic inches. In five minutes it had evaporated, and the cat began to stagger in its walk. In two minutes more it was unable to stand. Five minutes after the chloroform had evaporated the cat was quite insensible, and breathing one hundred and twenty-six times in a minute. It was now taken out. The temperature in the axilla was 98°. In half an hour after its removal from the jar it had recovered its consciousness, but was still drowsy.
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> "It was now put into the jar again, and the same quantity of chloroform was introduced in the same manner as before. In five minutes it had evaporated and the cat was again insensible. In other five minutes it was breathing rather deeply forty-eight times in the minute. Twelve minutes after the chloroform had evaporated, the cat was breathing in the same manner, but sixty-eight times in the minute. The breathing afterwards became shallow and feeble, and half an hour after the chloroform had evaporated it was eighty-eight in the minute. In five minutes more the breathing ceased. The cat was taken out of the jar, and the stethoscope was applied to the chest. The heart could not be heard to beat at first, but in a short time the cat gave a gasp, and the heart's action returned, and the breathing became re-established."

The conclusions which Dr. Snow draws from the experiments are probably known to our readers; but no harm can result from those which have an especial practical bearing being again put upon record. He shows that one grain of chloroform to each hundred cubic inches of air suffices in cats, guinea-pigs, and mice, to induce the second degree of narcotism, or that state in which consciousness and voluntary motion are nearly but not entirely abolished. Again, he finds that serum at 100°, and at the ordinary pressure of the atmosphere, dissolves about its own volume of vapour of chloroform; and he calculates that one part in 16,285 is the average proportion of chloroform by measure in the blood in the second degree of narcotism. Assuming the total amount of blood in the human adult to be, according to Valentine's calculations, about 30 pounds, he infers that twelve minims of chloroform is the quantity which must be received into the circulation to produce narcotism of the second degree. A larger amount is employed in practice, because so much is lost in the processes ordinarily used for inhalation; but Dr. Snow has found that by introducing twelve minims into a bladder, and causing the vapour to be inhaled over and over again, as is done with laughing gas, narcotism of the second degree is produced.

This is clearly one important fact not to be lost sight of in the administration of chloroform. The second and parallel one in its bearing upon practice is the proved necessity of avoiding the introduction of a large quantity of vapour suddenly into the circulation, in order to enable the system to accommodate itself to the new conditions under which it is placed. Although we feel assured that death may be induced by chloroform in various ways, there is ample evidence to prove that in the great majority of cases it results from the paralyzing influence which the vapour exercises upon the heart. In most of the fatal cases, the presence of a weak and more or less degenerated heart has facilitated this deleterious action; but Dr. Snow has proved to us that even in diseased conditions of the heart we need not fear the administration of chloroform by inhalation, provided we guard against the introduction of a disproportionate amount at once—provided we see that the air taken into the circulation is not charged with too much of the vapour of chloroform, especially at the commencement of the operation. It is manifest that it must be desirable to graduate the amount of chloroform vapour taken up by the air inhaled with as much exactitude as possible, and that it is impossible to do so if the chloroform be applied to the nostrils on a handkerchief, or on lint folded together. The most exact way, as might suggest itself from what has preceded, would be to introduce a measured quantity into a bag, and having filled it by means of bellows, to allow the patient to inhale it again and again. Dr. Snow tried this plan, and found that he obtained very uniform results; but although he regarded the plan as perfect so far as regarded the safety of the patient, he abandoned it on account of certain practical inconveniences which accompanied it. We should also imagine that were this system generally carried out, although there would be no danger from paralysis of the heart, the danger from asphyxia would still be liable to interfere with the uniform and absolute success of the agent. To meet the various requirements of the case, Dr. Snow introduced an inhaler (invented by Dr. Sibson), which allows of a gradual admixture of air and chloroform vapour, and enables the person giving the chloroform to dilute the vapour to any extent by the adjustment of the valves.[*] The only objection that can be offered to this apparatus is, that patients are apt to take alarm at the use of an unknown instrument, more than at the application of a pocket-handkerchief to their mouth and nostrils. But medical men ought to be aware of the grounds upon which they should seek to overcome such trivial objections, since the question involved is not one of mere convenience, but may be one of life or death.

The collection of fatal cases which had occurred up to the time of Dr. Snow's death, and the careful analysis of the symptoms and post-mortem appearances, alone serve to render the work one of great value. We again see a reason for admiring the assiduity of the author, as well as the scientific zeal which induced him carefully to probe the virtues of his favourite remedy in every direction. It was shown by Dr. Glover, as early as 1842, that chloroform destroys the irritability of the hearts of animals in whom it had been injected into the jugular vein. Dr. Snow has found that the direct application of the vapour of chloroform to the heart arrests its action; the suddenness of the fatal issue in most of the persons who have died from the effects of chloroform, the sudden pallor which spreads over the patient, the arrest of the pulse commonly even before respiration has come to a complete standstill, the frequent presence of fatty degeneration of the muscular tissue in the heart, lead to the conclusion that arrest of the heart's action is the cause of death. It is fair to infer that in that case it is due to an overcharge of the vapour in the blood carried back from the lungs to the left side of the organ. But it is impossible to study the cases brought together by Dr. Snow, without arriving at the conclusion, that if death ensues in the majority of cases by paralysis of the heart, spasm of the glottis, asphyxia, and coma,[†] though less frequently, come in for their share in the causation of the fatal issue. It behoves the chloroformist to bear in mind that each of the organs in which the thread of life may be cut deserves watching, but that the greatest danger, the cessation of the heart's action, may prove irretrievable, if at the first outset, as well as during the later stages of inhalation, the blood conveyed to the heart is charged beyond a certain point with the narcotizing vapour.

In concluding this brief notice of Dr. Snow's labours, we would once again urge upon the profession at large the importance of studying carefully the results obtained by our laborious and lamented fellow-worker. We shall be glad to find that his mantle has fallen upon some earnest inquirer, whose claims upon our attention shall be based upon a higher merit than the only one of having administered chloroform, or having seen it administered, in so many thousand cases. We may not, for the sake of science or humanity, allow the great achievement of modern medicine to slip from our grasp because some flaws are yet perceived in its structure. Whether by local applications, or by bringing the whole system under a Lethean influence, we succeed in diminishing bodily suffering, we must seek to obtain perfect control over our agent; but we can no more revert to the times when surgical manipulation and the infliction of pain were synonymous. Yet a little more perseverance and a little more patient research, and we may hope to secure anæsthesia without even the comparatively trifling dangers which still surround its production when properly given. But whenever, in the history of medicine, the subject shall be touched upon, the name of Dr. Snow will stand out prominently, and will receive reverential mention. To use the words which we remember to have heard some ten years ago in the Harveian oration delivered in the College of Physicians by Dr. Wilson, he will ever remain "*Nix per æthera notus.*"

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[*] It may be well to suggest the necessity of always carefully examining the valves, to be certain that they act readily; the valves occasionally get disarranged, so that powerful efforts at inspiration become necessary, which may give rise to injurious results.

[†] We may refer the reader for further information on the cause of death from chloroform to the experiments of Dr. Faure (Archives Génerales de Medecine, Juin, Juillet, Aout, Sept., Oct., 1858); and of Dr. Chapman (Med. Times and Gazette, Oct. 30, 1858).

Review V.

1. *Klimatologische Untersuchungen, oder Grundzüge der Klimatologie in ihrer Beziehung auf die Gesundheits-Verhältnisse der Bevölkerungen.* Von A. Mühry, M.D., &c.—*Leipzig und Heidelberg*, 1858. 8vo, pp. 816.

Climatological Investigations, or Outlines of Climatology in its relation to the Sanitary Condition of the Population in different Countries. By A. Mühry, M.D.

2. *Hygiene, or Health as depending upon the Conditions of the Atmosphere, Foods and Drinks, Motion and Rest, Sleep and Wakefulness, Secretions, Excretions, and Retentions, Mental Emotions, Clothing, Bathing, &c.* By James H. Pickford, M.D. —*London*, 1858. 8vo, pp. 290.

" 'Climate,' in our sense of the word, comprehends all the physico-geographical forces which act perceptibly upon our organs, which are consequently variously distributed throughout the earth, and are possessed of etiological importance. In the more extended sense of the term, it relates to all organic beings, and therefore to the sciences of botany and rural economy; but in the present work it is considered, at least in the first instance, only in reference to the human race."

Such is the definition of Climate given by the author of the work whose title stands at the head of this article. It is, of course, principally in a medical point of view that we shall just now have to consider climate; but there are other aspects scarcely less important to the well-informed physician under which the subject may be studied. These may be found ably unfolded by Mr. Buckle* in the second chapter of his recently published and very remarkable work, in which he considers the "influence exercised by physical laws over the organization of society, and over the character of individuals."

The physical agents alluded to Mr. Buckle classes under four heads—Climate, Food, Soil, and the General Aspect of Nature. "The word climate, I always," he says, "use in the narrow and popular sense." Dr Forry and many previous writers make it nearly coincide with "physical geography." "Climate constitutes," says Dr. Forry, "the aggregate of all the external physical circumstances appertaining to each locality in its relation to organic nature." (p. 37, *note.*)

Mr. Buckle considers climate, food, and soil, not separately, but under the several heads of the effects produced by their united action—one of the earliest and most important of which is through the accumulation of wealth, the creation of an intellectual class, and the consequent increase of knowledge; secondly, the increase of population, and especially of the labouring class, will be modified by the cheapness and abundance, or, on the other hand, by the scarceness and dearness, of the national food. That the abundance or scarcity of food will in a great measure directly depend upon the climate, is self-evident. But in this respect climate will also have an indirect action; for as in hot countries the animal heat is more easily kept up, and as, on the whole, the bodily exertions of their inhabitants are less frequent, and the decay of tissue is consequently less rapid, smaller supplies of both non-azotized and azotized food will be required in the hotter than in the colder regions. But it follows that in colder regions a more highly carbonized diet will be required; and it is a remarkable fact, that highly carbonized food is more costly than food in which comparatively little carbon is found. Moreover, carbonized food being drawn chiefly from the animal world, and being more difficult to obtain than oxidized food, which is derived from the vegetable kingdom, there is for the most part displayed, even in the infancy of society, among those who require the former, a bolder and more adventurous character than is found in nations "whose ordinary nutriment, being highly oxidized, is easily obtained, and, indeed, is supplied to them, by the bounty of Nature, gratuitously and without a struggle" After some further observations on the subject, the author continues:—"We shall now, I trust, be able to discern, with a clearness hitherto unknown, the intimate connexion between the physical and the moral world, the laws by which that connexion is governed, and the reasons why so many ancient civilizations reached a certain stage of development, and then fell away, unable to resist the pressure of nature, or make head against those external obstacles by which their progress was effectually retarded." (p. 63.)

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* History of Civilization in England. By Henry Thomas Buckle. London, J. W. Parker & Son, 1857. Vol. i. General Introduction, Chap. 2.

The foregoing may suffice to show the importance of climate in the points of view under which it is so ably considered by Mr. Buckle. In now turning to the more medical aspects of the question, we shall place ourselves chiefly under the guidance of Dr. Mühry.

This writer commences with the climatology of mountains, in investigating the effects of which upon the human organism the following elements are to be taken into consideration:—1, the diminishing temperature as we ascend; 2, the diminishing density of the atmosphere; 3, the diminishing quantity of watery vapour (with, at a certain height, in consequence of the fall of temperature, an increased amount of precipitated moisture); 4, the increasing force of evaporation.

As the surface of the earth between the Equator and the Poles is divided in reference to temperature into three zones, the author distinguishes in mountains three corresponding vertical regions: 1, the lowest or hot region, extending at the Equator from the level of the sea to a height of 3000 feet; 2, the middle or temperate region, reaching at the Equator from 3000 to 12,000 feet; 3, the superior or cold region, extending at the Equator from 12,000 to 16,000 feet.* But as these elevations diminish as we recede from the Equator, though not everywhere with uniformity, it is evident that the conditions of the several zones as to atmospheric pressure, moisture, &c., must vary in different latitudes.

In reference to the nosography of mountains, four classes of diseases are to be distinguished: 1, diseases which occur everywhere, and are consequently to be met with here; 2, diseases the distribution of which depends exclusively on temperature; 3, diseases which are to be termed proper mountain diseases; 4, diseases which are absent in these regions.

I. Ubiquitous diseases are those which are not limited by temperature, and occur in all three regions—zymotic diseases, small-pox, scarlatina, measles, hooping-cough, catarrh, influenza; also diseases depending upon dyscrasia; tuberculosis (but not pulmonary tubercle), scurvy (?), scrofula, hæmorrhoids, dropsy, furunculosis, anthrax, gangrænescentia (?), carcinoma, helminthiasis, herpes, scabies, strumosis, uro-lithiasis.

II. The three vertical regions, as they are above assumed, are characterized by the following distinctive morbid constitutions, so far as these are dependent on temperature; which morbid conditions both afford analogy to the horizontal distribution of disease over the degrees of latitude, and are also confirmed by experience; on this point, however, much is still to be expected from more accurate statistical data. The author enumerates them in detail, that the accuracy of the classification may be tested as opportunity offers.

1. *Upper, or cold region*, mean temperature from 32° to 41° F. The physiological predisposition is: plethora, the general character is inflammatory, the predominant organic tendency is to the respiratory organs.

Diseases of frequent occurrence are: influenza (whether it occurs more frequently, or as often as at lower elevations, is indeed not yet decided), erysipelas, metritis puerperalis (only supposed from analogy), catarrh, pneumonia (these two are designated very severe diseases in elevated situations), croup, trismus neonatorum (these two forms are only conjecturally of frequent occurrence), ophthalmia.

Absent diseases are, the three terrestrial miasmatic epidemics: malaria fever (at least it is much rarer and weaker), yellow fever (partly on account of the distance from the sea, partly on account of the temperature), and Indian cholera (at least in the highest region the occurrence of this disease is scarcely possible). Phthisis may here very properly be said to be absent (although probably not in consequence of the temperature, but of the rarefied condition of the air); dysentery is scarcely to be found so high, neither is hepatitis, nor lepra, nor gangrene, nor are indolent ulcers to be met with.

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* The measurements of heights in Dr. Mühry's book are given in Parisian feet, of which 15 are equivalent to about 16 English feet. A Parisian is to a Prussian foot nearly as 29 : 30.

2. *Middle, or temperate region*, distinguished into cooler and warmer, namely, from 41° to 54°·5 F., and from 54°·5 to 72°·5 F. respectively. The physiolgical predisposition is: fluctuation of the phenomena according to the seasons; their character is in winter more inflammatory; in summer it is more torpid: the predominant organic tendency, too, is in winter rather to the respiratory, in summer to the digestive organs, but the difference produced by the seasons is not so great as in the lower region.

The more frequent diseases are: typhus, gout, diseases caused by cold, as catarrh, angina (refrigerosen).

Absent diseases: the violence of the diseases which characterize the two extreme regions is here less; that is, on the one hand, pneumonia, erysipelas, catarrh; on the other malaria fever, dysentery, hepatitis, ophthalmic, impetiginous affections, gangrene, indolent ulcers.

3. *The lower, or hot region*, mean temperature from 72°·5 to 81°·5 F., presents exactly the class of tropical diseases.

Physiological predisposition: the opposite of plethora, blood poor in fibrin and diminished in quantity; the character of the phenomena is permanently torpid or adynamic; the predominant tendency is to the digestive organs, especially to the liver and intestinal canal, also to the spinal marrow and skin.

Frequent diseases are: all the three terrestrio-miasmatic affections in an intense degree; hepatitis, dysentery, encephalitis, tetanus, lepra, pachydermia, gangrænescentia, ophthalmias, impetigines.

Absent forms: typhus, plague, cretinism; gout and kidney affections are rare.

III. Having disposed of the above diseases dependent on temperature, we pass to the peculiar orographic morbid forms, which are determined by the meteoric relations of mountains rather than temperature. Undoubtedly the next most powerful etiological principle is the rarefied condition of the air, then in some measure the state of moisture of the atmosphere, and in connexion with both the increasing force of evaporation.

The physiological influence of these causes affects principally the respiration, the circulation, and the evaporation from the skin and lungs.

The proper orographic morbid states endemic on all mountains enumerated by the author, are—1. Mountain asthma, the best means of relieving which, after denser air itself, are, rest, the maintenance of warmth; in plethoric patients the abstraction of blood, and coca leaves (*erythroxylum coca*), with their almost miraculous property of stimulating and strengthening. 2. Dryness and chapping of the skin. 3. Ophthalmia; the dazzling appearance of the snow, increased by the intensity of the light, occasionally produces a sudden attack of conjunctivitis, combined with intolerance of light. 4. Hæmorrhages do not occur with frequency until we reach an elevation of between 17,000 and 20,000 feet. 5. The "Tabardillo" of the Andes possibly comprises encephalitis, pneumonia, and typhus. 6. Goitre and cretinism.

At a moderate elevation certain physiological changes take place which are worth mentioning, such as a feeling of lightness and strength, cheerfulness, increased innervation; the digestion is strengthened, sleep is improved, the circulation is excited, the skin becomes more ruddy, slight headache is produced. There is also a diminished susceptibility of the intoxicating effect of spirituous drinks, which is probably connected with the increased cutaneous perspiration.

The diseases which diminish or wholly disappear, and are therefore endemically absent in elevated situations, are next enumerated. Traumatic inflammation runs a lighter course; wounds heal better. This is probably owing to the combined influence of the rarefied state and of the dryness of the air; or in other words, to the increased force of evaporation. Indolent ulcers are only exceptionally heard of; dysenteryi s less formidable. The numerous chronic cutaneous affections of hot low countries, from lepra to the impetigines, disappear in elevated regions.

2. But the most important, and possibly the most practically useful, influence of the climate of the higher regions, and which is to be ascribed specially to the rarefied condition of the air, is the decrease and complete absence of tubercles of the lung, pulmo-

nary phthisis, the diminution of this disease being plainly perceptible at an elevation of 4500 feet.

3. Obesity is rare, probably owing to the strong evaporating power of the atmosphere. 4. Probably from the same cause the renal function is less exercised, and perhaps diabetis and albuminuria will more rarely occur. 5. The psychical influence of mountains on their inhabitants, and still more on visitors, which is in general of an exhilarating character, is not to be left out of view. 6. The diseases which are avoided, especially in the torrid zone, by ascending the adjoining mountains, are numerous: malaria fever, yellow fever, and in a great measure Indian cholera; dysentery, especially in the chronic form, is relieved; dyspepsia, hepatitis; many other diseases are warded off, or cured, and among them, as has already been observed, phthisis. On the contrary, affections which are endemic in the mountains are equally benefited by a descent to the lower regions, examples of which are found in bronchitis, catarrh, erysipelas, gout, typhus.

The author briefly describes the data from which the foregoing deductions have been made. They consist of reports from Cerro de Pasco, a mining town in the Andes, containing at an elevation of 13,600 feet, a population varying from 6000 to 12,000; and from other localities at 14,000, 11,200, 11,000 feet, &c., above the level of the sea. The author states that his is the first attempt to frame a general climatology of mountains, Dr. Lombard's work, 'Des Climats de Montagne,' &c., having been confined almost entirely to the local circumstances of Switzerland; and he enumerates Mexico, Puebla, Santa Fè de Bogotá, Quito, Arequipa, Chuguisaca, Potosi, La Paz, Cerro de Pasco, Cuzco; and in the East Indies, Outacamund, Simla, Landour, Dorjiling, Caboul, and Cashmere, and Erzerum in Armenia, as places where valuable observations might be made.

We come now to a very important subject; the absence of phthisis in some districts, and particularly in the rarefied air of elevated regions. The importance of discovering the climates where this disease, which indisputably, next to convulsions in the first weeks of life, occupies the largest place among the causes of mortality of the human race, will be at once apparent. Pulmonary tuberculosis is wanting in no zone, nor is it by any means absent in the torrid zone; in some localities of this region it is even particularly frequent. Consequently the geographical distribution of temperature has no influence on its occurrence, although, as is well known, where the lungs are already diseased, cold, and especially sudden change from warmth to cold, is very injurious; and on the contrary, removal to a higher temperature may act very beneficially: but we are now speaking only of the endemic occurrence of pulmonary consumption. On the other hand, there are—1, certain regions in all zones where phthisis is of rare occurrence, or is almost entirely absent; 2, there is, moreover, the very great probability, we may even say the certainty, of its frequency decreasing as the perpendicular height increases, until it completely disappears.

1. Districts in the several zones which exhibit an exemption from phthisis. Phthisis is said to be very rare in Iceland; in the Feroe Islands it is reported as being rare. We are informed that on the borders of Canada, at Fort Kent, Maine, where the mean temperature is 35°·8 F., and where quicksilver freezes in winter, phthisis scarcely ever occurs.

In the temperate zone there are three districts remarkable for special immunity from phthisis—namely, Algiers, Egypt, and Syria, and the steppes of the Kirghiz, near Orenburg. We may add that in Marstrand, an island within six or seven miles of the coast of Sweden, consisting of a single rock of granite, mixed with gneiss, pulmonary consumption is scarcely known among the natives of the island, which, from some peculiarities in its climate, has received the name of the Madeira of Sweden; and the disease appears to be remarkably arrested in such phthisical patients as sojourn there during the summer months, viz., from the beginning of July to the end of September.*

With respect to the torrid zone in general, it is first of all to be observed that phthisis does not occur less frequently in it than in colder regions. In Senegal, Angola, and Benguela it is rare. The East Indies, with all their insalubrity from hepatitis, dysentery, malaria fever, and cholera, enjoy a remarkable immunity from phthisis.

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* See Dr. Magnus Huss, Om Sverges Endemiska Sjukdomar. Stockholm, 1852, p. 51.

In the fourth zone, the southern temperate zone, including the important British settlements in Australia, Van Diemen's Land, and New Zealand, which is in general so distinguished for its salubrity, phthisis is one of the five most usual affections, the others being dysentery, pneumonia, catarrh, and ophthalmia.

It is still unknown on what climatic conditions the endemic absence of pulmonary tubercle in the above districts depends. Neither the temperature, nor the hygrometric state of the atmosphere, nor the geological conditions of the soil, afford an explanation of the fact. It does not depend on dryness of the air combined with heat; for though the air in Egypt is very dry, in the East Indies it is very damp and moist, and in both localities phthisis is absent, or at least extremely rare. On the other hand, Nubia, Chili, and Lima also possess a very dry atmosphere, and in these countries phthisis is particularly prevalent.

2. Phthisis diminishes decidedly in elevated situations, in consequence of the great rarefaction of the air. The only exceptions to this general rule have been supplied from Abyssinia. Notwithstanding these exceptions, the mass of evidence brought forward seems sufficient to establish that *the occurrence of phthisis diminishes with the atmospheric pressure in vertical elevations.* We shall scarcely err in attributing this to the greatly increased rarefaction of the air. It does not appear that the formation of tubercle in the system in general diminishes and ceases on lofty mountains; but it does appear that on account of the rarefied state of the atmosphere with diminished pressure, and an absolutely diminished amount of oxygen, the formation of tubercle is impeded in the lungs, a situation in which it is otherwise particularly liable to occur. This may depend either on the diminished quantity of oxygen, which at a height of 12,000 feet is less by more than one-fourth, or it may be caused merely mechanically by the extension of the act of inspiration thus rendered necessary, with greater dilatation of the thorax and bronchi. We agree with Dr. Mühry that the latter is the more probable cause of the exemption alluded to. We cannot see how a diminution of the supply of oxygen to the lungs should be unfavourable to the formation of tubercle. On the other hand, two ethiological facts are in favour of the latter view. First, the theory that the exemption is due to increased expansion of the lung, agrees perfectly with the fact that the summit of the lung is principally or almost solely the seat of tubercles, and with the probability that this is a result of the less degree of dilatation which precisely this part of the lung must experience in consequence of the conical shape of the chest. Secondly, the structure of the thorax in mountaineers who dwell at a great elevation is very remarkable, the chest being very broad, and the lungs highly developed.

In taking advantage, in a hygienic point of view, of the districts exempt from pulmonary tuberculosis, whether the areal or the mountain regions, we should remember that there is a great difference between sending a phthisical patient merely to a warmer and more uniform climate—which relieves only the diseased condition of the lungs, but where the form of disease is not itself endemically absent or rare—and causing him to reside in a climate where the combined etiological sources of his malady do not exist. If we acknowledge the good effects of the climates of Italy, the South of France, the South of Spain, Madeira, &c., on phthisical northerns, we must expect still more beneficial results from those of Egypt, Algiers, the East Indies, and still more, as we may now confidently hope, of the higher mountain regions. Dr. Mühry suggests that as rarefied air acts by producing a greater expansion of the lung, we may infer that in cases presenting a tendency to pulmonary tuberculosis the frequent and constant practice of deep inspirations would be a rational proceeding of easy adoption. But the first place must be awarded to the climatic treatment, and particularly to residence at sufficient heights on suitable mountains. Dr. Mühry supposes that as the proper and powerful effect of an elevated situation commences at 4500′ and may increase until we attain a height of 10,000′, which heights correspond to average barometric pressures of 23″ and 19″ respectively, we shall perhaps find 7000′ or 8000′ to be the most favorable elevation, corresponding to barometric pressures of 21 and 20 inches. Perhaps, however, a less degree of elevation may be found useful to those accustomed to lowland habitations. It is likely that at no distant period Sanatoria may be established in different situations to render mountain climates more available for sanatory purposes.

Suitable localities might be found on the eastern side of the Andes, in Mexico, with their beautiful terraces, valleys, and table-lands; their large, and in part gorgeous, cities, and their mild temperature, which affords an agreeable climate to an elevation of 8000′; or in the Sierra of the Andes of South America, on their east side, or in the midst of them—localities now rendered accessible by the steam navigation of the River Amazon.

The salubrity of a climate depends in general and principally on—1, a moderate and comparatively equable temperature of the air; 2, on a certain dry condition of the soil; 3, in a minor degree, on certain unknown endemi-physical predispositions (we do not now speak of social circumstances) to particular morbid conditions.

The salubrity of different climates, which is thus a product of several factors, is in many respects relative. There is scarcely a climate which is equally healthy to all men, or which is decidedly unfavourable to its indigenous population. In the first place, the salubrity of foreign lands has usually been estimated from the effects of the climate on European visitors. For obvious reasons, the inhabitants of the temperate zone are those most capable of ubiquity, and the frigid zone is comparatively more tolerable to them than the torrid. In the East Indies, the English rulers have never maintained their ground for three generations; still less have the Dutch done so in Java. On the west coast of Africa, the second generation has never survived. In the somewhat more temperate climate of the West Indies, a Creole population has been able to exist. The greater part of the population derived from the South of Europe, and now naturalized in America within the torrid zone, inhabits, as is well known, the cool mountain districts. The children of Europeans in the East Indies begin to fall away after the fourth year, and the Creoles in the West Indies have in general lost strength and liveliness. Still, in the East Indies, a mixed posterity of the Portuguese is found in later generations; Jews and Armenians also give here, as well as everywhere, proofs of general acclimatization. A remarkable example of acclimatization is afforded by the extension of the Indian people in tropical America, from the torrid base of the Andes to their icy heights—a difference of elevation of 13,000 feet.

In connexion with this question it is to be observed that there are diseases for which the receptivity of strangers is diminished through a prolonged stay in the country, while there are others the receptivity of strangers for which never diminishes. To the former class belong, in the first place, yellow fever, and also disorders depending on plethora and dyspepsia. To the second class belongs the influence of malaria, in respect to which there is no acclimatization; on the contrary, proclivity constantly increases. In addition, there is a third class of forms of disease for which decided receptivity of strangers does not commence until after a continued residence in the land; these are, certain chronic affections of a cachectic nature—for example, lepra, pachydermia, elephantiasis, frambœsia (?), impetigines, tendency to gangrene, &c. Lastly, there is a fourth class, consisting of diseases which first appear in the second generation in children; for example, atrophy, arising after the period of dentition. In general, acclimatization for hot countries is to be defined as consisting, in the first place, in an accommodation of the quantity of blood to the high temperature, as a reduction of the relative plethora with which the northern arrived, and probably of the quantity of fibrin. Two years' residence is, on an average, considered to be necessary to this end. The signs of the change are to be found in the disappearance of the high complexion of the skin, and the substitution for it of a paler and yellower colour. In the West Indies, a diminished receptivity to yellow fever commences with this change. There is no proper so-called acclimatization fever, though every first feverish attack which affects a newcomer has been indefinitely so styled; it is most commonly a malaria fever. When inhabitants of the tropics have lived for some time in colder zones, or in the cool mountain regions of their own country, they likewise acquire a corresponding acclimatization, their blood increasing in quantity and in fibrin; and after their return home they present a more inflammatory tendency, and diminished immunity from the miasm of yellow fever. Europeans inhabiting the tropics experience after a residence in cold countries a more rapid disacclimatization in reference to the torrid zone.

With reference to relative salubrity, it is to be observed, secondly, that a climate may be only individually healthy or unhealthy. Thus Italy possesses an individual salubrity for patients from the north affected with pulmonary or rheumatic diseases; but the tables of disease and mortality show that, while it has a more beautiful, it has by no means a more healthy climate than northern Europe. Canada, on the contrary, has an unhealthy climate with respect to the diseases just mentioned, but its climate is in itself more healthy than that of Italy.

That a difference exists between the races of men with respect to susceptibility of certain forms of disease, cannot be denied. Negroes possess a very extensive immunity from the malaria intoxication, such as the Indians in Asia and America do not possess; this immunity will in general be found to hold good also in reference to insolation and cerebral affections. On the other hand, they are peculiarly susceptible to cold, and to the contagions of small-pox and of frambœsia; the spinal marrow, too, is in them particularly excitable, tetanus is of frequent occurrence, after exposure to cold or after wounds; delirium, however, even in fever, is rare. If they change to colder climates or to more elevated regions, the affections of the respiratory organs predominate as the causes of their mortality—pneumonia, but still more, pulmonary tuberculosis; also typhus, with which at home they are absolutely unacquainted. But it belongs to the class of inexplicable anomalies, that they do not last in some climates situated in the tropical zone—for example, in the Mauritius, and still less in Ceylon; phthisis is the proximate cause of their mortality in these localities. On the other hand, they constitute an increasing and numerous population in the southern States of North America, where, nevertheless, severe winters occur; and they prosper remarkably in the temperate zone of the southern hemisphere, in the states of La Plata. Of the Indians in America we find it remarked, that in New Granada, where goîtres occur in the highest degree combined with cretinism, they are quite free from these affections. This exemption may also be regarded as a peculiarity of race.

Thirdly. We must recognise a more general or partial salubrity or insalubrity. A particular form of disease may prevail to such an extent that it alone makes a particular locality to be avoided; or *vice versâ*, the absence of a particular formidable disease may invest a district with great advantage. Examples of this we have in malaria fever, yellow fever, the plague, goître, malignant carbuncle, Guinea worm, diseases caused by cold (pneumonia, croup, catarrh); or some perfectly endemic forms of disease, as the Aleppo abscess, the Yemen ulcer, Beriberi, plica, &c. Particular examples of such partial salubrity are also afforded by some superficial districts in reference to phthisis, and by others with respect to carcinoma.

If we review the districts in the tropical zone most remarkable for insalubrity, we shall find that, next to the high temperature, moisture is the most important element in producing an unhealthy state. It is a low-lying clayey soil, rich in mould, most frequently alluvial, at the mouths of rivers, or along the coasts or the banks of rivers, and in marshes, which is most pernicious, and which acts almost exclusively by producing malaria. In fact, where the soil is dry, as on sand and coral formations, or in districts free from rain or rivers, the high temperature of the air is much less injurious, so far as the production of malaria is concerned; while other miasmatic diseases, as, for example, yellow fever and cholera, are but slightly connected with the conditions of the soil, and dysentery and hepatitis not at all so. By attending to these points the insalubrity of many districts has been in a great measure overcome. Batavia has for several decennial periods ceased to deserve the epithet of "the grave of the Europeans;" the part of the city built on alluvial soil has been almost deserted as a residence by the Dutch, who now keep only their stores there, but live on higher ground. In Sierra Leone, on the east side of which lies a large undrainable marsh, an improvement of the great insalubrity has been obtained in another mode, namely, by the introduction of better water for drinking, in iron tubes from the heights. Arica, on the coast of Peru, situated in that rainless dry desert, used formerly to become, after the overflow of a small river, a dangerous abode, but has now been rendered healthy by preventing that occurrence. Acapulco, on the west coast of Mexico, has been rendered more healthy since, by cutting through a mountain, a freer draught of air has been obtained, and the drying of the soil has thereby been promoted. In Surinam, and in numberless other places, health has improved simultaneously with the culture of the soil, as well as after the removal of woods and the drainage of the land.

If we look in Europe for some standards of comparison, we find the mortality in London, 1:40 to 44; at Edinburgh, 1:35; at Geneva, 1:43; at Hamburgh, 1:30; at Berlin, 1:38; at Breslau, 1:26; at Vienna, 1:24. The great difference exhibited by these numbers is probably in a great measure due to the unfavourable effects of the extreme cold of the more eastern countries during the first year of life. In youth, the deaths from phthisis constitute one-seventh of the entire mortality, in many cities their proportion amounts even to one-fifth. In manhood, pneumonia and bronchitis, in old age, apoplexy and paralysis, are the most frequent causes of death. Some well-known large cities afford examples of the importance of different conditions of the soil. Amsterdam lies on a damp clayey soil, and suffers from malaria; Berlin is situated on sand, and is indebted chiefly to it for its salubrity; Vienna and Paris are built on dusty chalk, which is probably one reason of the frequency of pulmonary tubercles in those cities. Munich, again, is situated on an elevated plain exposed to the north wind, and therefore suffers much from pneumonia; St. Petersburgh has in its neighbourhood a low damp soil, and though malaria has here almost attained its geographical limit and is nearly absent, and the mortality is usually given incorrectly and as exaggeratedly unfavourable, still an extraordinary fatality exists in the first year of life.

If we look for particularly healthy districts, small maritime islands in general carry off the palm—for example, the sandy islands in Northern Europe; also the more southern islands—the Azores, Madeira, Rhodes, &c.: for they combine the moderate and uniform sea climate with a dry soil, and are in a better position to prevent the entrance of importable diseases.

The temperate zone of the southern hemisphere is, however, incomparably before all others in point of salubrity. This is proved unexceptionally in Chili, Patagonia, Buenos Ayres, the southern provinces of Brazil, the Cape of Good Hope, Port Natal, South Australia, Van Diemen's Land, and New Zealand. Here there is, on the one hand, absence of the principal zymotic diseases which have not as yet been imported or become endemic; on the other, we have the admitted general conditions of a sanitary state. The soil is free from malaria south of the isothermal line of 68° Fahrenheit; the temperature is moderate, but as it is liable to considerable oscillation, the greatest mortality is derived from this source, and is attributable to diseases of the respiratory organs—pneumonia, phthisis, &c.: in addition, catarrh and ophthalmia constitute the major part of the morbid constitution. Of this zone it is even asserted that the European settlers improve as a race in their posterity.

The subject at present under our consideration resolves itself into three practical questions: 1, How the insalubrity of climates is to be avoided; 2, How it is to be diminished; 3, How the salubrity of climates, especially of such as are singularly healthy, is to be used (climatic therapeutics).

The chemical constitution of the atmosphere is so generally known that we need not enter into it here. Dr. Mühry divides its non-essential admixtures into two classes, the chemical and the inherent. The first are again subdivided into those which are and those which are not constantly present. Of the former, it is observed that carbonic acid, being capable of absorption by water, will in general be found in less proportion over a damp than over a dry soil, while over the sea scarcely a trace of it will be discoverable. Of the latter, traces of nitric acid are found in rain falling after thunderstorms, probably formed by the electrical discharges from azote and oxygen. Ozone, the separate existence of which is still in some measure problematical, belongs to this class. Other gases are found in particular localities, but are too limited in extent to be of climateric importance. The inherent non-essential admixtures of the atmosphere are small microscopic particles suspended in the air, consisting of the most minute detritus of substances partly mineral, partly vegetable and animal, partly also of the most minute organisms (fungi and vibriones). Our knowledge of such of this class as are ordinary constituents of the atmosphere is as yet too imperfect to enable us to distinguish those which are extraordinary, or to decide whether they are of etiological importance. It is certain that the majority are indifferent to the human organism. While the expression "development of miasms" is so generally used, as representing the undefined causes of definite forms of disease of a sporadic or epidemic nature, it is frequently not borne in mind, that such a causal connexion has not been demonstrated. As a nosogenetic or peculiar atmospheric miasm, we can properly assume but one, that is, the miasm of influenza; three others we assume, developed in the soil, viz., malaria, the miasms of yellow fever, and of Indian cholera.*

In reference to geographical distribution, we find the atmosphere in its essential constituents homogeneously composed throughout the whole world; but in accidental, gaseous, or inherent admixtures we find some peculiarities over the sea, over the continent, and over particular limited districts of the latter. These peculiarities are connected with the soil, and are partly products of human cultivation, but they are too locally limited to affect the atmosphere to any extent, or too indifferent in themselves to communicate a special quality to a climate. In a geographical point of view there are no zones or areal districts where the proportions of the atmosphere do not possess the general homogeneousness. Hence it follows that the significance of the often-used expression "pure or impure air," is at best but strictly local, and probably referrible chiefly to the peculiar exhalations of crowded human beings, and always more to the lower strata of the atmosphere, especially when in a state of stagnant rest.

In addition to the regular decrease of gravitation from the poles to the equator, irregular local differences have been shown by observations with the pendulum to exist in different localities. These facts prove that in some places either inequalities in the form of the surface, or varying thickness of the internal mass must occur. The intensity of gravitation increases in the neighbourhood of volcanic regions, and in other places, from causes which are not sufficiently understood. Deviations of the pendulum from this cause are observed at the island of St. Helena, Ascension Isle, St. Thomas, and the Isle of France. The intensity of gravitation is very weak at Bordeaux, it increases suddenly at Clermont Ferrand, at Mailand, at Padua, where it attains its maximum, thence extending in the same condition to Parma. It does not appear that these differences, the extreme of which is one-half per cent., have any influence on the human organism.

Small islands, where the population is limited, and consequently does not contain so many receptive individuals as to keep up contagion by regeneration, enable us to arrive at conclusions as to the original occurrence of diseases, or the importation of contagion or miasms. On observations made upon them Dr. Mühry bases the following classification:—

I. Diseases which do not occur originally or spontaneously, but only after importation, occasionally spreading epidemically, are—

1. The contagious: small-pox, scarlatina, measles, hooping-cough, typhus, plague, pustula maligna; these are regenerated only in the organism.

2. The terrestrial miasmatic: the West Indian yellow fever (hæmogastric fever), East Indian cholera (serogastric fever), they are imported, very probably, germinating in the wood of ships; the third terrestrial miasm, malaria, is not imported.

II. Diseases which very probably are capable of spontaneously generating contagion in the system, are—

Dysentery, ophthalmia (both contagions of the mucous membrane), erysipelas, puerperal metritis, hospital gangrene.

III. Diseases which actually originate in the air: of these there is only one, influenza; it depends on a really atmospheric miasm, which, unknown in its essence, at times arises in the air, and is not imported.

As to the meteorology of small islands, the temperature is more uniform and moister; the proximity of a great continent must certainly, when the wind is on that side, make a difference and bring dryness, heat or cold. The regularity of the sea and land winds, the former by day, the latter by night, is also no small advantage. What has been said of the insular, will also be true of the sea-coast or littoral climate. The following may be enumerated as littoral and insular diseases: yellow fever, which affords but a few, and these only apparent examples of its occurrence at a distance from the coast; the Yemen ulcer, found only on both sides of the Red Sea, and never in the interior of the adjoining countries; the beriberi, in the East Indies, a rheumatic œdematous paralysis, apparently met with only in a certain proximity to the sea; the spedalskhed, or lepra septentrionalis, occurring in Norway, Iceland, Greenland, Kamtschatka, but not in the interior of continents.

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* Investigations of the atmosphere in reference to such etiological particles have been instituted by Ehrenberg, A. Vogel, R. D. Thomson (Appendix to the Report on the Cholera, 1855); (Gazette Médicale de Paris, Oct. 1855), on the occasion of epidemics of cholera.

Phthisis pulmonalis appears to be rather frequent in islands and in coast districts; a fact probably connected with the great barometric pressure at the sea level, in opposition to the much slighter pressure in elevated regions on the mountains.

Sea-ports have many disadvantages; the temperature is more moderate, the air is damper, the barometric pressure greater, the soil is more frequently alluvial, and moreover, the opportunities for the importation of diseases are abundant; all are circumstances favourable to the occurrence of malaria, yellow fever, cholera, phthisis, indolent ulcers, chronic affections of the skin, &c.

Having treated at considerable length of the four climatic regions of Germany, into which part of his subject we shall not follow him, Dr. Mühry divides diseases into three classes, with reference to their epidemic movement; viz., the persistent, the regularly fluctuating (according to the seasons), and the irregularly fluctuating or wandering. To the first class, which in the temperate zone experience no change with the temperature, but occur equally at all seasons, belong the following:—zymotic diseases: small-pox, scarlatina, measles, (hooping-cough, typhus, erysipelas, puerperal metritis), mumps, hospital gangrene, contagious ophthalmia. Those in the parenthesis are slightly influenced by the seasons, typhus being augmented in autumn, the rest in winter. Dyscrasias: tuberculosis, scrofula, cholera, dropsy, furunculosis, anthrax, tendency to gangrene, urolithiasis, helminthiasis, herpes, hemorrhages, hemorrhoids. Local determinations: apoplexy, trismus, tetanus, epilepsy.

The diseases regularly fluctuating according to the seasons.

a. The summer constitution is thus characterized: physiological disposition. The amount of blood in the system, formed in winter, is too great for the exalted temperature, producing relative plethora, which gradually lessens as the organism accommodates itself to the heat of summer. This may perhaps explain the fact, that persons who habitually suffer from vertigo, are more seriously affected with this symptom in the beginning of summer than at any other season of the year. The character of the reaction of the system against hurtful impressions retrogrades from the inflammatory, and approaches to the torpid or adynamic. The tendency to localization is predominantly directed to the digestive apparatus, especially to the liver and intestinal canal, as well as to the skin, while the lungs and kidneys become freer.

The more frequent of the summer forms of disease are: *zymotic*—malaria fever, yellow fever, Indian cholera, plague (although it ceases directly at a temperature over 80° F.), dysentery, aphthæ, pustula maligna, indigenous cholera, diarrhœa; *dyscrasias*—chronic affections of the skin increase, leproid affections appear (on the shores of the Mediterranean), herpes increases. Pneumonia is rarer; phthisical, arthritic and rheumatic patients are relieved, but plethoric patients and those labouring under cardiac disease suffer more, at least at first.

b. The winter constitution is generally characterized as follows: physiological disposition. The quantity of blood now again increases, and the blood probably gains in fibrin. The character of the reaction of the system becomes increasingly inflammatory. The tendency of localization is chiefly to the respiratory organs, and probably to the kidneys; the digestive apparatus, especially the liver and intestinal canal, become freer. The three terrestrial miasms disappear, yellow fever at a temperature under 70°, malaria during the whole winter season, Indian cholera in frost.

To the irregularly fluctuating or wandering forms belong the proper epidemic diseases, which are almost all contagious. Of these some are promoted by heat, as malaria fever, yellow fever and Indian cholera (the three miasmatic diseases), dysentery, pustula maligna, aphthæ, plague (to a certain degree of temperature); some by cold, as erysipelas, puerperal metritis, pertussis, croup, from a certain degree typhus, and the atmospheric miasmatic influenza; while others are wholly independent of temperature and seasons, and are all more contagious, especially the most usual, best known, and most dreaded—small-pox, scarlatina, measles, mumps, hospital gangrene, hooping-cough, and typhus.

We shall conclude this article with a few brief remarks on the geographical limits of some diseases.

It is a remarkable fact that, while in the northern hemisphere the polar boundary of malaria, or intermittent fever, corresponds to the isothermal line from 38° 75′ F. to 41° F., in the southern the countries south of the isotherm 68° F., are free from this disease.

The plague is limited both by heat and cold, and also geographically. This contagious disease possesses, moreover, a certain singular *endemicity*, the centre of which is found in Egypt, while its southern boundary does not exceed the isothermal line of 77° F. Towards the east the plague, with few exceptions, does not pass a meridian line drawn from the Persian Gulf to the Caspian Sea, and on the west it has never reached America. Dr. Mühry adduces several reasons for believing that towards the west also there exists an "endemic meridian boundary of the plague." The plague is extinguished in the frost of an European winter.

Gout is almost entirely absent in the torrid zone, and even European arthritic patients residing there soon lose their symptoms.

It might be supposed that in consequence of the increased action of the skin, and of the vicarious or alternating relation between the two organs, diseases of the kidneys should be comparatively rare in hot countries. The majority of the reports quoted by Dr. Mühry appear to favour this view.

Obesity is rare in very dry countries, as in the elevated regions of the Andes, Abyssinia, and Thibet, in South-eastern Arabia, the interior of North America, and at Sydney, in South-eastern Australia; on the contrary, it is frequently met with in highly saturated climates. Thus, in Madeira, Mittermayer found it in boys and girls.

Wounds have been said to heal with difficulty in some tropical countries, as Brazil and the West Indies, while the contrary has been stated of other regions, as Guiana, presenting similar physical characters.

Carcinoma is reported to be rare in Guiana, Sierra Leone, Egypt, Syria, New Zealand, and the East Indies.

The whole subject of climatology and medical geography, of vast importance as it undoubtedly is, is still only in its infancy, yet it is one which must receive an immense impulse from the means of rapid intercommunication which science has lately placed at our command, and which the progress of the age is from day to day more fully developing. Meanwhile, we gladly hail the appearance of such important contributions as Dr. Muhry's work, and of so useful and carefully prepared a compendium as Dr. Pickford's "Hygiene," on which we have yet to say a few words.

Dr. Pickford's work is by no means confined to the subject of climate; it contains, in a comparatively small space, and in an accessible and readable form, an immense amount of information on the principal topics connected with the important and, just now, much-ventilated questions of "Sanitary Reform" and the "Public Health." The part already published "embraces the physics of the atmosphere, the seasons, temperature, rain, winds, and pressure; the respiration of plants and animals, the circulation of the blood, the chemistry of respiration, and animal heat; infection, contagion, malaria, sewerage, drainage, ventilation, and climate in connexion with disease." The interesting observations of Dr. Roscoe are noticed, which show "that the beneficial action of the *brick and mortar walls* of our dwellings is not confined to the mere absorbing from, or restoring moisture to, the atmosphere, but that it extends to a very large diffusive interchange between the carbonic acid gas of the apartments and the external atmosphere; that, in fact, *brick walls* are powerful aids to ventilation. Dr. Roscoe ascertained that in a closed space, the air of which contained 16 per cent. of carbonic acid gas, 3.25 per cent. escaped in two hours through the solid brick." (p. 219).

On the other hand, "the unhealthiness of iron, or new and damp houses, is probably partly accounted for by the absence of all diffusive interchange through iron and through wet walls." (p. 220.)

Dr. Pickford naturally discusses the questions of "sewerage" and the "Thames;"

but vitally important as these topics are, they do not strictly belong to the main subject of this article. Much that he states in his excellent and comprehensive chapters on "climate" and on "climate and disease," we have already drawn from the work of Dr. Mühry.

Review VI.

1. *Anatomy, Descriptive and Surgical.* By Henry Gray, Lecturer on Anatomy at St. George's Hospital. The Drawings by H. V. Carter, M.D., late Demonstrator of Anatomy at St. George's Hospital. The Dissections jointly by the Author and Dr. Carter.—*London*, 1858. pp. 750.
2. *Handbuch der Systemischen Anatomie des Menschen.* Von Dr. J. Henle, Professor der Anatomie in Göttingen. In drei Bänden. Mit zahlreichen in den Text eingedruckten Holzschnitten, Braunschweig.—1855.
   Handbook of Systematic Human Anatomy. By Dr. Henle.
3. *A Treatise on the Human Skeleton (including the Joints).* By George Murray Humphrey, Esq., M.B. Cantab., F.R.C.S., Surgeon to Addenbrooke's Hospital, Lecturer on Surgery and Anatomy in the Cambridge University Medical School, Cambridge. pp. 620.
4. *The Master-Builder's Plan; or the Principles of Organic Architecture, as indicated in the typical Forms of Animals.* By George Ogilvie, M.D., Lecturer on the Institutes of Medicine, &c., in the Marischal College and University, Aberdeen.—*London*, 1858. pp. 192.

The work of Mr. Gray is likely to become very popular in the schools of anatomy, for it is carefully written, correct in detail, and both well and copiously illustrated. But we do not think that the author has quite done himself justice, nor realized by this production the position of which his earlier works gave such high promise. Perhaps, however, he has contented himself with writing up to the mark of requirements at the present examining boards, an union of examiners and teachers constituting an anomaly scarce known out of the medical profession.

First as to the illustrations. They are of larger size than usual, and decidedly well executed, some as faithful representatives, others as diagrams. But we often recognise "old friends" throughout the volume—the Osteology being after Holden, the Venous System after Breschet, the Lymphatics after Mascagni. And although we do not deny the legal right, nor absolutely ignore the practice of any one thus to appropriate literary property, still we could have wished that one so justly respected as Mr. Gray had not so frequently followed the precedent. Look at the occipital bone in Mr. Holden's work (p. 56) and the same cranial element in Mr. Gray's work (pp. 19 and 21); the bones of the hand (Holden, pp. 154 and following); with Mr. Gray's delineations (pp. 104 and 106). It is but just to say that the references are acknowledged in the preface.

The drawings of the skin (p. 542), of the tongue (p. 548), or the nose (p. 553), &c., present no novelties; but we anticipate the reply, that a truly accurate anatomical drawing cannot vary, though repeated from the subject a hundred times. Let us, however, turn to what we may praise. The drawings of the bones, although after Holden, are doubtless well done, and perhaps on an improved plan; the outlines are clear, and the margins of articulation carefully indicated, as well as the attachment of muscles. The process of development is greatly elucidated by such diagrams as in pages 70, 71, where one may also see at a glance how arrest of development may be followed by fissured sternum (drawing 61). The plan of development of the foot (p. 131) is particularly clearly executed, and may be studied with advantage.

If we turn to the chapter on the muscles, we see that the same care has been exhibited throughout. For instance, the quadrilateral and triangular spaces formed by the humerus, the long head of the triceps, and the teres major and minor muscles (p. 279) can be comprehended by a glance at figure 154; and there the student may see how the long head of the triceps separates the two latter muscles. The passage of the flexor tendons of the palm of the hand with the ligaments binding them in their place is shown in a very good drawing at page 265. Indeed, we may go on multiplying these examples to the end of the work; and we conclude with saying that, although some are diagrams, as contrasted with nature, all are instructive and tell their own story, and by far the greater portion are equally creditable to the anatomist as to the artist.

And now for the text. Upon considering the object which Mr. Gray has had in view, namely, that of producing a work which should assist the student of anatomy in his attempts to learn the proper kind and amount of information required by the usual examining boards, we must say that he has succeeded. Such material, delivered in the form of lectures, would naturally command the attention of the class.

What right have we to complain that, amidst so much that is good, space is devoted to the fact of announcing that bones are long, short, and flat, irregular or mixed, that they are united by sutura vera, sutura notha, sutura dentata, serrata, limbosa, squamosa; by harmonia, schindylesis, gomphosis, arthrodia, enarthrosis, ginglymus; diarthrosis rotatorius. Such statements have not yet received an open condemnation as "rubbish," and therefore Mr. Gray has done quite correctly in introducing them in their proper place; but with a quiet sarcasm he couples explanations to these expressions of elucidation. He tells us, for instance (p. 137), that diarthrosis rotatorius means "articulation by a pivot process turning within a ring, or ring around a pivot; as in the superior radio-ulnar articulation, and atlo-axoid joint," and so forth.

The practice of introducing surgical remarks into such a work as the present, is of very questionable advantage, if we consider the matter in a purely scientific light. But then for students there is something attractive in having the mind relieved from the weariness of anatomical detail, by the presentation of some well-known practical point upon which may chance to bear the very information that they have been laboriously acquiring in the dissecting-room. The drawing, explanations, and references are doubtless from the experience of St. George's Hospital, or from the pathological collection in the museum, and must therefore bear the proper stamp of value.

That this work will be in large demand we entertain no doubt, and we sincerely trust that Mr. Gray will be repaid for the time and labour which he has devoted to the subject. Of the mode in which the work is "got up" it is impossible to speak in too high terms.

And now we are going to quarrel with Mr. Gray—not as an individual—for he has long been highly esteemed by his professional brethren as a man of industry, originality, and science. But, as a teacher of anatomy of no inconsiderable weight and experience, we ask him wherefore he has so completely ignored the revelations of homology and comparison in his present work? Should he not maintain his place among the pioneers of science? Should not the student be taught in such a work, so laboriously and expensively prepared, something higher than would be fitted for a dissecting-room manual, however accurate in detail the latter might be?

In the description of the lumbar vertebræ (p. 10), Mr. Gray tells us (and we believe for the first time in such a work) that a tubercle projecting backwards from each of the superior articulating processes is the representative of the transverse processes in the dorsal and cervical region; but why not add that the lumbar transverse processes, of common anatomical works, are the representatives of the ribs—that they should, in fact, be named the lumbar ribs, and are short as osseous structures, because the abdominal walls, unlike those of the chest, must be soft, muscular, and yielding? Had attention been paid to the homologies of the vertebræ, Mr. Gray might have gently simplified the account of the muscles of the back, and thus got rid of one opprobrium to anatomy, for it is quite the fashion to ignore that part of the human body in modern schools. Again, irregularities in the origin and distribution of arteries are merely recorded, accurately it is true, but without any attempt to systematize these deviations from the normal state, and yet there is "a method" in abnormities. The chapters on the surgical anatomy of hernia, and on the perineum, are well worth perusal, and explain some of the more important regions of the body in their relative co-operations. We are sure that Mr. Gray will take our few objections kindly, because he must feel that he possesses both the talent and the information to render this work, of no slight value in its present form, one of the most complete treatises of the day, when a second edition is brought to light. But in order to do that he must give his talents freer play, and fearlessly attack all that part of the subject which rests upon insufficient or unsound foundations. Human anatomy must not be studied so exclusively, and future systems must partake of the lessons taught us by a Hunter, a Cuvier, or an Owen.

Henle* commences the work before us by enumerating a just and a high view of the human archetype. The human body, as that of the vertebrata, generally consists, as traced to its archetype, of two parallel cylindrical tubes, opposed to one another in their whole length. The one contains the central organs of the nervous system, wherein reside the functions of thought, of feeling, and of will; organs which raise the animal above the vegetable world. The other tube incloses organs proper to the functions of nutrition and procreation, which are proper to plants as well as animals. Hence one of these tubes is called that of animal, the other that of vegetable life. (p. 1.)

Taking this view of the vertebrate form, it follows that the ribs must be regarded as constituting part of a vertebra, more especially as it has been shown by the skeletons of many of the saurian and ophidian reptiles that the long arched dorsal ribs are represented in the neck by the apices of the transverse processes and the long fixed transverse process of the lumbar region. In fact, the hæmal arch is as much part of a vertebra as the upper or neural arch. This brings us to the question of what a true vertebra consists.

Mr. Humphrey, admitting Henle's view of the vertebral column, and the principles of Homology generally, proposes to improve upon and to simplify the system as introduced by Professor Owen. He regrets (p. 590) that so much new and difficult nomenclature has been introduced by the learned professor, and yet we find the terms supra and subcentral growths, neural arches, neural alæ, transverse alæ, hæmal parts, &c., by which we are left in no better condition. We think the following passage would be difficult of comprehension to a beginner:—

> "Their shape (i. e., the bodies or centra of the vertebræ) usually approximates to that of a sphere, but sometimes deviates from it very considerably. One of the modifications is caused by out-growths from the superior or inferior surface of the centrum. Those in the former situation, such as the posterior clinoid processes of the sphenoid bone, and crista galli of the æthmoid, may be called supra-central," &c. (p. 591.)

We may remark, *en passant*, that ethmoid should not be spelt with the diphthong, being derived from the Greek word ἠθμός, a sieve.

But to revert to the question of nomenclature. Mr. Humphrey retains the expression, "articulating process," in place of Professor Owen's "zygapophysis." Now, in the snake there are many articulating surfaces between each two vertebræ, but of these only one pair represent the zygapophyses of human anatomy. The old term, therefore, retains in familiarity what it loses in accuracy. Again, we cannot see the merit of neural alæ and transverse alæ, over neurapophyses and pleurapophyses. The word apophysis is not harder to remember than process; and a student might complain of having to distinguish between a transverse ala and a transverse process, which in the dorsal region would be distinct, in the lumbar the same.

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* We should have presented our readers with an analysis of Henle's work before this, had we not hoped to receive the concluding parts, the first parts only having reached us. We regret to say that our foreign friends frequently forget to forward the continuations of works appearing in serial numbers, a circumstance that must be our apology for not always paying them that early attention which they might otherwise merit.—Ed.

Mr. Humphrey gives us the drawing of an ideal vertebra, of which the simplicity consists in there being twenty-one component parts—namely, centrum 1; neural processes 2; neural alæ 2; neural spines 2; superior transverse processes 2; inferior transverse processes 2; superior transverse alæ 2; inferior transverse alæ 2; hæmal processes 2; hæmal alæ 2; hæmal spines 2. We wait to see the specimens which will be brought forward to illustrate this compound structure, reminding Mr. Humphrey that Professor Owen's typical vertebra of sixteen elementary pieces can be demonstrated in many animals, of which the one selected is usually the crocodile.

In thus speaking of homological anatomy, we beg to refer the reader to an unpretending, yet well-written and scientific little work by Dr. George Ogilvie, Lecturer on the Institutes of Medicine in Marischal College, Aberdeen. He need not have apologized in his preface for the popular form of the work, inasmuch as it is precisely the thing wanted, and we can readily foresee that the book will pass into very general use. In the appendix there is drawn up a table of the exceptions taken to Owen's system by Maclise and Goodsir (p. 173); and one of these seems especially to have influenced the conclusion of Mr. Humphrey, namely, "it is contended that, in limiting the pieces of a vertebra, as he has done, Professor Owen has given his system a stringency which does not exist in nature." But to this we reply that the essence of homological anatomy is "stringency." And unless it be true that all varieties of form can be referred to an archetype, and every element of bone assigned a fixed and proper place, the system must inevitably expire, as Dr. Ogilvie has expressed, although there is no single plan of construction applicable to all animals, yet a certain uniformity of organization is observable in each primary division.

We decline any remarks upon the vertebrate construction of the skull until we see a fuller account of the arguments upon which Mr. Humphrey's system is founded. But we hope to read a better style of anatomy than that published in his homology of the limbs. The homology between the patella and the olecranon will not be received in the present day, for it must of necessity establish a relation between the thumb and the little toe, or *vice versâ*. Let Mr. Humphrey study comparative anatomy for some years more, and use his powers of reflection, which are considerable, for a similar period, exclusively on the subject, and then, perhaps, he may present to us remarks on so grand a subject which will increase his fame. But we protest against the multiplication of baseless theories, each of which acquires a local reputation just as great as is proportioned to the accidental position of the author and the difficulties of the subject.

A peculiar feature in Henle's work is the representation of vertical segments of joints and limbs, through both soft and harder parts. These drawings are beautifully executed, and are somewhat out of the common. Take, for example, the drawings of the hip (p. 129), of the knee (p. 137); of the three cuneiform bones with their ligaments (p. 177); or of the articulations between the os calcis and astragalus (p. 160). Throughout the whole of this work there is an accuracy and an attention to detail almost wearisome to a reviewer, but nevertheless it is a point which renders the work invaluable as a book of reference, and which stamps it with a merit which will doubtless be appreciated by both lecturer and student. But even he falls into confusion in instituting a comparison between the bones of the upper and lower extremities. He contrasts the olecranon attached to the ulna, with the patella attached to the tibia, and consequently repeats the errors of Vicq-d'Azyr and of Cruveilhier, of whom the latter remarked, "The superior extremity of the tibia is represented by the superior half of the ulna, and the inferior half of the tibia by the inferior half of the radius, while the fibula is represented by the superior half of the radius and the inferior half of the ulna." ('Anatomie Descriptive,' t. i. p. 315.) Vicq-d'Azyr and Cuvier conceived that the anterior extremity was not parallel, or repeated by the posterior one of its own side, but by that of the opposite side.

Professor Owen has reminded us that the late Dr. Barclay, by his extensive knowledge of comparative anatomy, showed long ago how the ulna was the homotype of the fibula, and exhibited the same variety and unsteadiness of character, sometimes large, sometimes small; and sometimes merely a process of the more constant bone of their respective segments. In the leg-bones of the woombat (Phascolomys) this correcter view is maintained by the presence of a detached sesamoid bone upon the broad, high, and expanded process from the proximal extremity of the fibula.

As an illustration of the superior manner in which Henle has performed his task, we refer the reader to the description of the knee-joint (p. 132), of which there are eighteen beautifully executed drawings. No work with which we are acquainted shows so accurately or variedly the relation of bones, ligaments, tendons, muscles, bursæ, &c., both in the extended and the fixed position of the limb. He concludes the account in the following words:—

> "Complicated as is the knee-joint considered anatomically, so is its mechanism simple. The deductions which the brothers Weber have given upon the subject leave scarcely anything to add. As compared with the elbow-joint, the peculiarity of the knee-joint consists chiefly in this,—that the conditions to limit the movements to certain directions are supplied in the former by the configuration of the bones; in the latter, by peculiar ligaments. The whole ligamentous apparatus of the knee is directed to favour no other movement than flexion out of the extended position. Particularly the ligamenta cruciata, with the posterior part of the capsule, oppose any propagation of the movement of extension by which the tibia might form an obtuse angle with the femur. With flexion, on the other hand (a slipping and rolling movement of the condyles in the articular surface of the tibia), commences a general relaxation of the ligaments, which, while the crucial ligaments constantly hold the opposed articulating surfaces in apposition, permits a rotatory movement of the tibia in its long axis (Pronation and Supination according to Weber); a rotation during which the middle part of the joint is relatively at rest; the lateral parts are in motion (p. 149)."

But the internal condyle of the femur, in the rotatory movement of the leg when in the flexed position, acts somewhat as the axis, in which plays the articulating surface of the tibia; and this is the reason why the internal semilunar cartilage is fixed at both extremities as well as at its most prominent central part, while the external semilunar cartilage is rather circular in form, attached only by the converging extremities, and free in the rest of its circumference to follow the movements of the tibia.

In writing upon such a subject it is a good surgical point to remember that, after chronic inflammatory disease of the knee by which the lateral ligaments have become softened and elongated, the leg is almost invariably drawn backwards and rotated outwards, the internal articulating surfaces still keeping their normal relation.

A new feature in Henle's diagrams and illustrations is the colouring of the muscular tissue red, whereby the eye can at once distinguish it from bone or tendon, and this is particularly useful in such segmental representations as those of the foot. (pp. 158—160.)

These drawings, as the author observes, are without exception original, and the sections are taken after nature. In order to obtain them, the joints and their ligaments with the other soft parts in the required position were sawn through, some of them in a frozen, others in an artificially hardened state. The dried parts regained their fulness and yielding condition by maceration in water, although in large sections, such as the knee, a considerable time, *e. g.*, several weeks, was necessary.

The description of the articulation of the vertebral column is full of interest. Henle points out to the student how the direction of the articulating surfaces of the dorsal vertebræ favours the development of lateral curvature (skoliosis). The transition from the antero-posterior of the dorsal to the lateral directions of the lumbar articulating processes permits the springing movements of that trunk, and in the gradual transition of one to the other lies the cause of that rotatory displacement of the vertebræ which produces in spinal deformities such suffering to the patient and anxiety to the surgeon. There are many remarks of interest referring to the composition and structure of the intervetebral cartilages, which are as well illustrated as the other parts of the work. The fasciculus upon the muscles has just appeared. The illustrations are again coloured, and are executed with an accuracy which will excite just admiration.

The greater part of Mr. Humphrey's work exhibits considerable research. The first chapter contains a very fair exposition of the views entertained by Stark, Bibra, Hoppe, Quain, Sharpey, Owen, Rees, and others; followed by well-expressed observations of his own upon the form, processes, and peculiarities of bones, coupled with remarks upon some points of pathology. He rightly tells us that the term "duality" is not applicable to the skeleton. "We hear sometimes the expression 'duality' applied to the nervous system; and theories with regard to the duality of the mind have been based upon that construction of the brain in two halves which has suggested the phrase. With as much propriety might we speak of the duality of the skeleton, or of any body whose parts are disposed symmetrically about a centre." (p. 16.) We must content ourselves with mentioning the general arrangement of the work. The description of the periosteum, blood-vessels, of bone, nerves, and lymphatics, are such as would be appreciated by the learner. Mr. Humphrey will have it that there are 220 bones in the skeleton; and Mr. Gray is equally positive that there are but 206. Both are equally right. The numbering of the bones depends upon the age of the subject, and the fancy, homological or otherwise, of the author; but we protest against such statements in modern works. Of what possible good can they be when two authors, both in the possession of abundant resources, and with skeletons without end at command, cannot arrive at the same conclusion?

In the account of the vertebral column there is much useful information, though nothing of particular novelty. We pass on to the description of the skull, the account of the cranial development being after Rathke and Reichert. The process of ossification, particularly "the parts formed respectively from bone and cartilage, being after Kölliker." It would be foreign to the purpose of a review to quote descriptions of bones, which are here correctly though purposely diffusely given, and intermixed with remarks from foreign authors. Mr. Humphrey has consulted the works of Kerkringius, Autenrieth, Spix, Blandin, Otto, Hyrtl, Lobstein, Weber, Béclard, Meckel, Luschka, Virchow, Hildebrant, Jourdan, Tiedemann, &c. &c., and has accordingly gathered together a quantity of information, which he has arranged and made use of.

We have allusion to that hypothetical accident, which from time immemorial has been made "a point" in the anatomical lectures; namely, dislocation of the lower angle of the scapula over the latissimus dorsi.

> "The importance to the movements of the arm of such provision for the efficient action of these muscles is well illustrated by cases in which the latter are weak, or paralyzed, or inactive from some cause, or in which the angle of the scapula has slipped from beneath the edge of the latissimus dorsi. In such cases the contraction of the deltoid, instead of causing the head of the humerus to rotate in the glenoid cavity, causes the glenoid cavity to roll upon the humerus, and pulls the lower angle of the scapula backwards and upwards, so that it projects beside the spine. The patient may thus be rendered quite unable to raise the arm, each attempt to do so being followed by the revolution of the scapula instead of by the elevation of the elbow." (Humphrey, p. 365.)

Now, the lower angle of the scapula is perpetually rolling over the upper border of the latissimus dorsi, in the daily movement of the arm, and as perpetually rolling back again. The attachment of the serratus magnus to the scapula is such as to preclude the possibility of the accident here described; we know of no surgeon of authority who would affirm that he had ever witnessed the occurrence in an unmistakeable form. Moreover, in cases of deformity (lateral curvation), in which with the posterior projection of the ribs the scapula is so thrown backwards that the inferior angle never comes into relation with the latissimus dorsi, the movements of the arm at the shoulder-joint are quite perfect. Had Mr. Humphrey availed himself more liberally of Professor Owen's great discoveries and generalizations, and less frequently of detached remarks from our German brethren, he might have spared himself the anomaly of putting in the form of "a note" the explanation of the homologies of the olecranon and the patella; nor would he have called the pisiform bone of the carpus a sesamoid bone.

We do not recommend this work to quite the same class of persons who should avail themselves of the labours of Mr. Gray. There is a want of that "harsh conciseness," which would be felt by the anatomical student. But as lectures to a general or university audience (which is in truth their proper character), they would command attention, and gain for the author the repute of industry and thought. He must have continued his researches for many years, and we wish him every success in his endeavours to unravel some of the mysteries which ignorance still attaches to the science of anatomy. To him (with others) is undoubtedly due the merit of early introducing into his lectures the system of homology, and if we recommend him to pause ere he detaches himself from the followers of Owen, we do so with the firm conviction that a brighter path of renown will be open to him by further developing that which the great Hunterian Professor has established on a basis not easily to be shaken, than by ranging himself with those who somewhat hastily and captiously wrangle over a subject which it needs a lifetime to master.

The execution of the Drawings by Mrs. Humphrey is a work equally honourable to her as a wife and a lady of talent. Her example may be followed by others with advantage.

Dr. Ogilvie puts forward a claim, which we readily accord him, of popularizing the views now generally held by philosophical naturalists in regard to a common plan of construction traceable in each of the primary divisions of the animal kingdom:—

> "We are as far as ever from any general form common to all animals. We cannot, for instance, say in what direction the body should be extended, what sort of appendages it should have, or how they should be arranged, we can predicate nothing of its supporting framework, or of the conformation and disposition of its internal organs. Hence the impossibility apparent, on a moment's reflection, of making the rudest sketch, or even conceiving one, which shall stand for an animal in general, without the specialities of any particular class, of delineating anything which, without representing exclusively a star-fish or a snail, a worm or an insect, a fish or a bird, a reptile or a quadruped, or any other *particular kind* of animal, shall yet indicate so much as is common to them all. Not that it is intended to deny the possibility of assigning a common form which shall be more or less applicable to many different animals." (p. 3.)

He proceeds to give a very clear account of the peculiarities of the vertebrate type, from which all controversial anatomy is banished. The following is a specimen of his style of writing:—

> "When these bones (i. e., the vertebræ) are in their natural connexion, the bodies form by their superposition the massive column of the spine, while the superior arches jointly form a latticed canal termed neural or nervous, from its lodging the great nervous chord; and the inferior arches form another, called hæmal, from its enclosing the heart and other blood organs along with the alimentary canal. This hæmal canal is much more irregular than the neural; it is in some places very defective, so far as the bony skeleton is concerned; but when it has such bony walls it is in general much larger than the other, from the greater bulk of the

> organs it has to enclose. Of this we have illustration in the chest and pelvis, where the whole circuit of the ribs and breastbone in the one, and of the pelvic bones in the other, are considered as entering into the hæmal arches of the corresponding vertebræ." (p. 14.)

The work consists of nine chapters, in which the author leads us from the vertebrate type, through the articulate and mollusca, to the radiata and protozoa. He speaks of the mutual relations of the leading types of organization, of type and design co-extensive with organic nature, and of the bearing of such knowledge on natural theology.

> "There are some (he says) who still think all the points of animal organization explicable on principles of mere adaptation. It may not, therefore, be out of place to adduce a few illustrations of their inadequacy to account for the peculiarities observed; and indeed we need not go far to look for such, for in the very exact symmetry of our own bodies, and of those of animals generally, we meet with an instance of an arrangement of parts to which we can rarely assign any obvious end of direct utility. The further we trace back the course of development, the more marked is the symmetry, and the more universal. It is therefore evidently the rule, the departures from it being due to the subsequent disproportionate development of various organs. These changes we can occasionally account for on principles of adaptation; as, for instance, the greater size of that one of the anterior pair of pinching claws with which the hermit-crab closes the mouth of its borrowed shell. But we cannot show that the symmetry itself is subservient to the well-being of the animal in any constant or general way; though in special cases, of course, it may; thus in birds and insects it probably makes the balance more perfect for flight; and it has been observed that in these, of all animals, this arrangement is most perfectly carried out. Yet the singular fact of the non-development of the right ovary in birds shows that we must not even here lay too much stress on the symmetry of their other organs." (p. 140.)

We cannot forbear quoting one of Dr. Ogilvie's concluding passages:—

> "Strange to say, neither in Oken, by whose penetrating intellect were laid the foundations of the science of typical forms, nor in some of those who since have most successfully prosecuted it, did the principles they unfolded awaken any recognition of the moral attributes of God. Immersed in a dreamy pantheism, they could regard Him only as the animating principle of the universe, or lower still, simply as a necessary existence inevitably manifesting itself by a continued succession of phenomena, like a great panorama ever unrolling. But the reproach which has in consequence attached to such investigations, is in reality most unfounded; for so long as the truth of the Divine personality is firmly grasped, the evidences of unity of organization, instead of militating against the free agency of the Creator, tend greatly to elevate our conceptions of His power and wisdom. We then see that in His works a greater problem is solved than the mere adaptation of means to ends; for this, without losing any of its completeness, is combined with a certain harmony and uniformity in the means themselves. We see the Almighty Creator, for the manifestation of His glory or other wise purposes, subjecting himself, as it were, to laws, that the power and wisdom which bring it to perfection all the same, may be the more apparent." (p. 167.)

We trust that the recent changes which the examining boards have thought fit to authorize—namely, that of the establishment of practical examinations in anatomy before the final test on general professional proficiency—will have as one good result the re-development of industry among students in the dissecting rooms. For many years the lecture theatre has been the great centre of attraction; a false but captivating system of tuition has been all-popular; a young man from an agricultural district, with just a bare knowledge of his mother-tongue, gasps in wonder to hear a fluent lecturer quote the opinions, the theories, and the writings of anatomists, physiologists, and *savans* of all ages, of all countries and languages. He does not know that this stupendous knowledge rests on no firmer basis than Schmidt's Jahrbücher, and that it is as easily acquired as cast aside. The first business of the student of medicine is to gain a thorough knowledge of the human body. For this end he must frequent the dissecting rooms, and he may obtain all the assistance which he requires from writers of his own country. Where can a more accurate work be found than Ellis's 'Demonstration?' where a more complete treatise than Quain and Sharpey's? To these must be added

Gray's 'Descriptive Anatomy.' And in appealing to these and other gentlemen to do their best to raise the subject to the rank of a science, we feel assured we are only uttering the wishes of the best-informed of educated society.

REVIEW VII.

Pathologie und Therapie der Muskellähmung. Von Dr. HERMANN FRIEDBERG, Dirigenten einer chirurgischen und augenärztlichen Privatklinik, Docenten an der Friedrich-Wilhelm's Universität in Berlin. Mit 4 Tafeln Abbildungen.—*Weimar*, 1858. pp. 349.

The Pathology and Therapeutics of Muscular Paralysis. By Dr. HERMANN FRIEDBERG. With Plates.

MORBID anatomists often resemble the engineer, who, in determining the causes of an accident on a railroad, would seek for them only in the construction of the steam-engine, in the safety-valve, the boiler, or some part of the moving power of the whole apparatus, while he neglected the axletrees of the carriages or their wheels, or some other extraneous part of the structure, which might no less have been faulty, and have thus given rise to a fatal accident. Similarly have medical men been disposed to look to the viscera too exclusively for the interpretation of morbid phenomena, forgetting or overlooking the important bearing of the external organs upon disease and its manifestations. There can be little difference of opinion as to the relative importance of the various organs of the body; their necessity to the preservation of physical and mental life and individuality at once enables us to fix their rank; but while we willingly concede to the nervous centres, the organs of circulation and respiration, the organs of assimilation, a higher position in the scale than to the organs of locomotion and protection, yet we fail in taking a comprehensive view of the physiology and pathology of these parts if we deny to the latter the undoubted prerogative that they possess of initiating processes which may re-act upon the former. We do not claim absolute mathematical stringency for any classification of morbid processes; we know that it is scarcely ever possible that in man one organ should suffer without consensual suffering of the entire system; yet, unless we are to lose ourselves in vague generalities which lead to nothing, we cannot but see that, physiologically and pathologically, certain functions and derangements of function take place in one or more organs antecedently to changes that are effected in other organs, and that these two series of changes bear to one another the relation of cause and effect.

The more profound our knowledge of the changes that accompany functional derangement of the body, the more completely we disprove the evidence of ancient sluggishness, that life could be arrested without material change in the organs necessary to life. Every autopsy in which we fail to discover those changes proves merely that our knowledge of life and death is defective, not that the changes do not exist. How many sudden deaths for which formerly no apparent cause could be assigned, are now readily traceable to plugging of the arteries, or to fatty degeneration of the heart. Every advance made by aid of the scalpel, the microscope, or the test tube, towards the determination of organic change, diminishes those *opprobria medicinæ* the cases of disease and death for which no adequate cause can be assigned. Nervous pathology has been in medicine the scapegoat upon which more professional sins have been heaped than any other; and hysteria, neuralgia, nervous apoplexy, hyperæsthesia and anæsthesia, spinal irritation, are a few of the many terms with which we seek to cloak our ignorance of the real nature of many disorders, the intimate nature of which is beyond our ken. But there are many and able workers in the field. Every now and then a stride is made in advance which opens out a new vista, while it shows the value of steady perseverance even where no immediate result promises to accrue from our researches. At the present moment the organs of locomotion engage the attention of inquirers more than they have done before, and the muscles are made responsible for derangements seriously affecting the health of the individual and involving his prospects of life, which have long been regarded as depending upon disease of the nervous centres. The interesting work of Dr. Inman has recently drawn our attention forcibly to the fact, that overworked muscles may cause pain in their tendinous insertions, simulating inflammation of vital viscera or neuralgia of nervous cords; he has shown the close relation between anæmia and exhaustion, and tendon-pain and muscular spasm, from which important corollaries follow regarding the treatment of the disorders in question. Dr. Inman's researches and observations have materially narrowed the field of hysteria and spinal irritation. Dr. Roberts has successfully collected the evidence we possess regarding the existence of fatty degeneration of the external muscles, giving rise to a disease which, until a recent date, was confounded with paralysis arising from lesion of the brain or spinal cord. Dr. Friedberg, the author of the work heading this article, has still further extended the pathology of this disease, and investigated the question of muscular degeneration in connexion with various disorders which have not hitherto been regarded as being primarily seated in the muscles. While we believe that Dr. Friedberg has by no means exhausted the topic, and while it appears to us that he somewhat confuses the subject by mixing up primary and secondary morbid conditions as essentially identical, we owe him thanks for enlarging our field of view, and trying to combine into one chain numerous detached links which by themselves lacked meaning or have been erroneously interpreted.

The main difference between neural and muscular paralysis, in regard to the nutrition of the muscle, consists in this, that in the former the nutrition of the muscle is secondarily affected, whereas in the latter it is the primary disturbance which attracts the attention of the patient and his physician: in order to define the disease as originating in the muscle itself and depending upon a lesion of its proper texture without involving himself in any pathological theory as to the nature of the changes that are effected, Dr. Friedberg coins the word *myopathia*, with its derivative *myopathic*. Myopathia is a disease of the muscles, and as movement is the main function of these organs, it is scarcely conceivable that disease should settle in them without proportionately impairing this property. Loss of motor power is one form of paralysis, hence myopathic paralysis is almost necessarily a term at once required to designate the site and chief expression of muscular disease. The main objection that we see to this new term is, that it multiplies the designations of a disease already known as progressive muscular atrophy, fatty degeneration of the muscles, progressive muscular paralysis, and wasting palsy. In itself it is good, because it involves no hypothesis, but simply expresses a fact. Having in our last number* dwelt upon the symptomatology and anatomical changes that take place in the muscles in this disease, we need not again recur to them, but shall only draw attention to the features peculiar to Dr. Friedberg's work.

After passing in review the doctrines advocated by Romberg, Cruveilhier, Valentin, and Leubuscher regarding the disease under consideration, Dr. Friedberg sums up his arguments in favour of muscular atrophy occurring independently of lesion of the nervous centres and the nerves, thus:—

> "1. The grey matter of the cord may be extensively diseased, without the occurrence of progressive atrophy and paralysis of the muscles.
>
> "2. Other parts of the spinal cord may be diseased in progressive atrophy of the muscles without the grey matter being involved.

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* Oct. 1858.

"3. Even when the entire muscular system is involved in the atrophy and paralysis, both the spinal cord and the nerves proceeding from it have been found perfectly healthy.

"4. The assumption that degeneration of the roots of the spinal nerves is the cause of the paralysis connected with muscular atrophy, is opposed to scientific data." (p. 118.)

Dr. Meryon's well-known cases,* with others that have been examined with equal care, are brought forward in support of the above doctrines. The change itself which takes place in the muscular fibre may be derived from various causes, which the author classifies under six heads, and accordingly he establishes six varieties of myopathia.

"The derangement of nutrition of the muscles inducing paralysis may arise—

"1. From propagation of a similar morbid process from adjoining organs (myopathia propagata sive communicata).

"2. From mechanical injury (myopathia traumatica).

"3. From sudden change of temperature (myopathia rheumatica).

"4. From diseased conditions of the blood (myopathia dyscrasica).

"5. From diminished supply of blood and diminished exercise (myopathia marasmodes).

"6. From causes which are beyond our reach (myopathia simplex)." (p. 136.)

Why we should term that disease *simplex* of which we know least, we are puzzled to determine; but we are acquainted with the difficulty of inventing a nomenclature, so let that pass. We have, however, a much more serious objection to raise to the pathological doctrines taught by our author in the ensuing section of his work, doctrines for which we and our readers would be the less prepared after learning that the author lays so much stress upon the derangement of nutrition accompanying the muscular atrophy, and after perusing the list of circumstances to which that derangement is attributable. Dr. Friedberg enunciates that inflammation of the muscle induces the changes which characterize muscular paralysis, and that muscular atrophy can only be the consequence of a disorder which alters the nutrition of the muscle 'in the same way as inflammation;' in other words, that in the six varieties of muscular paralysis enumerated above, inflammation is equally the *causa proxima* in all. With all respect for the learned author, we cannot but think that his desire to generalize has here led him into a serious error, for the time has passed when degenerative processes and inflammation were regarded as identical. Traumatic influences, sudden changes of temperature, and the propagation of inflammation from adjoining textures, may undoubtedly cause muscular paralysis by exciting inflammation in the muscle, but we are unable to understand how Dr. Friedberg's fourth and fifth varieties can be regarded as dependent upon inflammatory action. Under those forms which depend upon diseased blood he enumerates typhus and lead poisoning as causes; these can, as little as arrest of the supply of blood or diminished exercise, be regarded as in any way suggestive of inflammation. While, then, we will not deny that primary inflammation of the muscles may be the immediate cause of muscular paralysis, we think that it is essentially a degenerative process that leads to these results, without at any time showing symptoms that are of an inflammatory character. Moreover, we would suggest that the majority, if not all, of the cases in which inflammation is the cause, may be classed together as second to, or symptomatic of, some other affection; at all events there is, even according to the author's showing, a wide difference between those which may be regarded as inflammatory and those in which inflammation manifestly has no part. We have at this moment a case of well-marked muscular paralysis under our care, in which it is impossible to trace any symptoms indicating in its history, inflammation; it has been from the first degenerative; a gradual wasting, dependent doubtless upon an impaired state of nutrition, in which *calor*, *rubor*, *tumor*, *dolor*, at no time or in any combination, formed an item. But even if in one or other of the varieties of muscular atrophy, inflammation is the first step in the morbid process, it generally comes under the notice of the practitioner only when all trace of inflammatory action has subsided, when he has to deal only with a condition demanding the most active and persistent stimuli that we are able to apply. Nor does it appear that there is any material difference between our views and those of the author upon the question of treatment. His theory does not interfere with the practical tact which directs him to avoid all antiphlogistic remedies for the cure of muscular paralysis. Of this we will speak again. For the present a brief analysis of the features and circumstances characterizing and surrounding the various forms of muscular paralysis spoken of by the author, may be the subject more immediately interesting to our readers.

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* Medico-Chirurgical Transactions, vol. xxxv.

1. *Myopathia Propagata.*—A muscle that is in contact with inflamed tissue may cease to contract, either because movement of the adjoining parts is painful, or because the inflammation is communicated to the muscle itself. In peritonitis and in pleuritis, the abdominal muscles and intercostals may be respectively involved in this way; the right half of the diaphragm may thus be paralysed in inflammation of the liver, and the muscular tissue cease to respond to the stimulus of the blood in pericarditis. Neuropathologists are apt to treat all these and analogous cases as the result of reflex nervous paralysis, overlooking the most immediate effects of influence by mere contact. Again, conjunctivitis may in a similar manner induce paralysis of the levator palpebræ superioris and of the orbicularis palpebrarum, so as to give rise to ptosis and ectropium. Inflammation of the lumbar muscles may result from nephritis; the muscles surrounding the shoulder and other joints are attacked by inflammation, and its degenerative sequelæ, as a consequence of inflammation of the parts immediately involved in the structure of the articulation. All these are clearly not primary affections of the respective muscles, and therefore differ essentially from Cruveilhier's disease.

2. *Myopathia Traumatica.*—A change in the nutrition of the muscles may be induced by an external injury, which need not necessarily be very severe to give rise to the effect spoken of; there may be comparatively little pain or other evidence of a serious lesion, where subsequently the degenerative process is developed.

> "Traumatic myopathia not unfrequently occurs in children who, for instance, have been suddenly caught up by the arm when they were on the point of falling, or were roughly pulled by the leg while they were undressing. Some hours, or at the latest some days after the injury, one finds, generally when the child is taken out of bed, that one arm or leg is motionless and very painful to the touch. I have seen cases of this kind, in which a partial dislocation or an affection of the nervous centres had been erroneously diagnosed."

Here, as in regard to the other varieties, the author gives detailed cases that have fallen under his own observation, confirmative of his views, and analyses critically the accounts found in various authors of affections which, as he avers, have hitherto been falsely interpreted. Thus he considers that the debility and emaciation of fractured extremities after the consolidation of the fracture is attributable rather to inflammation set up in the muscles contiguous to the injured bone, than to mere inactivity, because the loss of power and substance bear no ratio to the period during which the limb has been inactive. Again, after amputation, we meet with a traumatic myopathia in the muscles of the stump, as evidenced by the twitchings, the fatty degeneration, the atrophy of the muscles, and by the bad position of the stump.

> "The twitchings are at first to be attributed to the altered pressure of the blood, but subsequently to the deranged nutrition of the muscles, resulting from the lesion and leading to atrophy and paralysis. This does not exclude the possibility of the muscular twitchings originating in the lesion of the nerves, nor of the muscular atrophy resulting from loss of movement. Here we also see the paralysis affecting chiefly the extensors; hence the stump assumes the flexed position."

Dr. Friedberg points out that excessive fatigue and other lesions of certain muscles may induce traumatic myopathia in them, and refers to cases reported by Darwall, Aran, himself, and other writers, in evidence of his observations.

3. *Myopathia Rheumatica.*—Dr. Friedberg avoids a discussion as to the intimate nature of rheumatism, by defining rheumatic myopathia to be that variety of the affection in which degeneration of the muscles results from exposure to sudden changes of temperature. It may be acute or chronic, and lead to universal degenerative atrophy and palsy of the muscles. The author describes rheumatic inflammation of the muscles as proceeding either from the fascial and subfascial areolar tissue, or attacking *ab initio* the interstitial areolar tissue of the muscles. The continuity of the different layers of areolar tissue leads to infiltration of the deeper and superficial parts coincidently; this infiltration has been termed by Froriep the rheumatic callosity (*rheumatische Schwiele*), and he asserts that it invariably occurs in the skin and the subcutaneous areolar tissue as an accompaniment of rheumatic pains. Among the various instances brought forward by Dr. Friedberg, we may briefly quote the main features of one detailed by Cruveilhier. A rope dancer, named Lecomte, spent a night, in 1848, on damp ground in the open air; on waking up he felt a numbness pervading the right side of the body. Three weeks later he perceived so great a weakness in the right hand that he found it very difficult to grasp anything. This remained the only symptom for about a year; he then again passed a night in the open air, in cold damp weather, followed by considerable weakness in the lower extremities. From this time the disease made rapid strides, and when the man came under the care of Professor Cruveilhier, the paralysis and atrophy, which were accompanied by the characteristic fibrillary tremors, had already involved numerous muscles. The special senses, the intellect and sensibility, throughout remained unaffected. The upper extremities, the muscles of deglutition, those of the larynx, the thorax, and the face, were affected, though they still contracted under the galvanic stimulus. The patient could only swallow with great difficulty after forcing the mouthful into the pharynx. The paralysis and emaciation of the muscles continued to increase, and the patient died in January, 1853, of influenza.

It is manifest that rheumatic myopathia may, as the author points out, be complicated with an affection of the cord, and it will often be a matter of some difficulty to establish the diagnosis; on the one hand, we have in idiopathic muscular paralysis atrophy of the muscles disproportioned to the duration of the disease, and affecting prominently certain groups of muscles in which fibrillary twitchings or tremors are perceived, sensibility remaining unimpaired and the affection spreading consecutively from one group of muscles and one extremity to another; in pure spinal paralysis, both sides of the body are commonly affected at the same time, motion and sensation are generally both more or less impaired, there is tenderness and pain in some portion of the spinal column, the wasting is more uniform and more gradual, and the sphincter muscles are more liable to become paralysed than in muscular palsy. The galvanic test may also be of assistance in cases of doubt, inasmuch as the reaction to the galvanic current will in muscular palsy be exactly proportionate to the amount of degeneration which has taken place. Whereas, in spinal paralysis dependent upon disease of the cord, the galvanic stimulus will from the first cease to produce a reaction, and all the muscles of an extremity will equally show the same amount of impairment.

4. *Myopathia Dyscrasica.*—We have already expressed the objections that we feel to the author's mode of viewing idiopathic muscular palsy, and should be disposed in all cases to assume the presence of a dyscrasia, without which the change of nutrition characteristic of the disease could not be excited. If any affection could, in our opinion, be cited as peculiarly representing a dyscrasic disease, it is fatty degeneration of the voluntary muscles. Dr. Friedberg, however, limits the term to certain forms of the malady accompanying or following diseases that impair the nutritive functions. Cholera, dysentery, typhus, gastric fever, the exanthematic fevers, are successively reviewed in their bearings upon the disease in question, and cases are adduced to show that muscular degeneration may occur as one of their sequelæ. With regard to the paralytic affection sometimes occurring during pregnancy and after confinement, the author remarks, that although

this is attributed to pressure exerted upon the nerves, this cannot be the case where the paralysis occurs at an early period of pregnancy or after the birth of a child with a head that is too small to exercise any considerable pressure upon the lumbar plexus. Moreover, the nervous origin of these palsies must be rejected when the paralysed muscles do not correspond to the nerves that may be thus compressed. The author brings forward the authority of Lobstein and Virchow to prove that the violent muscular efforts during labour may give rise to inflammation of the muscles; he forgets, however, that the paralysis he speaks of does not occur in the muscles that are most implicated, those of the abdomen, but in those of the lower extremities, which are not more taxed than the muscles of respiration during the act. Rickets, osteomalacia, and lead-poisoning are also examined in reference to their capability of producing muscular palsy.

5. *Myopathia Marasmodes.*—This term is intended to define the variety of degenerative disease of the muscles resulting from the absence of one of the three constituents of healthy muscle, the due alternation of expansion and contraction, the necessary supply of blood, and the requisite innervation. Myopathia is not necessarily caused by deficient exercise of the muscles, but occurs when this element of disease exists and is accompanied by other morbid tendencies in the muscle; firm bandages, splints, and other applications may act thus if too great pressure is used, which interferes with the nutrition of the muscle; the diseased condition of the muscles results invariably when the joints remain immovable in consequence of organic changes, a process which being altogether secondary, and one that only affects the organs of motion because they can no longer be put to their normal uses, is as much physiological as pathological; similar conditions will readily suggest themselves to our readers, in which loss of function entails loss of structure. Defective supply of blood acts as an inducing 'cause in the wasting of old age, in the nutritive changes of the muscles accompanying disease of the vessels or their obliteration, and resulting from the pressure exercised by tumours.

Thus in a lady, who had suffered for several years from severe pains and complete motor paralysis of the lower extremities, though the adipose tissue of the body was much developed, the muscles of the legs universally presented fatty degeneration; some were atrophic, others, owing to the deposit of more fat, were found by Dr. Friedberg to have increased in volume. The abdominal aorta from below the point at which the inferior mesenteric artery was given off, was considerably diminished in size, the parietes thickened, and exhibiting extensive atheroma and chalky deposit. The various branches showed a similar morbid condition. For cases illustrative of the other varieties we must refer to the work itself. We pass to the consideration of the sixth and last form of the disease.

6. *Myopathia simplex* is defined by the author as that variety which occurs without known cause, and which may attack numerous sets of muscles, or be confined to single muscles. These, according to Dr. Friedberg, are the cases which are ordinarily regarded as paralytic affections of individual nerves. From this point of view, he criticises several cases reported by authors, of which we may specially mention one, because readily accessible to our readers. Professor Romberg, in his work on the nervous diseases* of man diagnosed in a woman, aged 69, paralysis of the hypoglossus; the whole tongue was uniformly paralysed and atrophied, while taste and sensibility remained unimpaired. Our author argues that Romberg's view regarding the nature of this woman's disease must be erroneous, because many of the muscles that receive filaments from the hypoglossi did not manifest any paralytic affection, and he regards the lingual palsy only as a part of a more wide-spread degeneration of the muscles of the neck and thorax.

In determining the treatment of myopathic paralysis we must, as the author justly observes, consider the restoration of the normal nutrition of the muscles as the chief indication. The result will be more or less favourable, according to the period of the disease at which curative proceedings are commenced. The intensity of the degenerative process, the number of muscles affected, and the cause of the disease, will modify the prognosis. An hereditary taint materially adds to the difficulties and diminishes the prospect of cure. The treatment itself is indicated by the pathology of the affection. Where acute inflammatory symptoms continue antiphlogistic remedies will be necessary; but though Dr. Friedberg gives this advice, in order that he may be consistent with his theory regarding the cause of the affection, he makes little of it, and at once adds the caution not to pursue the antiphlogistic course as soon as the inflammation has entered into the chronic stage. We lay more stress upon the remark that "the chief remedy for the protracted nutritive disturbance of the muscle consists in stimulating it methodically to contract." Contraction of the muscle promotes its nutrition by rousing the circulation, and thus counteracts the degenerative process. Electricity and suitable gymnastic exercises are specially adapted to cause muscular contraction. Both should be used perseveringly and systematically to produce a good result. The former especially requires to be administered with care and with a due knowledge of the object to be attained, and of the parts to be acted upon. The current may be applied either directly to the muscles affected, or it may be conveyed to them by application of the negative pole to the nerve going to the muscle, while the positive electrode is applied to the distal termination of the muscle. The second method is more suitable where a set of muscles is to be acted upon, the first where we desire to stimulate the individual muscle. The electric or galvanic agent must be supported by friction and active and passive movement varied and increased according to the patient's condition.

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* Dr. Sieveking's translation, vol. ii. p. 307.

The work concludes with some remarks on the mode of counteracting and removing contractions of joints resulting from muscular degeneration, but which need not detain us.

In bringing this account of Dr. Friedberg's labours to a close, we do not hesitate to recommend his work to our readers. His facts and arguments deserve to be studied, and, although we have been unable to coincide with him on all points, we have perused his book with much interest, and regard it as eminently suggestive and practical. If a second edition should be called for, we think that the treatment of the subject may be much simplified, and especially does it appear to us that some confusion would be avoided by clearly distinguishing between those cases of muscular degeneration which are primary or idiopathic, and those which are secondary or symptomatic.

REVIEW VIII.

1. *Traité des Applications de l'Electricité à la Thérapeutique Médicale et Chirurgicale.* Par A. BECQUEREL, Médecin de l'Hôpital de la Pitié, &c.—*Paris*, 1857.

Treatise on the Application of Electricity to Medical and Surgical Therapeutics. By A. BECQUEREL.—*Paris*, 1857.

2. *Die Electricität in der Medicin.* Studien von D. HUGO ZIEMSSEN. Privat-Docent und Assistenz-arzt an der Medicinischen Klinik zu Greifswald. *Berlin*, 1857.

Electricity in Medicine. Studies by Dr. HUGO ZIEMSSEN.—*Berlin*, 1857.

3. *Ueber das Hemmungs-Nerven-System für die peristaltischen Bewegungen der Gedärme.* Von Dr. EDWARD PFLÜGER.—*Berlin*, 1857.

Upon the Inhibitory Nervous-System for the Peristaltic Movements of the Intestines. By Dr. EDWARD PFLÜGER.—*Berlin*, 1857.

4. *Preliminary Account of an Inquiry into the Functions of the Visceral Nerves,*

with special reference to the so-called "Inhibitory System." By **Joseph Lister**, F.R.C.S., Eng. and Edin. ('Proceedings of the Royal Society of London; vol. ix. No. 32.')

5. *Epilepsy and other Convulsive Affections, their Pathology and Treatment.* By **Charles Bland Radcliffe**, M.D., F.R.C.P. *Preliminary Considerations respecting the Physiology of Muscular Motion.—London*, 1858.

6. *Ueber Methodische Electrisirung gelähmter Muskeln.* Von Dr. R. **Remak**.—*Berlin*, 1845.

Upon Methodical Electrification of Paralysed Muscles. By Dr: **Remak**. (Revised by **Duchenne**, in Schmidt's 'Jahrbücher, 1856. Bd. 89. p. 250.')*

7. *On the Therapeutical Action of the Constant Galvanic Current.* By Dr. **Robert Remak**. ('Medical Times and Gazette, May 8, 1848.')

8. *Bericht über medicinische Electricität.* Von Dr. H. E. **Richter**, und Dr. B. A. **Godmann**. ('Schmidt's Jahrbücher, Bd. 94, Heft i. p. 97.')

Review of Medicinal Electricity. By Drs. **Richter** and **Godmann**.

9. *Recherches Expérimentales sur la Possibilité du Passage à travers le Centre Nerveux, de Courants Electro-Magnetiques appliqués à la Peau, chez l'Homme.* Par M. F. **Bonnefin**. ('Journal de la Physiologie, tom. i. No. 3.')

Experimental Researches upon the possibility of Passing Electro-Magnetic Currents through the Nervous Centres of Man, by means of their application to the Skin. By M. F. **Bonnefin**. ('Journal of Physiology, edited by Dr. **Brown Séquard**. No. 3.')

10. *On Local Anæsthesia and Electricity.* By **Benjamin W. Richardson**, M.D., &c. ('Medical Times and Gazette, September 11, 1858.')

11. *On the Treatment of Paralysis by Electricity.* By J. **Althaus**, M.D. ('Medical Times and Gazette, December 26, 1857.')

12. *On Localized Galvanism, applied to the Treatment of Paralysis and Muscular Contractions.* By **Richard Moore Lawrence**, M.D.—*London*, 1858.

If, jealous for its reputation as a therapeutic agent, electricity could just now become articulate, its earnest cry would be, "Save me from my friends!" Rescued, but a few years ago, from comparative poverty, obscurity, and disgrace, by the praiseworthy efforts of Golding Bird, Duchenne, and Richter, it advanced under the guardianship of these and like-minded men until it found itself not only in good society, but enjoying a somewhat proud position in the apparatus medicus of England, Germany, and France. But, so soon as it had attained this eminence, and attracted the regard of the general community, numbers of men, who, judging from their own works, were unable to observe correctly or think wisely, gathered round the popular novelty; with a grand flourish of brazen trumpets announced themselves its protectors, and issued proclamations, in the form of worthless books, that this mighty agent, under the guidance of their mightier selves, not only would, but had healed all and every one of the "thousand natural shocks that flesh is heir to." From such vain pretension nothing but disappointment and disgust could follow, and already the tide of popularity is turning. Electricity cannot do what these, its so-called "friends," have asserted that it can; and, despite its manifold powers for good, it is in danger of being again regarded as a quackery, and condemned to another period of obscurity and neglect.

In France there are treatises on Electricity "appliquée au traitement curatif des nevralgies, des rhumatismes, des paralysies, des tumeurs, &c., et en général des affections morbides, souvent reputées incurables;"† and in Germany there are similar productions, written, as, Richter says, in "halb-populärer echt-französisch floskel-reicher Weise," while in our own country there are individuals who write works for the purpose of instructing the profession and the public in the theory and practice of electric therapeutics, but manifest a lamentable want of acquaintance with some of the first principles of physical and physiological science. The unqualified laudation which this valuable agent receives at the hands of such writers is not likely to secure to it a permanent place in our *materia medica*.

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* We much regret that an important work by Dr. Remak, entitled, 'Galvano Therapie der Nerven und Muskelkrankheiten' (Berlin, 1858), has reached us too late to be noticed in the present article. Ed.

† By M. Briand.

Convinced of the great utility of a well-applied electricity in properly selected cases, it is with earnestness that we protest against its ruthless and quack-like advertisement as a panacea, and shall endeavour, in the following article, to point out its real physiological effects, its true therapeutic position, and the best modes for its application.

Within the last ten years two articles on the employment of electricity in medicine have made their appearance in this journal. In the first of them* the researches of Dr. Golding Bird, Mr. Donavan, and others, were brought under review; and in the second† an account was given of the then recent labours of Duchenne, Meyer, Guitard, and Richter; we shall not, therefore, in the present article, revert further to those positions which we considered established in the year 1855, but shall confine our remarks to the history of electricity, and especially as a therapeutic agent, since that period.

Those effects of electricity which may be termed "physiologic," differ widely in their character; their variety being dependent upon two classes of conditions, one appertaining to the electricity, the other to the organism.

As to the conditions inherent in the electricity, we must notice the quantity, intensity, and mode of transmission of the currents, each of which influences the physiologic effect.

The quantity of electricity is dependent, theoretically, upon the number of polar chains that can be established at the same time in a particular voltaic arrangement; and is determined, practically, by the size of the positive plate, and the relatively larger size of the negative. Other conditions affect the quantity of electricity, but do so only to a trifling degree. Now, although variations in the quantity of electricity occasion corresponding variations in its physiologic effects, the latter are, for the most part, brought about indirectly; *i.e.*, through the intervention of some other changes, either thermal or chemical, which come between the electric force and the vital or physiologic result. Thus, the amount of heat generated in a wire connecting the two poles of a battery is in direct proportion to the size of a positive plate, or to the quantity of electricity evolved; and therefore, when an apparatus of such kind as to yield a large quantity is employed, a very painful amount of caloric is produced, which not only modifies the physiologic effects, but complicates and renders difficult the therapeutical application. Again, the amount of chemical decomposition which is produced by a voltaic apparatus, is determined by the quantity of electricity, or the number of polar chains which can be established; and this amount is sometimes so great that tissues are acted upon chemically, and their proper vital functions held in abeyance. A very strong current, as Bernard has shown, may destroy the property of a nerve, by acting chemically upon its tissue, thus producing a condition which differs entirely from that of exhaustion by a direct continuous current; for under the latter circumstances the inverse current restores the property, whereas in the former it is impossible to restore it.

Thus, the employment of electricity in too large quantity determines two classes of result which modify the physiologic effects—on the one hand the phenomena of sensation are deranged by the caloric produced; and on the other, nervous properties, of any kind, may be destroyed by chemical decomposition of the tissue. There are circumstances, however, in which both the thermal and chemical effects of galvanism are needed, such as the employment of the cautery, the coagulation of blood in an aneurysmal sac, the attempted solution of calculi, or the extraction of metallic poisons from the body; and as under all these circumstances a large quantity of electricity is required, the choice of instruments for such operations will be determined by their fulfilling that condition of supply.

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* Med. Chir. Rev., vol. iii., 1849. † Med. Chir. Rev., No. xxix., 1855.

The degree of tension, or intensity of the electric current, is, however, more influential than its quantity in determining physiological results; and it is to variations in this quality that we ordinarily apply the terms "strong" and "weak." There are several currents in common use for physiological experiment and therapeutical exhibition; and as this quality of tension or intensity is predicated of all of them, it is necessary that we should describe separately the conditions upon which its variations depend. But before doing so, inasmuch as some confusion has crept into the language of modern electricians, we will state as concisely as possible what these several currents are, what are their proper names, and in what way they have been erroneously designated.

In the wire which unites the two poles of a voltaic arrangement,—whether this consists of one pair of plates, or of one hundred pairs,—there is, when the wire is unbroken, a current of electricity, termed the "*initial current.*" This passes in the direction from the copper or negative metal, through the wire, to the zinc or positive plate. If this wire is broken, and the two ends of it are grasped by the hands, the individual so doing, becomes in that part of his body which intervenes between those two ends, a part of the voltaic apparatus; and the initial current passes through him in the direction described. If this wire, in any part of its course, be broken, there is at the moment of division, and existing at that moment only, another current setting in the opposite direction to that taken by the initial current. This has received various names: Duchenne has termed it an "induced current of the first order," but its proper designation is the "*extra-current.*"

Another wire placed near and parallel to the conducting wire,—viz., that through which the initial current passes,—has its polar condition so affected that an "*induced current*" is propagated through it in an opposite direction to the initial current. Several of such wires may be employed, at different degrees of proximity to the conducting wire, and in all of them there is an induced current, that which is nearest to the conducting wire being called the "*induced current of the first order,*" the next of "*the second order,*" and so on.

The currents employed by M. Duchenne, and about the different properties of which so much has been said and written, are the "extra-current" in the conducting wire, and an "induced current of the first order," in a parallel wire; and M. Becquerel states, that for Duchenne to designate them induced currents of the first and second order respectively:—"c'est créer . . . . un langage tout à fait différent de celui qui est employé par tous les physiciens" (p. 89).

The most striking differences between these various currents are to be referred to their degree of intensity, and this is determined by different conditions, of which the following are the most important. The "initial current" is intense in proportion to the number of the active cells in the battery, the nature of the electrolytes employed, and the integrity of conducting material throughout the whole circuit. The force of the "extra-current" is determined by the same circumstances; but that of the "induced currents" depends partly upon these, and also upon other conditions—viz, the size of the wire, the length of it which is brought into proximity with the conducting wire, and the presence and degree of additional magneto-electric induction. *Cæteris paribus*, the finer the wire and the greater its length the more intense is the induction.

In order to obtain great, and at the same time convenient, length of the wires, they are twisted into the form of a hollow spiral, or helix, the latter becoming, in itself, endowed with magnetic properties, one end of the helix being a north and the other a south pole. If into the hollow of this spiral or helix there are introduced bars of soft iron or steel, these bars become magnetic by induction; and thus the electrical force, developed in the battery cells by chemical action, becomes resolved into the correlated force of magnetism. But precisely the reverse order of induction may take place in another apparatus, and the "lifter" of a permanent magnet around which a copper wire is twisted spirally, at the instant that it becomes a magnet by induction, from contact with the poles of the permanent magnet, developes chemico-polarity, or electricity in the copper wire. The former arrangement is termed "electro-magnetic," the latter "magneto-electric;" in the one, electricity is developed from chemical decomposition, in the other, from magnetism; but in the former—inasmuch as magnetism is induced by the initial current—there is in addition to the "primary induced current," that order of induction which exists alone in the latter, and the addition of this is one mode of augmenting the intensity of the current.

Thus, then, the initial current developes magnetism in the bars of soft iron which are inserted into the hollow of its helix, and the presence of magnetism in these bars, at the moment of its induction, developes an electrical current in the copper wires; the intensity of the latter induced current, being, *cæteris paribus*, in proportion to the size of the temporary magnet, and determined or regulated by the length to which these soft bars are inserted in the helix. The tension, therefore, of the induced current depends upon that of the initial current, upon the size of the wire, upon its length—i.e., upon the number of turns in the spiral—and upon the force of magnetism temporarily developed in the bars of soft iron.

Whatever form of current is employed, the nature and degree of its physiologic effects—i.e., of its power to occasion vital phenomena, as distinct from chemical and thermal—are determined mainly by differences in this quality of tension. Generally speaking, a weak current produces feeble contractions of the muscles, and slight effects upon the organs of sensation; whereas a powerful current produces strong contractions and violent sensations. Both sensory and motor phenomena may be occasioned by the application of any one of these currents, but their variations in intensity render some more useful for one class of effects, and others for a second class. Thus, Duchenne has drawn considerable attention to the fact that the "extra-current" acts very readily on the muscles, and that the "induced current" affects more powerfully than the extra-current, the skin, nerves and retina. This difference of action he refers to a special elective power on the part of the two currents respectively; but Becquerel has proved that, in reality, it is merely dependent upon the difference of their intensity, the induced current having much greater tension than the extra-current. M. Becquerel has shown, by a simple experiment, in which he modifies the arrangement of the wires, that the effects which Duchenne attributes to the one current may be obtained from the other, and *vice versâ*, p. 90.

In proportion to the intensity of the current employed, electricity has the power of evoking the ordinary physiologic action of a nerve or muscle; of occasioning excessive and perverted action; of exhausting the functional activity for a time; or of destroying it altogether. In the first degree there is sensation or motion, each of these being within the limits of physiologic function; thus, luminous appearances, gentle sounds, gustatory effects, &c., on the one hand, and slight muscular contraction on the other,—contraction so slight as merely to exhibit the persistence of muscular contractility, and not to test its power,—are the results of applying an electric current of low intensity. If a stronger current is employed, the impressions upon the sensory organs become excessive in degree and painful in character; while, in the place of gentle muscular contraction, there is distressing cramp, or arrested (inhibited) action in certain organs. A still more violent current exhausts both nerve and muscle; and here sensation and contraction, though for a time withdrawn, are capable of being restored by repose, or by the inverted current; whereas the electricity may be so powerful as at once to put an end to the vitality of the tissues—i.e., to kill the nerve, limb, or individual through which it passes.

It is owing to these different effects of variations in intensity that electricity may be employed both physiologically and therapeutically for so many different purposes. As a test of irritability, or a gentle stimulus of weakened sensibility and contractility, the current of low intensity may be employed. For the sake of displaying the inhibiting influence of the vagi and the splanchnic nerves, or for awakening the torpid nervous centres of an individual poisoned by opium or alcohol, a more powerful current is required. Whereas for the relief of excessive muscular contraction, or of neuralgia, a still more intense current, one that shall temporarily exhaust the nervous function, may be employed.

Besides the quantity and tension of a current, the mode of its transmission exerts a notable influence upon its physiologic effects. Under this head we place the different actions of the continuous and interrupted currents; and with regard to the former, the changes produced by altering their direction; and with regard to the latter, their convection by means of moist or dry conductors, the rapidity or slowness of their interruption, and the degree of pressure with which the conductors are applied.

The most general differences between the effects of the continuous and interrupted current, are displayed very simply by an arrangement of M. Claude Bernard's, in which there are introduced into the same current from a small Cruickshank's battery; 1st, the nerve of a frog's leg, and 2nd, a delicate voltameter; the apparatus being so constructed that the current may be either continuous or intermittent. By this arrangement, says M. Bernard, it is shown that—

> "so long as the current is continuous, chemical effects are produced, and the physiological effects are 'nuls,' or at all events inappreciable. The facts are, that the water in the voltameter is decomposed by the current, whilst the limb of the frog remains perfectly motionless. But immediately that, by means of the interrupter, the current is rendered intermittent, everything is changed; the decomposition of water ceases in the voltameter, and the frog's limb becomes violently convulsed."[*]

But this experiment, although it illustrates very aptly the broadly marked difference between the effects of the continuous and intermittent current, by no means exhausts the subject of that difference, nor does it accurately represent all the facts. For the continuous current is not devoid of physiologic action, nor is the interrupted, under all circumstances, incapable of acting chemically. True, there is no visible contraction of the frog's leg, but under certain conditions the irritability of the nerve is exhausted, and under others it becomes increased. True, there is no sign of sensation in an amputated frog's leg, but the continuous current can produce sensory effects; for the proof of which let any one pass a continuous current through his tongue, or eyeballs; or, as Purkinje did, through the ears.[†] And, further, it is quite easy to produce permanent, *i.e.* tonic contraction of a muscle or group of muscles, as we have often done, by a current of this kind; and there is evidence to show that not only persistent contraction of muscles may be relaxed by such influence,[‡] but that hyperæsthesia may be reduced.[§]

Here then we have evidence of four kinds of physiologic action due to the continuous current, viz., the production of sensory effects, and also of motor, as well as the relaxation of spasm, and the reduction of hyperæsthesia, the different manner in which the current acts being mainly due to its intensity.

Other circumstances, however, influence the quality and degree of action exerted, viz., the direction of the current. Generally speaking, the transmission of a continuous current through a nerve, in the direction from the centre to the periphery, exhausts the vital property of the nerve; whereas, a current passed in the opposite direction, *i.e.* from the periphery towards the centre, increases the vital property. The former is termed "direct," the latter "inverse." Again, the direct current acts more energetically than the inverse in producing muscular contractions. This we have often witnessed, when employing, for the purpose of experiment or therapeutic application, an ordinary Cruickshank's battery, and so making use of the initial current that its intensity could be regulated and measured by varying the number of plates employed. Not only is the muscular contraction produced by transmitting a current from twenty plates, much stronger when this current is direct than when it is inverse, but a current of such low intensity as to cause no appreciable contraction when transmitted in the latter direction (inverse), will occasion very evident action when passed in the former (direct).

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[*] Leçons sur la Physiologie et la Pathologie du Système Nerveux, 1858, tome i. p. 151.

[†] Rust's Magazin, bd. xxiii. p. 297.

[‡] Remak, Medical Times and Gazette, May 8, 1858.

[§] Becquerel, p, 97.

Thus the difference between these currents must be remembered in testing irritability, as well as in testing power. It is sometimes a source of fallacy in physiologic experiments; as, for example, in examining the irritability of muscles in a paralysed limb, by passing the current from one arm to the other. In this case, it is, of course, direct in one arm, and inverse in the other; and we have frequently seen the difference between the irritability of the muscles on the paralysed and non-paralysed sides so slight as merely to equal, or even fall below that which exists between the action of the inverse and direct current respectively. When such is the case, the irritability appears greater in that limb through which the direct current passes, whereas it may be really less.

Two conditions affect the result of applying the interrupted current; one of these being the rapidity of intermittence, and the other the degree of contact which is ensured. Dr. Lawrance states that—

> "if we cause a paralysed muscle (whose irritability is normal), for instance, the flexor communis digitorum, to contract alternately with quick and slow intermissions, hanging weights at the same time to the fingers acted on by this muscle, we shall find that a rapidly intermittent current does not enable the muscle to raise a heavy weight so readily as one which intermits slowly."[*]

When the intermittence is extremely rapid, the effects resemble, *pro tanto*, those of the continuous current,[†] viz., exhaustion of motor and sensory functions; whereas in proportion to the integrity of contact, there is, *cæteris paribus*, a relative depth of effect. Thus, if dry conductors are placed upon the dry skin, the skin alone is irritated, whereas when moist conductors are applied with pressure the underlying muscles are affected. To these differences attention has already been directed in an earlier number of this journal.

The physiologic effects of galvanism are in part determined by the organism, and this in two ways, 1st, by the special property or function of the organ (nerve or muscle), and 2nd, by its condition at the time of application. With regard to the first of these, let it be observed, that there are different kinds of action, and degrees of action, in the same organs, and that the effect of galvanism is, in relation to its intensity, to elicit these different degrees of action. Thus, a nerve may have the power of either causing, increasing, diminishing, or arresting activity in certain muscles; and any one of these effects may be produced by galvanism, the particular effect which follows its application being determined by its intensity. The experiment of Eduard Weber[‡] upon the cardiac branches of the vagus is familiar to every one; the subsequent researches of Pflüger[§] have shown that an influence upon the intestinal movements is exerted by the splanchnic nerve, similar to that which the pneumogastric exhibits upon the action of the heart; irritation of the splanchnic nerves producing almost immediate arrest (*erzeugt fast augenblicklichen Stillstand*) of the peristaltic movements of the small intestines. (p. 66.) But the conclusion at which Pflüger arrived, viz., that there is a certain set of nerve fibres, the "inhibitory system," whose peculiar function it is to arrest or diminish action, is, we think, ably refuted by Mr. Lister,[‖] who has shown that the same nerves may either increase or diminish muscular contraction, according to the degree to which they are stimulated by galvanism. Mr. Lister thus "sums up" that portion of his paper which refers to the intestines:—

> "It appears that the intestines possess an intrinsic ganglionic apparatus, which is in all cases essential to the peristaltic movements, and which, capable of independent action is liable to be stimulated or checked by other parts of the nervous system; the inhibiting influence being apparently due to the energetic operation of the same nerve fibres which, when working more mildly, produce increase of function." (p. 372.)

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[*] On Localized Galvanism, p. 44.

[†] Becquerel, p. 97.

[‡] Wagner's Handwörterbuch. Art. Muskelbewegung.

[§] Uebur das Hemmungs-Nervensystem für die peristaltischen Bewegungen der Gedärme.

[‖] Preliminary Account of an Inquiry into the Functions of the Visceral Nerves, with special reference to the so-called "Inhibitory System." Proceedings of the Royal Society of London, vol, ix. No. 32.

A similar difference of effect, in dependence upon the degree of stimulation, has been demonstrated by Mr. Lister to exist in regard of the influence not only of the vagi, but also of "the sympathetic branches connecting the cord with the cardiac ganglia" (p. 378); and he has, as we think, very ably shown the error of the conclusion to which Professor Schiff arrived—viz., that the "inhibiting influence depends upon nervous exhaustion." (p. 379.)

As the result of all these researches we may conclude that the different powers are inherent in the nerves, and that galvanic stimulation does but call one or the other of them into exercise, in the same manner that other "stimuli," or "occasions of action," are known to operate; for example, emotion, which may either accelerate, retard, or even arrest the action of the heart.

Whatever, then, may be the properties of a nerve; whether they are sensory or motor; in relation to the particular properties of matter (light, taste, sound), or to peculiar conditions of the organism (fatigue, exhaustion, excitement); whether their function is to increase or to repress muscular activity; to occasion slight contraction, or persistent spasm; whatever these nervous properties may be, they can be called into operation by means of galvanism. But the special property which is elicited stands in definite relation to a certain amount of galvanic stimulus; and just as electric irritation of the retina produces the sensation of light, while a similar irritation of the crural nerve occasions muscular contraction, so a definitely proportioned galvanic stimulus of the splanchnic or pneumo-gastric nerves will elicit their property of increasing muscular movement in the intestines or the heart; and the same stimulus, differently proportioned, will call forth their inhibiting influence, and arrest the rhythmic or peristaltic action which they are destined to control.

But further, the condition of the organism at the time of its exhibition very materially influences the effect of electricity. To this we have already partially referred in detailing the different results of the continuous current. A nerve in a state of hyperæsthesia may be reduced in sensibility; an enfeebled nerve may have its dormant faculties aroused; a motor nerve, half-paralysed from inaction, may be stirred up to healthy exercise; while a similar nerve, so irritated as to induce tonic spasm, may have its augmented irritability brought down to the average standard. But besides these results, there are some due to the action of electricity which should be borne in mind. The nerve may be so affected by a galvanic current that it becomes partially or completely "exhausted;" and at the different stages of its exhaustion there are different phenomena. Thus Bernard has shown, that when first operating upon a motor nerve, there is a simple contraction in the muscles it supplies at the entrance of the current, whether the latter be direct or inverse. That after a time there is contraction at both the exit and the entrance of either current. That subsequently there is contraction only at the entrance of the direct current, and at the exit of the inverse; and that, finally, there is contraction only at the entrance of the former. These four phases he terms respectively—1, unique; 2, double; 3, alterne; and 4, ultime;* and they represent different conditions of the nerve-function. In the first, there is the physiological result; in the second, there is, as we take it, somewhat augmented irritability; in the third, diminished irritability, or commencing exhaustion; and in the fourth, exhaustion carried to a further degree.

It appears, however, probable that many of the positions hitherto considered to be established with regard to the irritability of nerves will be found to require considerable modification. Eckhard has recently shown that if a constant current is transmitted upwards through a motor nerve (i.e., inversely), that the irritability of the whole nerve is diminished; but that if it is directed downwards (i.e., directly) through a portion of the nerve-trunk, diminution of irritability is found only in those parts through which, and above which, the current passes, while below the negative electrode the irritability is augmented.†

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* Leçons cit. p. 183,

† Schmidt's Jahrbücher, 1857, ii. p. 966.

Another difference of effect, in reality dependent upon the part of the organism to which electricity is applied, but practically determined by the mode or locality of administration, is that which Duchenne pointed out between what he termed "direct and indirect faradisation." This Remak and Ziemssen agree in referring to the excitation of the muscular nerves, without or within the muscles themselves, and which Remak terms "extra and intra-muscular galvanisation." Ziemssen further shows that the value of extra-muscular irritation is this, that a feeble current will cause a whole muscle to contract, whereas with intra-muscular excitation a much more powerful current is required. (p. 6.)

Much that has been said and written upon the subject of modern or "localised galvanism," as compared with the older method of applying that agent, rests simply upon the fact that whereas, years ago, the different tissues (skin, muscles, and nerves) were indiscriminately affected, now the action of electricity can be limited with considerable precision to either one of them. This we owe in great measure to Duchenne,[*] who discovered, clinically, that there were certain points of the surface, applied to which the electric currents acted more vigorously than when directed upon other points. Further experience has shown that, in the main, Duchenne was right; but a somewhat curious and unworthy controversy has arisen as to whom the credit belongs for having systematised and explained these facts.

Dr. Robert Remak[†] claims for himself the discovery that these "boasted points of election" (*diese berühmten Wahlpunkte*) are simply those at which the muscular nerves make their entrance. But Duchenne replies,[‡] that he had already, in 1852, exhibited his *modus operandi*, and the effects thereof, to Dr. Remak, and that he had deemed it quite unnecessary to give an anatomical dissertation to those distinguished men who attended his demonstrations, and among whom was Dr. Remak himself; and that now, instead of a great discovery having been made by his former visitor, and present critic, all that he can suppose is that in 1852 Dr. Remak was wanting in that anatomical knowledge which in 1855 he appears to have acquired.

Notwithstanding this reply by Duchenne, so lately as May, 1858,[§] Remak thus describes his share in the investigation :—

> "I was not a little curious to know the nature of these mystical points, and on directing my attention to this subject I soon found that they corresponded with the points of entrance of the muscular nerves, and that the degree of contraction of a muscle was proportioned exactly to the number of motory nerve-fibres embraced by the current at its point of application."

The truth of the matter appears to be that the practical discovery was Duchenne's; and that, whether or not he understood his own discovery, he did not distinctly explain it. Remak accomplished this part of the process, and gave a theory to account for the result; and this theory has, we think, been most ably proved to be correct by the laborious investigation of Ziemssen, who conducted two series of inquiries, in one of which he determined clinically the precise localities of these "points of election," and marked them upon the skin; in the other he examined post-mortem the course of the nerves, especially their motor branches, and noted accurately their points of entrance into the muscles; and, upon subsequent comparison of the two series, he found that they agreed perfectly with each other. (p. 3.)

In Dr. Lawrance's book some useful practical directions are given with regard to the points through which certain nerves and muscles may be reached most readily. (pp. 62 et seq.)

Ziemssen has also added to our definite knowledge of the influence exerted by the organism upon electric application two further facts, one that the conductibility of the tissues is in direct proportion to the quantity of water they contain (p. 39), and the other that the central organs of the nervous system, as well as nerve-branches in the large natural cavities of the body, escape the electric current on account of their envelopment in good conductors. He asserts, however, that with an extremely powerful current, these organs may be reached. (p. 12.)

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[*] Brit. and For. Med.-Chir. Rev., No. xxix.

[†] Ueber methodische Electrisirung gelähmter Muskeln. Berlin, 1855.

[‡] Schmidt's Jahrb. 1846. Bd. 89, p. 250.

[§] Medical Times and Gazette, No. 410, p. 479.

The latter point has been confirmed by M. F. Bonnefin,* who concludes from carefully conducted experiments, that it is possible to pass an electric current through the nervous centres, but that this current is always very feeble, even when a very powerful electro-magnetic apparatus is employed. He infers, further, that the influence exercised by those electro-magnetic currents which produce movements due to excitation of the spinal cord, only take place through the intervention of sensitive or excito-motor nerves. And hence, that in order to act upon the nervous centres, it is necessary to employ metallic conductors (and not moist sponges), so as to affect powerfully the cutaneous nerves. (p. 548.)

In regard of the theory or explanation of these diverse physiological effects of galvanism, but little that is satisfactory has been proposed. Du Bois Reymond, Matteucci, Dr. Radcliffe and others, starting from another ground, viz., the galvanic properties of the living tissues, have endeavoured to supply a rationale for many of the phenomena. Dr. Radcliffe says truly, concerning "the action of the ordinary galvanic current upon muscle, it is to be expected that the existence of the muscular current is not to be ignored," and he arrives at the conclusion that "it is difficult, if not impossible, to find any reason for supposing that the contractions (of a muscle) are due to any direct action of the current, natural or artificial" . . . . but that on the contrary "there appears to be only one course open, and that is, to connect the contraction with the *absence* of the current." (p. 36.)

The main facts upon which this opinion is based are these, 1, that during rest there is a galvanic current in the muscle; and 2, that during muscular contraction this current is weakened, or reduced to zero. This reduction takes place when the muscle is tetanised by an artificial current, and Dr. Radcliffe says, "there is no difficulty in connecting the contractions with that clashing and mutual neutralization of the muscular and artificial current" which takes place when the former is supplanted by the latter.

There is, according to Dr. Radcliffe, a "*moment of inaction*" between the disappearance of the one (muscular current) and the establishment of the other (artificial); and the whole tenor of his argument is to this effect, that the muscle contracts by virtue of its own inherent property; but that its contraction, instead of being stimulated or occasioned by galvanism, is prevented from occurring by the presence of that agent. The "moment of inaction," galvanically, is the moment of action for the muscle.

The argument so far amounts to this,—because A disappears when B is produced, the disappearance of A is a condition for the production of B. Or, because the several prismatic colours not only disappear from the surface of a card when it is rotated rapidly, but reappear when the card is brought to rest, therefore the disappearance of the colours is a condition which allows the rotation to take place. Ingeniously as Dr. Radcliffe has developed his theory, and partially met numerous objections, we regard the balance of the whole evidence as opposed to the view he entertains. There is the radical error of mistaking for a causative condition of a certain phenomenon that which may be more justly regarded as its effect; and there are many circumstances attending muscular action which are not fully met by the hypothesis. Thus, the force of muscular contraction is in proportion to the strength of the stimulus which is brought to bear upon it; whether this is galvanic, chemic, mechanical, or vital; and if contraction is to be referred to the absence of the muscular current, there are causes of contraction—such as pinching, percussion, and irritation with a non-conducting body—which cannot be shown to operate by producing this removal. Again, when we have on the one side a current so feeble that it requires an apparatus as delicate as that of Du Bois Reymond to demonstrate its existence; on the other, a current so powerful as to produce violent contraction of the muscles; and we advance to the idea of their bellicose "clashing" on the blood-stained field of a few muscular fibres, we cannot but think that the chances are very much against the result being a "drawn-battle;" a mere "neutralisation," and "moment of inaction," during which, and as the result of which, this most remarkable phenomenon of contraction occurs. The experiments of Weber, Pflüger, and Lister, show that entirely different effects are produced by currents differing only in intensity, and we must confess that in the present state of physiological science there is yet wanting a satisfactory rationale of the phenomena.

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* Recherches Exp. sur la possibilité du passage à travers le Centre Nerveux de Courants El-Magn. appliqués à la peau, chez l'Homme. Journal de la Physiologie. Par E. Brown-Séquard. tom. i. No. III.

For a perfectly successful therapeutic exhibition of electricity, such a comprehensive knowledge of the *modus operandi* of the agent, physiologically, is required; but while this is still a desideratum, much may be and has been done towards solving the problem of its practical utility. Attempts have been made to relieve many morbid conditions, and these, though sometimes directed by a true deduction, sometimes by *à priori* considerations, and at other times by simple empiricism, have met with varied, but in the main increasing success; and it is to these attempts and their results that we would now direct attention.

There are four principal results which electricity may be called upon to effect: 1, restoration of contractility in the muscles; 2, re-establishment of sensibility; 3, reduction of augmented contractility; and 4, diminution of hyperæsthesia.

1. *Treatment of Paralysis.* Becquerel states that when this is dependent upon a persistent lesion of the brain, spinal cord, or nerves, electricity is "tout-à-fait contre-indiquée, elle ne pourrait qu' être nuisible." (p. 125.) This is substantially the same as Duchenne's statement made many years ago; but M. Becquerel goes on to affirm that when the central lesion is cured, "et que le diagnostic permet d'établir que cette cicatrisation s'est ôperée" (a nice point for diagnosis!), then electricity may be employed with advantage. When cerebral hæmorrhage has been the cause of paralysis, the experience of M. Becquerel is, that in the "immense majority of cases" electricity exerts no favourable influence. (p. 138.)

It appears, as the result of experience in the treatment of paralysis from cerebral hæmorrhage, (*a*) that nothing electric ought to be attempted for many months after the attack; (*b*) that a certain proportion of cases get quite well at the end of that time without any treatment of this kind; (*c*) that others at that period present paralysis to the will, but the irritability of muscles to electricity persists; and in such cases the application of electricity is not wanted, does no good, and sometimes is mischievous in its results; and (*d*) that in other cases the electric irritability is diminished, and here electric treatment is of use.

M. Becquerel agrees with Drs. Marshall Hall and Duchenne, that in the great majority of cases the contractility of the muscles persists; but he states that, in old paralysis, the "prolonged inaction of the muscles almost always diminishes" that property. According to our own experience the electric contractility of the muscles is more frequently diminished than either increased or unaffected; and further, this diminution has not appeared related to the length of time during which the paralysis has lasted.

With regard to paralysis dependent upon cerebral softening, M. Becquerel states that the contractility is preserved in recent cases, but lost in those of longer standing. He gives no results of practical experience on the matter, but asserts that the utility or uselessness of galvanism depends upon a recognition of the persistence or "cicatrisation" of the softening! When paralysis depends upon the existence of an intra-cranial tumour, M. Becquerel says that galvanism should be rejected altogether. (p. 146.)

When paralysis is caused by an injury or disease of the spinal cord, there may or may not be loss of muscular contractility. The original statement of Dr. Marshall Hall upon this question has been, in reality, confirmed by every subsequent observer. It is, that when the muscle is functionally separated from the cord, there is diminution and loss of irritability; and this is Dr. Hall's "spinal paralysis." But a disease in the spinal cord, although it may cause paralysis,—i.e., separation of the muscles from the will ("cerebral paralysis")—need not, and often does not, sever the functional relationship of those muscles and the cord itself. Such cases Dr. Hall did not term "spinal paralysis," but "cerebral."* If a disease destroys a portion of the medulla, the muscles supplied by nerves coming from that disintegrated portion present "spinal paralysis" and loss of irritability; but those muscles which are supplied by nerves arising from that portion of the cord which remains uninjured below the lesion, present only "cerebral paralysis," and retain their irritability. Nothing appears more distinct than Dr. Hall's statements upon this question; and it is not less distinct that their truthfulness has been abundantly confirmed even by those modern electricians who, while recognising the facts, seem, curiously enough, but almost universally, to mistake the meaning of Dr. Hall.

In a former article in this journal a true statement of the case was made, and we should not again have referred to the matter, had not the misstatement been recently reiterated both in England and France. M. Becquerel does not escape the error, but he gives some further information on the subject, in affirming that the degree of electric irritability which persists is in direct proportion to that of the capacity for volitional exercise. (p. 152.) Further, that the treatment of paraplegia by electricity is positively injurious when the paralysis is progressing, and is of service only when this symptom is either stationary or diminishing. In complete paraplegia with irritability diminished, electricity does no good, and it is useful only in those cases of incomplete paralysis in which there is no diminution of irritability. (p. 155.)

In "traumatic paralysis," or true "spinal paralysis," electricity is sometimes of service. Professor Oré relates a case of cure of facial paralysis of eight and a half years' duration, in which all muscular irritability was lost.† In our own practice we have seen notable improvement of a case of facial paralysis after fourteen years' duration; but in this case, although the distortion of features and lagophthalmia were extreme, some slight electric contractility remained.

Duchenne states that, in cases of traumatic paralysis it is sometimes observed that volitional power remains partially while electric contractility is destroyed. But this M. Becquerel appears to doubt (p. 159), stating that, in a great number of cases, he has observed that the two properties "marchent ensemble." When there is not a complete loss of both, there may be one of the two following conditions:—1, voluntary power lost completely; sensibility diminished or not; electric contractility intact or diminished; 2, voluntary power incompletely lost; sensibility diminished or not; electric contractility intact or diminished. The first class of cases is curable, but the second curable much more readily. With regard to hysteric paralyses there is nothing new; they are well known to be amenable to this kind of treatment, and M. Becquerel says truly, "le traitement de ces affections a fait la fortune de plus d'un electriseur!" (p. 175.)

The paralyses which is symptomatic of such genito-urinary lesions as nephritis, calculus, stricture of the urethra; and which persists after the removal of its remote cause, may be rapidly cured by electricity. The paralysis remaining after the inaction of a limb from rheumatism, may likewise be treated successfully in the same manner; and M. Becquerel describes an essential or idiopathic paralysis, which is amenable to this agent. The characters of this form of paralysis are, A, positive;—paraplegia, complete or not; anæsthesia, or not; digestive and urinary functions natural; other "nervous" phenomena present; embonpoint normal; contractility completely preserved. B, negative;—no spinal pain; no feeling of cord round the trunk; no tonic contraction; no paralytic affection of bladder or rectum.‡ A case somewhat resembling this kind of paralysis was successfully treated by Dr. Althaus.§

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* Medico-Chir. Transactions, vol. xxx. p. 207.

† Quoted from Journal de Bord. Avril, 1856, in Schmidt's Jahrb., 1856. vol. iv. p. 55.

‡ Becquerel, p. 181.

§ Med. Times and Gazette, December 26, 1857, p. 656.

In lead palsy, M. Becquerel states, in opposition to Duchenne, that the contractility is not lost unless the palsy has come on slowly, and there is atrophy of the muscles; and further, the contractility which remains is in direct proportion to the voluntary power remaining. (p. 190.)

Writer's cramp may be completely cured.[*]

Intestinal atony has appeared in some instances to be overcome by galvanism; and notwithstanding the peculiar inhibiting influence of the splanchnic nerves it seems probable, from Ziemssen's observations, that this agent may be hereafter usefully employed upon the intestines. The latter observer states that powerful contractions of the intestinal walls may be produced, and that these persist after removal of the electrodes, and the only pain so caused is in the skin at the point of contact—"*diese äusserst stürmischen Actionen*"—continued for one quarter of an hour—"*aber ganz schmerzlos.*" (p. 13.) But as for the action of the galvanic current upon the bowels, Becquerel gives his experience thus: after placing one pole in the mouth, and another in the rectum, and trying all sorts and directions of currents, "je n'ai jamais obtenu aucun résultat." (p. 202.)

Duchenne has cured three cases of prolapsus ani; and M. Stacquez[†] relates cases of impotence cured by powerful shocks from a Leyden phial, passed from the lower part of the vertebral column to the tip of the penis—"de manière à produire l'explosion à ce dernier point."[‡] The individual who would submit to this operation could not be impotent, we should think, through "want of nerve."

In "wasting palsy," or progressive muscular atrophy,

> "The most effective remedy," says Mr. Roberts,[§] "is galvanism applied locally to the wasting muscles . . . . It has generally, indeed nearly always, been found to yield encouraging results: too often the amendment has been but temporary, but in several instances it has brought about arrest, and in a few, re-establishment of the bulk and power of the wasted muscles, either wholly or in part."

In a case of this kind, however, under the care of Dr. Hare, galvanism was applied twice daily, from the 20th July to the 24th August, the result being that the patient is described as "not deriving much benefit from this treatment."[‖] And in a case under our own care, four years ago, localised galvanism was negative in its effects.

2. *Treatment of Anæsthesia.* Not much that is of value has been added to our information on this subject. When an anæsthesia depends upon some general morbid condition, such as, for example, amaurosis in Bright's disease, electricity can accomplish nothing. When it is but the symptom of a distinct organic lesion, intra-cranial tumour, hæmorrhage, &c., there is the same negative result. But when the anæsthesia is idiopathic or essential, then, say the electricians, much may be accomplished. In such cases, however, the phenomenon frequently disappears with some general alterative treatment, or without any treatment at all.

With regard to analgesia, which may be regarded rather as a boon than a misfortune, M. Becquerel makes the very sensible remark that, "s'il y a anelgésie seule on la laissera parfaitement tranquille." (p. 220.)

3. *Treatment of Spasm, or augmented muscular contractility generally.* With regard to convulsions and contractions, M. Becquerel states that, notwithstanding the physiological and pathological basis for the employment of electricity, he has failed to obtain a cure. Others have been more successful. Dr. Remak states as follows:—

> "I was induced in July, 1856, to apply the constant current as a means of treatment of contractions of muscles, in cases of hemiplegia from cerebral apoplexy. The most important result of this application was the fact, that the continued current, applied for a few minutes to a contracted muscle, had the effect of immediately relaxing it to a certain extent, and rendering it amenable to the influence of volition."[¶]

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[*] Becquerel and Remak.

[†] Archives Belges de Méd. Militaire, 1849.

[‡] Becquerel, p 81.

[§] Essay on Wasting Palsy, 1858, p. 204.

[‖] Med. Times and Gazette, April 24, 1858, p. 426.

[¶] Ueber die Lösung paralytischer Contracturen. Deutsche Klinik, 1856, No. 28. Med. Times and Gazette, May 8 1858, p. 479.

Another mode of reducing permanent contraction of the muscles is that commonly employed by Duchenne, viz., the excitation of those muscles which are their natural antagonists. By this method Duchenne treated three cases of torticollis, but although there was improvement there was no cure.

The curability of cases of tonic contraction, when this has been of long duration, appears to us at best to be extremely doubtful; for in such cases there is reason to suspect the existence of a central lesion, which electricity has not yet been shown to be capable of affecting, even though it may temporarily suspend or counteract the symptom. And further, these tonic contractions, when produced artificially, are among the most persistent results of central injury, even when the latter has been of very trifling extent.

4. *Treatment of Hyperæsthesia.* Of late the activity of the professional mind has been especially directed towards the cure of neuralgia, or the artificial production of anæsthesia by means of electricity. In regard of the former there has been considerable success. Dr. Hiffelsheim presented to the Academy of Sciences a paper detailing the results of his experience in thirty-six cases in the wards of M. Rayer, and these are highly satisfactory.*

Dr. Althaus has published some of the results of his own experience, which are also satisfactory.† And M. Becquerel speaks with confidence of the good effects of galvanism in cases of this kind. But the utility of galvanism as an anæsthetic agent has yet to be shown. Dr. B. W. Richardson performed careful and painful experiments upon himself, but the result at which he arrived was "that the electric current cannot, according to our present knowledge of its application, be made practicable for the production of local anæsthesia."‡

To him replied Dr. Althaus, that when the current is applied in a different manner, "the sensibility is notably diminished."§ But Dr. Althaus further states that "the result is much more striking if there is a morbid increase of sensibility in a nerve, as in neuralgia, than if a nerve in its normal state is acted upon." And in the same number of the journal is a letter from Mr. Harry W. Lobb, which reminds us somewhat of the character of proceeding adopted by those individuals who present themselves "where angels fear to tread;" for Mr. Lobb, after stating with regard to electricity "I have never used it to prevent pain during the extraction of teeth," displays his qualifications for being a scientific observer by going on to say, "but from what I know of its success in toothache, the following plan will, no doubt, be found perfectly successful. Procure a 60-link . . . . &c., &c."‖

Mr. Eden, of Brighton, did "procure a 60-link &c. &c.," and "follow minutely the directions in Mr. Lobb's letter," but was "sorry to say" that he "did not obtain any diminution of sensibility."¶

Thus, then, stands the question of "Electrical Anæsthesia." It appears that hyperæsthesia may be reduced; and even that the normal sensibility may be diminished; but there is no evidence that the reduction can be carried so far as to render electricity a useful anæsthetic agent.

There are two modes in which electricity operates in obtaining this therapeutic effect, first, by direct reduction of the nervous sensibility, and secondly, by counter-irritation of the skin. M. Brown Séquard refers the beneficial results of electrical or other irritation of the skin, in the treatment of neuralgia, to a reflex action upon the vessels of the irritated nerve, producing temporary anæmia therein.

The other uses to which electricity has been applied, such as the treatment of amenorrhœa, of atrophy, and of aneurism, and its employment as a cautery for the removal of tumours, or for other purposes in which the actual cautery is required, need no comment now, as nothing of much value has been added to our knowledge upon the question.

But the mode in which electricity is applied has probably much, if not everything, to do with the beneficial or other results which follow. In regard of paralysis, it appears quite clear that the continuous current is of great practical utility; and the interrupted current of induction is also serviceable. Hiffelsheim is, we think, correct in saying that the physiological and therapeutical effects of the permanent continuous current are "not obtained by the contractions;" and in his further observation we entirely agree, viz., that "this might perhaps lead to the supposition that the interrupted current does not, any more than the continuous, act *immediately* by contraction; and that in both cases dynamic electricity acts directly on the different elementary acts of the complicated function called nutrition."*

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* Extract from the Minutes of the Meetings of the Academy, vol. xlvi.
† Med. Times and Gazette, Aug. 14, 1858.
‡ Ibid., Sept. 11, 1858. § Ibid., Sept. 18, 1858. ‖ Ibid., Sept. 18, 1858. ¶ Ibid., Oct. 9, 1858.

Practically, the application of either current should, in cerebral paralysis, be limited to the muscles; the electrodes being placed at short distances, and the intermissions (when an intermittent current is employed) should be rapid. But when the attempt is made to affect the muscles through the agency of their nerves, those special points must be selected which Duchenne was the first to point out. A most minute description, and careful delineation, by well-executed drawings of these points, will be found in the work of Ziemssen; and for practical information on the electric anatomy of man, it will prove more useful than any other treatise on the subject.

Where the object of electrical application is to awaken sensibility or induce certain movements in an individual intoxicated with alcohol, or poisoned by opium, the interrupted current of high intensity is required; but for the treatment of hyperæsthesia, either the interrupted or the continuous may be employed, and with an equal measure of success.

Thus, except in the particular instance referred to, the therapeutic effects of galvanism may be obtained from the one form of application as well as from the other; but inasmuch as the continuous current is free from pain, and can be so applied as to avoid all undue calorification, thus rendering it free from the injurious effects sometimes following an application of the interrupted, it possesses the decided advantage, and will, we believe, eventually be employed almost exclusively.

As to the apparatus to be used, but few words are necessary. The intermittent induced current may be obtained from the electro-magnetic, or the magneto-electric arrangement. In the one there is the trouble and frequent inconvenience of employing "exciting" fluids; in the other there is the necessity for rotation of the temporary magnet; but the latter will be found to be the lesser evil. When a continuous current is required, there is nothing which can equal the elegant chain-battery of M. Pulvermacher.

As a thoroughly successful application of electricity can be hoped for only by a comprehensive and profound knowledge of its physiological effects; the great desideratum of the present time is the acquisition of this knowledge. An agent which appears capable of inducing, increasing, reducing, or destroying the functions of both muscles and nerves, ought not to be employed without extensive information and careful adaptation to the exigencies of the several cases to which it is applied; and until it can be so exhibited its success will be partial and accidental only. What is required, then, is the most cautious experiment and logical induction; for by this both the science of physiology and the art of therapeutics will advance; while they can but retrograde and become the object of contempt, if a few accidental successes are made the basis of an advertised panacea.

While, in the present day, there are a few who are thus intent upon the progress of that which may prove a great boon to suffering humanity, and whose recommendation of electricity is always guided by a definite knowledge of the effects which it is already known to produce; there are the many whose careless employment of the agent in all kinds of maladies resembles rather the prescriptions of the middle ages, embracing every kind of material—from man's skull to sparrows' dung, and from diamond dust to copper filings—in the hope that some one of them might be of use to the sufferer, and that the others might mutually counteract their several injurious effects.

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* Extract from the Min. of the Meetings of the Acad. of Sciences, Paris. vol. xlvi.

Review IX.

1. *The British Army in India: its Preservation by an appropriate Clothing, Housing, Locating, Recreative Employment, and Hopeful Encouragement of the Troops; with an Appendix on India.* By Julius Jeffreys, F.R.S., formerly Staff-Surgeon at Cawnpore, and Civil Surgeon of Futtegurh.—*London*, 1858. pp. 393.
2. *A Brief Review of the Means of Preserving the Health of European Soldiers in India.* Part I. By Norman Chevers, M.D., Bengal Medical Service.—*Calcutta*, 1858. 8vo, pp. 131. Reprint from "Indian Annals of Medical Science," July, 1858, p. 632 to 762.

The author of the former of these works, during a lengthened period of service in India, was distinguished for the great attention which he bestowed upon every question which related to the health of the British soldier, for his proficiency in physical science, and for a vast amount of innate mechanical ingenuity. In this country he is better known as the inventor of the "Respirator," which bears his name, and which is in universal use. The volume before us, though written in a style somewhat quaint, is full of most original views and suggestions, and well deserves the serious attention of all who are interested in the preservation in health and strength of our armies in India. We proceed to give a brief epitome of its contents in the order followed by the author:—

I. *On the Specific Properties of matter through which heat operates and is to be resisted.*—It is argued that the example of the natives of India is an uncertain guide in dress. *Slow Conduction* is the principle chiefly relied on by them for protection from heat, and this also is the principle on which Europeans wear flannel cap covers, and hats constructed of "*Sola* pith." Slow conduction, however, though a principle of high value in conjunction with others, is shown to be insufficient in itself for warding off heat. *Evaporation* is another principle, by means of which the effects of heat may be averted, yet it is a power which cannot be placed under safe control, nor be always at command when most needed, as on a line of march. The two principles of heat, the protective power of which has not been employed by the natives, nor sufficiently recognised by Europeans, and upon which the author places the greatest reliance, are those of *Reflection* and *Slow Radiation*. Both these principles are possessed in an eminent degree by bright metallic surfaces, a more general adoption of which in the clothing of our troops is earnestly recommended. Some remarks follow on the *Convection* and *Subterranean Absorption* of heat, having more especial reference to the construction of dwellings.

II. *General Remarks on Tropical Virulence and Vital Resistance.*—According to Mr. Jeffreys, the three enemies with which the British soldier in India has to contend, are the sun, the atmosphere, and the ground. He believes that the direct rays of the sun not only produce sun-stroke, but also generate fevers, dysentery, and acute internal inflammations, while at the same time, when long continued, that they induce such a debility of the cutaneous system, as to predispose to the influence of malaria. "The skin's debility is malaria's opportunity." The exemption of the natives from the bad effects of the sun is shown to depend upon the extreme thickness of their scarf-skin. The great error also is pointed out of attempting to acclimatize an Englishman by exposure, and examples are given to show the baneful results of such an experiment. The immunity of the Indian has been the work of many generations: in endeavouring to acclimatize an Englishman, the work of many generations is attempted to be effected at once. The ordinary caps and helmets supplied to our troops are described as "vapour tight boxes," preventing all transpiration.

> "What then is to be said of coverings for the head, which not only let in solar heat to a large extent, but, as if they were intended as instruments of *capital* punishment, lock up the only door of escape by smothering the scalp altogether, or tantalizing it with a few ventilating holes of the size of a pin, or at most of a pencil, and at the crown only. The marvel is, how any warm-blooded animal should, with the head in such a predicament, escape apoplectic destruction, even though it were of the coolest temperament, emasculated and bovine! But that manly brains,—bulky and busy,—turgid with blood and with thoughts of blood!—with the spirits on fire and excited to combat!—should endure for an hour the double culinary process of roasting from without, and stewing within, is what no reflecting physiologist could have anticipated!"

The injurious effects of a heated atmosphere, apart from the direct rays of the sun, are next dwelt upon. Such an atmosphere reduces the tone of the cutaneous system, while at the same time, by failing to dissipate the sun's direct rays, it aggravates the evils which these are wont to inflict.

Mr. Jeffreys does not enter into the question of the mode of origin of malaria, but he observes, that the *ground* being a poor conductor, radiates and reflects heat upon the body of the soldier, and so materially increases the direct action of the sun from above.

III. *The Soldier's Dress.*—The head-dress is first considered:—

> "A head dress, to be effective, should possess such resisting power as to ward off entirely the whole rays of the sun throughout an exposure to its action of any duration; and not only from the skull, but also from the sides of the head, face and neck. It ought also to transmit so copious a ventilation over the head, as to encourage the perspiration to evaporate freely from it; and yet with a provision by which in cold weather the circulation of the air could be at once reduced or cut off. At the same time, such a head-dress should be no more cumbrous than was necessary to fulfil all those conditions completely."

Mr. Jeffreys gives several plans and descriptions of head-dresses combining all the properties which he recommends. The most complete provision is made for ventilation, while thin metallic plates are so arranged as to reflect the heat both from the exterior and from the surfaces of the ventilating spaces, and at the same time cause the little that is transmitted to be but slowly radiated in the direction of the head. Conduction is also called into play, by means of a layer of cotton wadding three-eighths of an inch thick, so as to transmit tardily whatever heat has not been intercepted by the reflection, slow radiation, and ventilation.

> "This principle is not placed in the van, but is introduced as a powerful reserve to retard the progress of rays which have broken through all the previous barriers; against which, such fibrous matter is effective, whereas it can offer but poor resistance to the solar rays, if it is so placed as to receive the brunt of them direct."

The fabric next the head is of wool, so arranged as to permit the perspiration to be exhaled freely, and so produce cold by evaporation. There is also an arrangement whereby the effects of evaporation may be increased by moistening this wool with water from without.

Mr. Jeffreys has ascertained that helmets may be constructed according to his plan, not weighing more than from two to two-and-three-quarter pounds. This is no doubt heavier than many of the shakos at present in use, the weight of which is a frequent subject of complaint; yet the oppressiveness of the ordinary head-dress is attributable not so much to its *weight*, as is usually imagined, as to its imperfect poising, its pressure, and its want of porosity. Nothing is said as to the expense at which the author's head-dresses might be constructed. Their shapes, also, are certainly not very elegant; and we fear that our military authorities will not "crucify fashion" to the extent which Mr. Jeffreys would desire. Yet their mode of construction, the principles on which this is founded, and the advantages to be derived from them, are deserving of serious attention.

As regards the *body dress* of the British soldier in India, it is argued that a thin dress is ill suited for much exposure to a hot atmosphere, and that a barrier between this and the skin is required. For this purpose, flannel "next to and enclosing the whole skin" is recommended, and in this recommendation we entirely concur. Flannel, being a non-conductor, wards off the excessive heat from without, while at the same time it maintains the temperature of the body within and prevents chills, and being also porous, it absorbs the perspiration and allows it to pass outwards readily, while it permits the ingress of atmospheric air. Those going to India should never fail to provide themselves with this useful material. Thick external clothing, during the hot months, is denounced, but in the cold season soldiers are recommended to be clad so as to be comfortably warm. The following sentence is well worthy of attention:

> "Many soldiers and officers likewise think that because they are in India, they cannot suffer by any carelessness with respect to cold, and many, as a consequence, bring themselves upon the sick list, and with ailments often of a tedious character."

There can be no doubt that sudden chills are quite as frequently the exciting causes of inflammatory diseases in the tropics as in temperate regions. The previous heat diminishes the resisting powers of the skin, and often induces copious perspiration, and the rapid evaporation of the perspiration itself, produced by a current of air, is not rarely the source of the chill.

Some remarks follow on a *Sun screen tunic*, the great principle in the construction of which is, that it should be of such material as to reflect off as many as possible of the rays of the Sun. Here again bright metallic surfaces are called into play, and the author appears to have succeeded in manufacturing a flexible India-rubber cloth, with such a surface as to answer his purpose. For the details of the construction of the Sun screen tunic, as well as of the helmets, we must refer to the original work.

IV. *On the Housing of British Troops in India.*—Many original and valuable suggestions will be found under this head, which it is hoped will not be lost sight of in the erection of new barracks in India. A few of these suggestions we shall allude to. The ill effects which may accrue from lodging Europeans upon the ground floor are pointed out, as well as the importance of all the barracks and houses of the officers being furnished with a double roof. The great importance of thorough ventilation is insisted upon, and the advantages and disadvantages of the means already in use, particularly of the "tattees" (wet matting suspended over the door-ways), are discussed. An ingenious mode of ventilating and cooling dwellings, as well as of heating them in cold weather, is proposed, which is derived from the circumstance that the earth, some feet below the surface, is cooler than the atmosphere in summer, warmer in winter. It is recommended to sink in the neighbourhood of a dwelling a number of deep wells, and to connect these by cross tunnels, so as to form a lengthened subterranean passage, one end of the series of these passages opening on the ground floor inside the house, and the other, which communicates with the air, being furnished with a large cowl or wind-sail, which will turn in any direction in which the wind blows. A constant subterranean current of air discharging itself in the basement floor of the house will thus be secured. Various observations on "thermantidotes" and "ventilating machines" follow, and a "refrigerator" of the author's own invention is described and figured.

V. *On the Locating of British Troops in India.*—Mr. Jeffreys dwells with much earnestness upon the necessity of establishing sanitary stations in India, and shows that he was an advocate for them so long ago as 1824. He agrees with Mr. Ranald Martin in thinking, that the proportion of European stations in the hills should be increased, and does not consider that the experience of the recent rebellion is unfavourable to such an arrangement, while the extension of railway communication would render the transfer of troops to any seat of disturbance at all times easy. The use of tramways in the construction of Indian roads is also recommended. Ravines near stations are pointed out as objectionable, being certain to become the rendezvous of the natives for depositing that which more refined communities carry off by sewers. The air, laden with putrid emanations, becomes stagnant in these ravines during the calm hot day, but as night advances, it becomes displaced by a colder and denser atmosphere from an adjacent cultivated plain, and is wafted through the open doors and windows of any houses lying to the leeward. A high river bank with land under cultivation, is spoken of as an eligible site for a station. Mr. Jeffreys dwells with much force on the moral necessity which devolves upon the Government for rearing the child of the European soldier in a healthy climate, either in Europe or in the hill stations of India. We entirely agree with him. The awful mortality among the children of English soldiers in India should long ago have suggested some such procedure.

VI. *On the Recreative Employment of the Soldier.*—None who have not experienced it for themselves can form any conception of the frightful *ennui* of a soldier's life in India during peace. This it is, and not the intensity of the heat, nor the frequency of disease and death, which renders life a burden often so difficult to bear. Any remedies for this state will prove one of the greatest blessings to the soldier, while at the same time they will operate as the most certain prophylactics against disease and intemperance. The Indian Government cannot be said to have been altogether remiss on this score. Regimental libraries and racket-courts are almost universal at the European stations. These provisions are, however, inadequate. Many of our soldiers are unable to read, while the racket-courts afford no protection from the sun, and the game of rackets is too fatiguing and trying to the constitution in India. Mr. Jeffreys points out that many of the soldiers have been originally artisans, and proposes to establish workshops for turning, punching, and other purposes. The formation of experimental farms would also afford occupation to many, and would be one of the most certain ways for developing the natural resources of the country. The profits from these various sources might be employed in defraying the necessary expenses, while the surplus might accumulate as an addition to the soldiers' future pensions. Few English soldiers would sit idle with such opportunities for action before them. Golf and billiards are spoken of as suitable games in preference to rackets.

VII. *On the hopeful encouragement of British soldiers serving in India.*—Under this head we find some suggestions which are perhaps more desirable than practicable. According to Mr. Jeffreys, the English soldier ought not to be kept in India against his inclination:

> "Military service in the ranks in India can never be in a just and safe position until its fundamental condition shall be an engagement, in the name of India, that the recruit shall be replaced at the home from which he was invited out, should he at the end of a year or two's trial find the service unacceptable or disappointing to him. Until this is conceded, it is mockery, and something worse, to tell him that he entered upon the engagement voluntarily with his eyes open."

It is also recommended that after a service of twenty years he should be entitled to a pension of 40*l.* or 50*l.* a year.

Mr. Jeffreys is also a strong advocate for introducing African troops into India, as a measure calculated to diminish the present enormous expenditure of British life.

The appendix contains much and varied and useful information upon subjects pertaining to the social and political economy of British India, and is well deserving of an attentive perusal.

The brief sketch which we have given will afford some idea of the numerous and varied topics discussed by the author. It is to be hoped that amidst the many changes and reforms which will soon be introduced into British India by its new rulers, Mr. Jeffreys' valuable suggestions will receive the attention which they deserve.

Dr. Norman Chevers, although comparatively a young officer, has long held a distinguished position in the service to which he belongs. The work before us is the result of great labour and research, and contains much valuable statistical information bearing upon the sanitary condition of the European soldier in India. His remarks have reference to the following main points of inquiry:—1. The amount of mortality and sickness among our troops; 2, the causes upon which this destruction of health and life depends; and 3, the modes of checking or removing those causes.

Since the commencement of the present century the average annual rate of mortality (in *hospital*), from all causes, among the men of H.M. and H.E.I.C.'s European forces in the three presidencies of India, has been 62.45 in the thousand, which is nearly twice as heavy as that which obtains among the general population of Liverpool—

"England's most unhealthy city"—this being only 33·5 per 1000. Of the three presidencies, Bengal occupies an undue pre-eminence in its high rate of mortality, but this circumstance is mainly attributed to the greater exposure of the Bengal army to the dangers and vicissitudes of war, and to the frequent postings of corps on the Bengal establishment in new stations, which are generally considered as highly inimical to the health of European soldiers. The mortality among the officers in India is about one-third of the general mortality above quoted, and is considerably less among the married than among the unmarried. High as this mortality undoubtedly is, it contrasts very favourably with that which obtains among European officers serving in other tropical stations, and more especially in the West India islands. The mortality among the wives of European soldiers in India is 35·47 per 1000, and here again it is greatest in the Bengal presidency; that of the wives of European officers appears wonderfully small, or only 15·85 per 1000. The mortality among European children in India is shown to be enormous. That of children under fifteen years of age in the Lower Orphan School, Calcutta, for a period of forty years, was 54·85 per 1000; and among male infants during the first year of life, it reached the rate of 171·84 per 1000 (the corresponding rate in England being only 71·65). As a remedy for this frightful state of things the author's recommendation is in perfect accordance with that of Mr. Jeffreys.

> "Early removal to the hills appears to be the most certain safeguard. The first reports of that admirable institution, the Lawrence Asylum—established at Sunawur, near Kussowlie, in 1847, by the philanthropy of the late Sir Henry Lawrence, for the reception of soldiers' children, chiefly orphans, show that the asylum then had, at the end of its second year's operations, 136 inmates, nearly half of whom were under ten years of age; and that only two deaths had occurred from the first, in children who had been only 'a week in the asylum, and who arrived in a state of disease.'"

As regards the rate of sickness in the European army in India, it is shown that the number of admissions into hospital during the year exceeds 2000 per 1000 of strength, or in other words that, on an average, every soldier is admitted into hospital twice during the year. The diseases which produce this great sickness and mortality are fevers, dysentery, diarrhœa, hepatic diseases, cholera, and phthisis; cholera, however, being apparently less frequent in Madras than in the other two presidencies. In reference to these diseases it is justly observed:

> "When it is recollected that, unlike many of the most fatal diseases of Europe, the whole of those maladies which yearly commit such dreadful havoc in the ranks of our European army are of that description which all investigators in recent times, unite in proving to be either removable or mitigable by vigorous and liberal measures of sanitation, the military surgeon in India cannot but feel, amidst the many disappointments which attend his labours, that wherever he succeeds in carrying into operation any one Hygienic law, he begins to exercise the greatest of the Almighty's gift to man—the power of arresting death."

In addition to the great sickness and mortality above-mentioned, the Indian army annually loses a large portion of its strength by invaliding. It is shown that 16½ per cent. serve less than five years, 43½ per cent. less than ten years, and 84 per cent. less than twenty years; leaving 15¾ per cent. only of men above twenty years of service. Soldiers under twenty years contributed the largest number to the invalids, thus showing the impropriety of sending out young recruits to India. The author concludes the first section of his inquiry by observing that the European army of Bengal has been nearly decimated annually by death and invaliding, having lost 90 per 1000 of its strength every year from these conjoined causes. It is gratifying, however, to find that, since the commencement of the present century, the mortality rates of Europeans serving in each of the three presidencies have considerably decreased. This fact holds out good ground for hope that much may still be done in effecting a still further reduction.

The means for reducing sickness and mortality among European troops in India are considered under the following heads:—1. The proper selection of recruits. 2. The provision of well-found ships to convey them to India. 3. The selection of the proper season for their arrival in the country. 4. Judicious sanitation for them on their first landing. 5. The choice of a proper time and place for drilling. 6. The selection of the fittest season for sending newly arrived troops up country. 7. Sanitation for them in the river trip. 8. Sanitation for them on the march. 9. The provision of proper clothing and food. 10. The encouragement of habits of temperance. 11. The choice of proper military stations in the plains. 12. The choice of sanitaria in the hills. 13. The provision of proper barracks and hospitals. 14. The maintenance of a thorough system of conservancy in cantonments. 15. The improvement of the soldiers' morale, and the profitable occupation of their minds and bodies in cantonment. 16. Sanitation in the field, in standing camps, besieged forts, and entrenched camps. 17. Arrangement for the accommodation and safe conveyance of sick and wounded in the field. 18. The regulation of punishments. 19. Amelioration of the condition of the women and children. 20. Regulation of the mode of invaliding, and the adoption of measures for sending the invalids down country and home in the best manner.

We cannot give more than an enumeration of the many topics just mentioned. Suffice it to say, that they are treated in great detail; and that, while the author has freely availed himself of the investigations of others, he makes many valuable suggestions and recommendations of his own. We would particularly direct attention to the chapters upon the proper selection of recruits and the encouragement among them of habits of temperance.

In conclusion we would only remark, that no two works could have been published more opportunely than those of Mr. Jeffreys and Dr. Chevers, and that both are well deserving the serious attention of those in authority in India, and of sanitary reformers generally.

REVIEW X.

1. *Syphilisationen som Curmethode.* Ved W. BOECK.
Syphilization as a Method of Cure. By WILLIAM BOECK.—*Christiania*, 1857. 8vo, pp. 142.
2. *Discussionen i det Norske Medicinske Selskab i Christiania angaaende Syphilisationen.*
Report of the Discussion on the subject of Syphilization in the Norwegian Medical Society. 1857, pp. 161.
3. *Undersögelse angaaende Inoculation af Vaccine og Chanker materie, for at constatere Immunitäts forholdene og deres Consequentser.* Ved Dr. F. C. FAYE.
Researches upon Inoculation of the Vaccine and Chancre Virus, to ascertain the conditions of Immunity, and consequences thereof. By Dr. F. C. FAYE.—*Christiania*, 1857, pp. 70.
4. *Syphilizationen anvendt mod Syphilis og Spedalskhed.* Ved D. C. DANIELSSEN.
Syphilization, as employed in Syphilis and Leprosy. By D. C. DANIELSSEN.—*Bergen*, 8vo, pp. 125.

THE war of the empirics and of the dogmatists rages fiercely just now in the cold north; and party spirit is as warm at the foot of the Dovrefjeld as it was once on either side of the Apennines. Every innovation in medical science is certain to be submitted to the alembic of opposition; its weak points will be brought prominently forward in discussion, and if there be truth in the new doctrine or practice, it is generally only after a long struggle that it becomes firmly established. The profession is right to exercise a just caution under such circumstances, and especially at the present day, when innovations of such daring character have found favour with the public. Above all is this hesitation requisite when we are called upon to adopt a method of treatment totally opposed to our preconceived ideas—a method, in fact, of cure that is devoid of any theoretical foundation whatsoever. In a former article we had occasion to lay before our readers the details of Professor Wm. Boeck's researches into the efficacy of syphilization in the cure of secondary and tertiary venereal symptoms. Our analysis of what had been done in this respect, both at Christiania and elsewhere, was necessarily somewhat of a partial character, as up to the period at which Dr. Boeck wrote, hardly a voice had been raised in Norway to question the accuracy of his conclusions. But the curious subject of syphilization has now been advanced a step further, it has been brought prominently forward in the Christiania Medical Society, and in a discussion which occupied six meetings, the whole subject has been considered on the basis of the most recent investigations. That a little warmth of feeling should have occasionally shown itself among the disputants, is, to us, not a matter of surprise, but both parties seem to have been influenced by an earnest love of truth, and in many cases they exhibit a degree of candour and honesty not always to be found in the French Academy of Medicine, or elsewhere. That a final conclusion, either adverse or otherwise, has been arrived at, is more than we can say, but much that was before doubtful or obscure has been cleared up, and new theories of the action of this treatment have been proposed, which, if true, may greatly modify the opinions hitherto held by its advocates, as well as by its opponents.

In Norway, as elsewhere, syphilization has now met with serious opposition, though we must confess at once it is more the theory than the facts of this practice that has been disputed. To this question we shall return hereafter; at present we may state that on the 5th of November, 1856, Professor Faye, of Christiania, commenced a discussion in the Norwegian Medical Society upon syphilization, and which was continued throughout five or six meetings to the beginning of March, 1857. The attack, which was led by Professor Faye, was seconded but faintly by his colleagues; indeed, it seems to us, that no one took an active part on that side of the discussion except the Professor himself. It may be objected that, as Professor of Midwifery, and as one who had not personally followed the practice of syphilization, Dr. Faye was not fully qualified to give an opinion upon the subject. The question raised, however, by Professor Faye, was not directly of a practical character; he did not impugn the facts of syphilization, or deny in any way the cures that had thus been effected, but he proposed to account for this success in a different way, and sought to explain the apparent paradoxes of this mode of treatment by the light of modern science. In a word, he endeavoured to bring syphilization within the pale of our present physiological and pathological knowledge, and to strip it of the marvellous and of the unaccountable, which must always impede the progress of true and logical investigation. Professor Boeck did not attempt a direct reply to the arguments of Professor Faye, but while he acknowledged that he could not account pathologically for the success attending this practice, he did not allow the weak points of his opponent's arguments to escape his vigilant observation. We observe, too, that in the course of the discussion, several members of the medical profession in Christiania gave in their adhesion to the practice, if not to the theory of syphilization. Thus Dr. Gjör acknowledges having been totally incredulous until experience had convinced him of the great value of syphilization, while Vogt, one of the committee of control, appointed to watch the progress of the treatment, states, that after having opposed the system as a folly, he has now become so thoroughly convinced of its efficacy, that were he himself ever to become the victim of secondary syphillis, he would at once submit to the treatment, and would advise others to do the same. Egeberg, another member of the committee, expresses himself more cautiously. He states the duty of the committee to be to ascertain, first, that those who are submitted to syphilization really are affected with syphilis; secondly, that those stated to be healed by the method really are free from venereal symptoms when they leave the hospital; and thirdly, to note down and publish any relapses that may occur, as scrupulously as the cases of cure. As judges of the matter they do not consider themselves; the period has not as yet, in Egeberg's opinion, arrived for publishing any positive decision; all that he can say is, that the treatment gives good hopes of success.

On the whole, the result of the debate seems to us to have been decidedly favourable to the practice of syphilization, however much any theories founded upon this practice might be impugned. On the one hand, however, Professor Faye insisted that the question of syphilization, as a means of cure, rested on that of immunity to the venereal virus being at length obtained, while on the other, Dr. Boeck and his supporters urged, that the theory of the treatment did not affect the practice. Professor Faye maintained that syphilization acted merely as a depurative and suppurative process by the skin, a powerful counter-irritant action being thereby established, under which the original disease, the papular affections, ulcers, nocturnal pains, periostitis, and the long catalogue of secondary symptoms, gradually disappeared. Had this theory been supported by a sufficient array of facts, it would appear deserving of credit, but unfortunately, in this very point consists the weakness of Prof. Faye's argument, for his experiments with depurative suppurations produced by other means have yet to be made. Until this is done, until his theories, excellently as they are enunciated, are corroborated by actual experiments, we must suspend our judgment on the controversy, though we shall endeavour to lay before our readers the principal arguments employed by Professor Faye to support his opinions. On the other side, theory seems to be altogether at fault, but the undeniable success of this wholly empirical practice calls for our earnest consideration, and demands that the energies of science should be directed to ascertaining, if possible, the true *modus operandi* of the treatment. Before entering, however, on this part of our subject, let us briefly examine what progress has been made by syphilization during the past two years.

Subsequent to the discussion in the Christiania Medical Society in the early part of last year (1857), Professor Faye published the pamphlet which stands third upon our list, and Dr. Boeck brought out his essay on 'Syphilization as a means of Cure' (No. I.), embodying the result of another hundred cases, wherein syphilization had been employed, and generally with signal success. In September, 1857, Professor Faye visited England and Scotland, and read a paper at the meeting of the British Association at Dublin, wherein he ably brought forward the views he had defended in the Christiania Medical Society. Shortly after, Dr. Boeck followed in his track, and on the 19th September, 1857, he published in the 'Medical Times and Gazette' a well written letter giving the results of his experience in two hundred cases of syphilization. In this letter Professor Boeck wisely abstains from theorizing, and contents himself with the practical details of his treatment, and of the success thereby obtained. In November, Dr. Lauder Lindsay published a very able review of the whole subject in the 'Edinburgh Medical and Surgical Journal,' and in March last there appeared a letter from Dr. Boeck in the same Journal embodying his most recent experiences. It is only, however, among Edinburgh medical men that the question seems to have excited any interest at all, though it is plain that the period is not far distant, when either accumulated testimony in its favour will force the subject on our notice, or the whole theory fall to pieces at the touch of the wand of science and of logical investigation. We say the theory, for certain it is and undeniable, that remarkable results have been obtained by this mode of treatment, that cases of inveterate secondary affections have been, to all appearance, effectually relieved, nay, we may say completely cured, while the number of relapses in comparison to those known to occur after the usual mercurial courses, has been extraordinarily small. We do not advocate the plan, and judging from a physiological or a pathological point of view, we would unhesitatingly reject it; but the united testimony both of the advocates and opponents of the practice convinces us that herein, or in some similar process, is to be found a powerful means of modifying, and probably of arresting, the progress of secondary venereal affections.

In France, syphilization seems to have made but little progress, though the practice originated in that country. Nélaton has recently, it is said, allowed Auzias Turenne to experiment on a single case in his clinique; but it was not a well-chosen one, as it was a girl in delicate health, suffering from hereditary syphilis. Sperino has continued his studies on this subject at Turin, though they were interrupted by illness during 1856. In Sweden, Dr. Stenberg has published a pamphlet on Syphilization, with accompanying experiments; and in this he relates the histories of three patients whom he treated by this method, and who, after the lapse of two years, remained perfectly well. In Copenhagen, Professor Hassing has turned his attention to the question; but the results of his experiments are not as yet known to us. In Christiania, the headquarters of syphilization in the north of Europe, this treatment has been most extensively practised during the past year (1857), and has been extended even to children of tender years.

In 1856, Dr. Boeck requested three of his colleagues—Vogt, Steffens, and Egeberg—to form themselves into a committee, to observe the progress and results of his special treatment. No report as yet has been made by these gentlemen; indeed, it would be premature to expect it until further experience has corroborated or contradicted the alleged successes. At present, extensive experiments are being made on the subject by many practitioners in Scandinavia; and among these we may name Dr. Danielssen of Bergen, from whom we may shortly expect a full report of his experiments on syphilization, both in secondary venereal disease, and in the hitherto incurable malady, the old leprosy of Europe. It was quite evident from the very first that so great an innovation in practice, and one apparently so utterly indefensible on theoretical grounds, could not long remain unquestioned among men so fond of investigation, and so earnest in the pursuit of truth, as are our brethren in the north of Europe. Syphilization has here had a fair field laid open for experiment, and its results have been most narrowly watched by those who doubted of its efficacy. In England, little or no attention was excited by the discussions in the French Academy on this subject; and syphilization was placed at once in the company of homœopathy and of the other fashionable follies of the day. Not so in Norway; there, Professor Faye, dissatisfied—and justly so, we think—with the theories propounded by the upholders of the practice, has come forward with a theory of his own, which bids fair to sap the foundation of the edifice so recently and so pleasingly constructed. In the Norwegian Medical Society he maintained, in a protracted discussion, that no analogy exists between the protective powers of vaccination against small-pox, and that of the syphilitic virus against the venereal disease. He denied, indeed, the protective power of syphilization altogether; he called it a delusion, only based on the alleged immunity to the virus; and this he said, was simply a temporary immunity of the over-stimulated skin. After the discussion in the Norwegian Medical Society was closed, Professor Faye turned his attention to the subject of the influence of cow-pox inoculation on the system, in comparison with that said to be produced by the formation of artificial chancres on the body.

The following questions presented themselves to his inquiring mind for solution:

I. Is our belief in the alterative powers of vaccination on the constitution based on experience of such a character that it may be regarded as complete, and as doing away with the necessity of revaccination? And again, does it, from its presumed relationship to variola, protect the system from that disease also?

II. Does the producing of chancres on the skin, by the inoculation of chancre virus, create a real immunity (after a greater or less number of inoculations) to syphilis, similar to that presumed to be obtained by vaccination, or by the inoculation of small-pox matter; and in what relation does the intensity of the virus stand to the susceptibility of the system to its effects?

> "It might have been expected," observes Dr. Faye, "that Jenner's immortal discovery, after the lapse of half a century, would have been so practically tested by daily observation, and by direct experiment, that positive rules for the guidance of the profession in all matters relating to vaccination could ere this have been laid down. This, however, is far from being the case, and we are still uncertain as to the length of the period during which the system is enabled to resist fresh infection, nor have we fairly decided on the general prophylactic powers of vaccination in regard to small-pox."

Cow-pox, however, differs from variola in so far as that it cannot be propagated by atmospheric infection—it requires actual contact with the skin; while the poison of variola may be inhaled by the respiratory organs, as well as conveyed by direct contact with the blood.

Dr. Faye seems, therefore, to doubt the identity of small-pox and cow-pox, in opposition to the opinions of Thiele, of Ceeley, and many others. He admits, however, that there is a great similarity between the two diseases. Small-pox, as we might naturally infer, is often epidemic; cow-pox is never so; but both are true blood-diseases, and, when introduced into the system, we cannot destroy or stay their progress. The great question, however, is, whether the variolic and vaccine virus stand so closely related in their alterative influence, that vaccination destroys in the constitution the liabilisy to receive the poison of variola.

Dr. Faye sums up the result of investigations made by himself and others on this question as follows:

1. That vaccination, when duly performed, and followed by a negative result on a second trial, renders, in most cases, the constitution insusceptible of the poison of variola for the remainder of life, even when subjected to the influence of small-pox prevailing as an epidemic.

2. If variola does occur after a successful vaccination, its virulence will be in general greatly modified; but neither of the viruses gives absolute and certain immunity to variola during the individual's life. Vaccination should render the individual insusceptible of a second vaccination for a long period, if not for the entire duration of life—as, if it does not protect effectually against itself, it can hardly be expected to do so against the more potent poison of variola.

Dr. Faye then speaks of the requisites for a successful vaccination—viz., a good virus, its complete contact with the tissues, and the due susceptibility of the individual. In vaccination, however, we do not find that the immediate consequences of the punctures are a local inflammation; this does not occur till after the lapse of a certain period of time. Vaccine virus seems to cause a species of fermentation in the blood, while other matters act directly upon the locality to which they are applied. That such a fermentation really takes place is shown by the fact that, if you vaccinate afresh on the 12th or 13th day, a local inflammation with acuminated pustules shows itself within *two* days. If the vaccine pustule produces matter at too early a period, the result, as is known, will probably be unfavourable for the security of the patient. These questions are of particular interest in their bearing on the inoculation and operation of the syphilitic virus, hereafter to be considered.

Some individuals show themselves to be refractory to the vaccine virus; but Dr. Faye is of opinion that, if properly and fully introduced, there are very few constitutions that will resist its influence. No doubt, there is great difference in the powers of absorption in the skin—in the amount of blood-vessels in each square inch of integument in individuals; but by inserting the virus deeper than ordinary, and ensuring thus its full contact with the tissues, Dr. Faye thinks a favourable result may almost always be ensured.

From his experiments, he concludes that there really does exist a true immunity, not of the skin only, but of the constitution, produced by the vaccine virus; and that when fully performed, the individual is rendered insusceptible of a fresh zymotic disease of the same kind for a considerable length of time, if not for his whole life. It would be interesting, observes Dr, Faye, to ascertain whether any source of fallacy exists in our present mode of vaccination; and to what extent, and for how long a period, the inoculation of *variolous* matter alters the constitution, and renders it insusceptible of a second attack of small-pox.

We now come to the question of syphilization as compared to the effects of the vaccine virus on the system, and to which, by some of its advocates, it has been assimilated.

In the first place, the venereal poison may be acquired by hereditary transmission, and in this it widely differs from vaccination; while it cannot, in all probability, be communicated through the medium of the atmosphere, as is unquestionably the case with small-pox. The primary and secondary forms of venereal disease preserve their specific character throughout; it is only in the tertiary and still more advanced forms that the type of syphilis seems to be more or less changed.

The experiments of Ricord, of Hunter, and of others, have satisfactorily shown that the matter of a primary chancre can with the greatest ease be inoculated, and that this inoculation is the best proof of the real character of the sore. According to Ricord, however, this virus is only inoculable for a certain period, which is often only of short duration; and the question then arises, whether such a sore, which has ceased to produce matter available for syphilitic inoculation, can yet give rise to syphilis under the ordinary contact of the mucous surfaces "*in coitu.*" On this point, however, as on many others in regard to this important subject, great variety of opinion seems to prevail. Thiry of Brussels, and Ricord, hold that the chancre with indurated base is alone capable of producing constitutional syphilis; while Sigmund of Vienna, an equal authority, does not admit of any specific character of ulcer as being alone capable of conveying the disease. Again, Professor Ricord holds that secondary syphilis cannot be transmitted by contagion, while a large array of authorities may be cited in favour of the contrary opinion. In Norway, the latter doctrine seems decidedly to be the prevailing one.

It has been proved with regard to primary syphilis that infection can be produced in the natural way from a chancre which has ceased to afford inoculable pus:

> "The numerous inoculations of syphilitic virus which have been performed, both here in Christiania and elsewhere, and occasionally, too, on persons who never had had syphilis in their lives, have plainly shown that the local effects of the venereal poison can be transferred to various parts of the skin at one and the same time, and that chancres may be there produced without giving rise to any appreciable infection of the system, and certainly without producing the supposed characteristic induration of the base of the chancre, though the character of the local manifestation may vary according to the intensity of the virus employed, to the peculiarities of the skin, and to the individual tendency of the constitution to the elimination of morbid matter through this great secreting organ. According, however, to the statements of the syphilizers, there arises sooner or later a condition of the system wherein the skin shows itself insensible to any further inoculation of the chancre virus. As this immunity is considered to be equal to a perfect immunity of the system, we are constrained to admit that a chronic poison, such as that of syphilis certainly is, is circulating freely through the system without producing its usual injurious effects, and yet is bringing about a similar condition of the constitution to that occasioned by an acute zymotic disease such as small-pox, or the consequences of vaccination. We are to admit, in fact, that a fermenting of the poison, such as we consider as the result of small-pox or vaccine inoculation, here pervades the system, yet produces little or no irritative fever." (Faye, p. 30.)

True it is that such immunity may be only temporary—that the same individual, after the lapse of a certain time, may again be inoculated with positive results; but it is asserted that absolute impunity is at length obtained.

> "If," says Professor Faye, "we put together these statements, we come to the following singular conclusions:—That the poison of a primary chancre can yet infect *in coitu*, where it has ceased to be inoculable by the lancet; while on the other hand, the so-called inoculable virus can be propagated by inoculation for a longer or a shorter period, on the same individual, until the skin of this individual ceases to secrete a virus sufficiently active to affect itself again, though it may yet be efficient on the skin of another person. Again, virus taken from another person may in its turn possibly affect the supposed immune individual."

Professor Faye then considers the presumed immunity to the syphilitic poison to be fictitious; he regards it as only a temporary insensibility of the skin to further inoculations, and not as the consequence of an alterative change in the system, or of a presumed blood fermentation, such as is spoken of in regard to variola and vaccinia. He maintains that if the syphilitic virus be applied more deeply in the case of such individuals, by inserting it in larger quantities, and deep beneath the skin, where it comes more in contact with the blood, such inoculations will rarely be found to fail, and the presumed immunity disappears.

He brings forward a case in proof of this assertion from the practice of Dr. Vogt, one of the three members of the committee superintending the experiments on syphilization. The experiment was made with pus from an indurated chancre which had been tried in the ordinary way on eight different individuals, to the extent of sixty inoculations, but in all had failed, though these persons could be inoculated by other chancre matter. An incision was now made through the skin, instead of the ordinary slight scratch with the lancet, and the matter was inserted, and daily renewed, and the whole kept carefully bound up. This time the inoculation succeeded, *and the matter was found to produce the specific sores when tried on others.*

Professor Faye considers the poison of secondary syphilis to be weaker, but still of the same character, and subject to the same, though less manifest, conditions of propagation. He believes it to require more prolonged contact with the tissues, as where a syphilitic child infects a healthy nurse, and thus it is with greater difficulty inoculated by the lancet.

Two cases are given of secondary syphilis where Professor Faye experimented with a view to corroborate the above opinion, after both of them, mother and daughter, had been submitted to syphilization. Neither of these had taken mercury, but they had twice relapsed after a very short process of syphilization. In both there was great insensibility of the skin, and the chancres could not be further transmitted. The cases, therefore, cannot be regarded as complete as regards syphilization; for it has all along been observed, that where immunity is rapidly produced, the tendency to relapse is proportionately increased. Here, by deeper inoculation, chancres were again produced, and their character proved by transmission to other individuals, and by retro-transmission to the two persons above-mentioned.

So far, we think, though the experiments are incomplete and very few in number, Professor Faye has shown good grounds for doubting the value of the immunity so much insisted upon by the early syphilizers; but we should have been glad of more sustained proof, and of a greater array of cases to support his opinion.

From these few experiments he draws the conclusion that there is no such thing as the presumed immunity of the system to the venereal poison. He tells us that the external layers of the skin from prolonged inoculation, lose their vitality, and become more or less insensible to the virus, and little or no reaction is produced; the indurated chancre does not appear, and the sore that is formed is covered quickly with a scab, instead of progressing to suppuration. Still, if the virus employed be very intense, the usual results may yet occur, though the skin is ill suited, from the loss of its vitality, to reproducing virus for continuing the transmission. Here, however, Professor Faye neglects to tell us how it is that the whole skin becomes thus insensible to the venereal poison, for we should imagine that the local effects would be confined strictly to that part of the integument where the inoculations had been practised. We cannot see why the whole skin should lose its vitality from constant punctures made in the arms, thighs, and chest alone. It would have been well if Professor Faye had carefully satisfied himself on this point, and had given us an adequate explanation of the fact—if fact it be—that under the process of syphilization applied to the above parts only, the whole of the integuments of other parts, as well as these, become insensible to the venereal poison. We confess this looks like some alterative change taking place through the blood, though, on physiological and pathological grounds, we should be inclined to refuse credence to such an opinion.

To return, however, to Professor Faye's conclusions: he tells us that should the virus, which has become inactive on one person, be transferred to another individual in whom there exists greater susceptiblity, and a greater tendency to elimination through that organ, we shall then find that it produces its full effect again. In a word, the susceptibility of the skin and the relative intensity of the virus will account for the gradations of difference in the supposed immunity, when the inoculations are performed in the usual superficial manner. Dr. Faye thinks that when the virus is introduced by a grooved needle into a subcutaneous canal, its intensity is much augmented by being thus cut off from air and light.

While Dr. Boeck, at the present day, does not regard immunity to all syphilitic virus as absolutely requisite, Dr. Faye, on the other hand, insists that it has a most important bearing on the whole question, and that if immunity be thrown aside, the whole fabric of the theory must fall, and we must seek elsewhere for an explanation of the success of the practice. In a word, the practice of syphilization will remain as an empirical method of treatment, and one quite inexplicable by our present knowledge of physiology and pathology. In saying this, Dr. Faye gives full testimony to the acknowledged fact that the secondary phenomena of constitutional syphilis have, in a great number of cases, disappeared under a course of syphilization—"*at secundaere Phaenomener af constitutionel Syphilis i et stort Antal tilfaelde ere svundne under Syphilisationscuren*" (p. 59) as far as the experience of the present time extends.

The variations that have been observed in the relations of immunity to the curative effects obtained by syphilization have been as follows:—

1. In many cases the prevailing symptoms have disappeared before any immunity to the syphilitic virus had exhibited itself.

2. In other cases the disappearance of the symptoms coincided with the supervention of immunity.

3. In certain cases the secondary phenomena did not vanish even when immunity had been obtained; and this has been particularly observed in those instances where relapses have occurred.

> "As far as I can see," continues Dr. Faye, "the curative inoculation of chancre virus loses by being deprived of its immunity, its most rational point of support, for then the method will, if considered as an alterative process carried on in the system, have to stand alone, without the aid of pathological science or physiological induction. No system of physiology or of pathology has as yet made us acquainted with a chronic zymosis, or blood-poisoning, which, under a constant re-introduction of the poison, operates in one case beneficially, and in another is followed by the most serious consequences." (p. 61.)

We see, then, that Professor Faye absolutely denies the existence of the supposed immunity to further infection, on which the earlier syphilizers laid so much stress. Among these we may reckon Dr. William Boeck himself, who certainly has contributed more than any other person to bring the subject under the notice of the profession in Norway; and to his latest observations we shall now briefly call the reader's attention. Two years of additional experience in the practice of syphilization seems only to have strengthened Dr. W. Boeck's confidence in this mode of treatment, and seems likewise to have induced him partially to moderate some of his theories. He does not hesitate to confess that the whole process by which syphilization cures constitutional syphilis is to him an enigma; but he still appeals to facts in confirmation of its efficacy. We must regard the practice, then, as purely empirical; but perhaps the same may be said with respect to the agency of quinine in the cure of intermittents. In the extensive *resumé* of his experience given at the end of the pamphlet which stands first upon our list, we have noticed some facts and observations which may be new to our readers, and which should be carefully compared with the opinions expressed by Dr. Boeck when we reviewed his former works in this Journal. As to the time that intervenes between the inoculation and the appearance of the pustule and chancre, Dr. Boeck says that it is generally only twenty-four hours, but in children a longer period is often required. Sometimes, even in adults, the chancre pustules are developed slowly; and occasionally there are exceptions to the rule that the earlier chancres are the most developed.

Great diversity is observed in the size of the chancres; those on the nates and thighs are usually the largest and deepest; and it has been observed that when inoculation has ceased to produce its effects on the arms and sidés, large chancres still followed when the virus was introduced on the nates or thighs. This, at first sight, would seem to corroborate Professor Faye's opinion, that no constitutional alterative effect is produced by the inoculations; but the contrary is observed to be the case when the inoculations are first confined to the thighs, for then the virus is often found inefficient when it is subsequently introduced on the arms and sides. If this is really the case, we have here a strong proof of some real change being produced in the system beyond a mere local immunity from repeated irritation of the skin. Dr. Boeck has occasionally seen the chancres take on a phagedenic character; but in all instances this untoward symptom vanished speedily under continued syphilization.

The time required for the healing of the chancres varies considerably; they may disappear in a few weeks, or may continue open for months. It has been a subject of dispute as to whether the venereal poison varies in intensity, or whether its effects are only modified by the constitution of the individual. The former opinion is strenuously maintained by our author.

Subsequent to the publication of his former pamphlets, the prolonged discussion on Syphilization took place in the Christiania Medical Society. On reading carefully the reports of that discussion, it does seem to us that Dr. Boeck partially abandoned his great point of immunity; but in his present publication he evidently upholds it as strenuously as ever:

> "In my earlier writings on Syphilization I considered immunity to be one of the most important points in the whole treatment, and with my further experience I have found no cause

for receding from this opinion. That under protracted inoculation of the syphilitic virus, the system finally becomes so insensible to the action of the poison, that you may introduce it with no more effect than would be produced by an equal quantity of water, is, in my opinion, a great physiological fact. The constitution of the individual after protracted inoculation assumes precisely the same position with regard to syphilis as we find in persons who have gone through measles, scarlatina, or small-pox, in respect to these exanthematous diseases. Against these last the constitution becomes effectually protected after an alterative process of a few days' duration; while in syphilis this only takes place after a long series of inoculations, but *then this disease is in its nature essentially chronic.*" (p. 120.)

Here, then, we have Dr. Boeck's profession of faith unaltered, even after the rude onslaught made upon it by Professor Faye. We think, however, that, still more recently, he has somewhat modified his opinions in this regard. In his letter to the *Medical Gazette*, Sept. 19, 1857, he says—"I will not engage in any strife as to the word 'immunity;' I would only insist on this, that the body is brought into a new and healthier condition by these inoculations." Dr. Boeck is incredulous of Professor Faye having produced afresh inoculable chancres after an individual had once attained the desired immunity. He says, in a more recent communication, bearing date March, 1858, with which he has kindly favoured us, that the pustules thus produced do not form regular chancres—that on the contrary, they crust over, dry up, and are abortive, healing from the periphery towards the centre, while the true chancre extends itself in a contrary direction.

"I will not, however," continues he, "hold to the word 'immunity,' but would use the following expressions of my opinions. That, by prolonged inoculation, there is developed a condition of the system which, in relation to the operation of the syphilitic poison, is altogether different from that which existed before syphilization was begun. Another question has been mixed up with that of immunity—viz., the question of its duration. To this I answer—1. I do not know if the same condition of the system which existed prior to syphilization will ever return, but I believe it will be a long time before it does so. 2. It is not long, however, before it is possible, by fresh inoculations, to produce small pustules and sores, and with these to effect a short series of inoculations, and the more distant the period is from that of the first syphilization the more numerous are the chancres, and the further can the series of inoculations be transmitted."

It would appear from these admissions that the immunity obtained by repeated syphilization is possibly not of long continuance, but on the other hand it is rather a remarkable fact as stated by Dr. Boeck at page 130 of this pamphlet, that of those individuals who had not been treated with mercury previous to syphilization (and their number amounts to more than a hundred) not one has come back to him with syphilis. It was not that these individuals had been leading unusually chaste lives (for many of them had taken gonorrhea), but it was as Dr. Boeck believes, because the syphilitic virus had little or no effect on their system when it was in contact with the mucous membranes. As comments on Professor Faye's experiments on syphilization, Dr. Boeck has turned his attention to the possibility of reproducing the vaccine pustule in children already vaccinated. In one case he operated on a child of three years on the 1st of May, and on the 8th there was a well-formed vaccine pustule on the arm, from whence he took matter and transferred it to the nates of the same child, when five out of the six punctures produced well-developed pustules, but on trying to transfer from these last to the arm and nates again the experiment failed. In his private communication to us, Dr. Boeck informs us, that besides the three published cases, he has since had one more case of relapse after syphilization, where the patient had not previously taken mercury.

Some further practical observations on the subject from the same source will interest our readers—

"1. The more the skin of the person to be syphilized is occupied by papular or pustular eruptions, the greater difficulty is experienced in inoculating, and the slower is the progress of the cure.

"2. If measles come on in the process of syphilization, the symptoms of syphilis are increased and the cure is protracted

> "3. Iritis, when it occurs in the progress of syphilization, and where mercury has not previously been employed, does not require any local treatment I have always succeeded in healing it speedily by steadily continuing the inoculations."

We have now endeavoured to lay before our readers a complete account of the position of syphilization in Norway up to the summer of this year (1858). The more we have examined into the subject, the more does it urge itself upon our attention. If the cures were disputed, if the fact that by repeated inoculations of chancre matter very serious cases of constitutional syphilis are cured, were denied, then we should deem it first to be our duty to inquire into the truth of the success of this treatment. Its success however is not denied, the rarity of relapses also after its employment is not questioned; the only matter in dispute is the modus operandi of the treatment. That it is empirical in the fullest sense of the word we at once concede, but its success in so intractable a disease as secondary and tertiary syphilis calls for our earnest consideration. Of the theory of its operation the less that is said the better; we think Dr. Boeck is in error if he still holds to the identity of action between syphilization and vaccination; while on the other hand we do not consider Professor Faye to have proved by experiment his assertion, that the cure is only brought about by a depurative suppuration. We should not ourselves imagine that such a purely mechanical process would tend to invigorate and restore the health of the patient, while it is well known that under syphilization almost all the patients improve in condition; on the other hand, we cannot believe this to be the result of the circulation of the venereal poison in the blood; we cannot understand how this could improve the health when introduced for months together, while a single venereal infection acts so deleteriously upon the system. We shall look with anxiety for further documents on this subject, and especially for the essay promised to us from the pen of that learned and acute observer, Dr. Danielssen of Bergen. Syphilization is now practised in many parts of Norway and Sweden, and we have such full confidence in the honesty, truthfulness, and skill of our Scandinavian brethren, that we are convinced they will investigate the whole subject thoroughly; and the result, be it adverse or favourable, will assuredly be a boon to medical science.

Since the above was written, we have received from Norway Dr. Danielssen's promised essay 'on Syphilization in Syphilis and Leprosy.' It is a modest pamphlet of 125 pages; but we regard it as one of the most important contributions yet made to the elucidation of the curious subject of syphilization. Dr. Danielssen had visited Paris shortly after Auzias Turenne's doctrine and practice had been so summarily condemned by the French Academy of Medicine; but it was not until the autumn of 1853 that he was enabled to follow Boeck's experiments at Christiania; for Bergen, though in the same kingdom, is yet many days' journey distant from the Norwegian capital. After having convinced himself at Christiania of the real efficacy of this mode of treatment in secondary syphilis, it occurred to him to test its efficacy in a non-syphilitic malady, which had hitherto proved intractable to ordinary remedies—viz., the leprosy of the western coast. Dr. Danielssen's reasons for adopting this mode of treatment are bold and peculiar—

> "According to my knowledge of spedalskhed (leprosy), it has no connexion with syphilis; the maladies are radically distinct, and therefore neither analogy nor experience inclined me to the belief that syphilization, as it was then employed, would have any beneficial effects on leprosy. It appeared to me, however, that if I could infect the leprous patients with universal syphilis (constitutional syphilis), it might follow that the syphilitic poison might prove superior to that of leprosy, and that thus the system might be brought to that of a person labouring under constitutional syphilis, and might so become subject to the ordinary process of syphilization." (p. 2.)

Dr. Danielssen accordingly inoculated four leprous patients with the syphilitic virus in 1854, and the characteristic pustules were produced. The resulting chancres were left to themselves, in the hope that they would ultimately produce constitutional syphilis. After remaining open for five weeks they began to close, and by the end of the seventh week they were entirely healed. No secondary symptoms, however, ensued, although years have now elapsed since the experiment was made. Meanwhile Professor Boeck continued his researches, and distinctly proved, according to Dr. Danielssen's testimony, that syphilization could effectually disperse the symptoms of secondary syphilis especially if they had not previously been treated with mercury. Boeck also decidedly proved that this method of treatment was devoid of all danger, and it became incumbent on all those who undertook the treatment of syphilitic disease, and who did not regard mercury as an infallible specific in that malady, to make trial of the new remedy.

Dr. Danielssen did not commence the regular practice of syphilization in the Hospital at Bergen till 1856. In August of that year, we saw a good many patients undergoing this treatment under his superintendence, and here, as well as at Christiania, they expressed themselves well satisfied with the process. Up to December, 1857, he and his colleague, Dr. Bull, had treated 25 cases of secondary syphilis in this manner, and Dr. Danielssen had likewise subjected 23 lepers to a similar course of treatment. He had now, however, relinquished his ingenious but singular idea of producing constitutional syphilis in lepers, in the hope of destroying the latter diathesis, and merely confined his experiments to the question, whether syphilization would directly influence the leprous onstitution or no.

It will be remembered by our readers that Boeck is little disposed to admit the great distinction established by Ricord between the simple soft chancre and the indurated chancre. In regard to syphilization, Dr. Boeck evidently considers the one as not more virulent than the other; while Dr. Danielssen regards the distinction as in every way important. Many chancres, observes Dr. Danielssen, continue primary throughout their whole course, and produce no effect upon the constitution. The hard chancre (chacre infêctant induré) he considers to be the true syphilitic sore, the simple soft chancre he looks upon as of more bastard character, and unable to produce the well-known specific effects on the constitution. Both chancres he believes to arise from the syphilitic virus, but he regards them as much distinct as vaccine matter is from the virus of variola. Vaccine virus has never produced small-pox, but its action is undoubtedly prophylactic of that disease. Here, however, in Dr. Danielssen's opinion, all analogy between the action of syphilization and that of vaccination ceases; and the former remains, as he observes, *a new fact* in practical medicine, the explanation of which will not be obtained till after further and more extended researches.

Dr. Danielssen at first gave full credit to Boeck's assertion, that under syphilization the virus actually passed through the constitution, and so produced its beneficial effects.

> "Under this impression," observes he, "I commenced my course of inoculations, both in cases of secondary syphilis and of leprosy, but experience has now convinced me that syphilization is not dependent for its effects upon the long-continued influence of the syphilitic virus upon the entire organism. We have not here to do with a new physiological fact, but with a pathological process, which, in its consequences, is of vast importance for practical medicine. It appears to me that we must first, in order to explain the operation of syphilization, seek to ascertain whether the virus by repeated inoculation really passes into the system, or whether its operation is confined to the immediate vicinity of the artificial chancre—in a word, whether its operation is universal or local."

Dr. Danielssen's opinion is decidedly in favour of a mere local action, and this he supports by five or six well detailed and interesting cases, where artificial inoculation, arrested long before the so-called immunity was produced, never gave rise to any constitutional symptoms. Two of these cases were in leprous subjects, and one was where the patient was syphilized for chronic eczema, which was not of syphilitic origin. We have only space for the remarks on these cases.

> "It appears from the above details, that neither one chancre, nor two, nor three, nor six, nor thirty-six, nor one hundred and thirty-six have in the preceding cases induced secondary syphilis, and that, therefore, the direct operation of the inoculations has been exclusively limited to the spot where the chancres had shown themselves. If such be the case, we are justified in assuming that no greater number of chancres will produce a different result. And this is confirmed by our experience; for with one exception, to which we shall subsequently allude, not one of those individuals, previously free from all syphilitic taint, whom I have

syphilized, have been affected by secondary syphilis; nor have they shown any signs of the existence of the venereal diathesis in their systems. Nor, in those already affected with syphilis, have I observed under syphilization the slightest evidence of their having imbibed the poison afresh. So far from seeing in syphilization a new physiological fact, as Boeck denominates it, I have, on the contrary, found it confirm a long established axiom—viz., that the simple soft chancre does not affect the system, and consequently does not produce constitutional syphilis. Among the many thousand artificial chancres that I have seen, I have not observed one (with a single objection) which was not of this character, both in my own practice and in that of my colleagues, and as inoculated on every part of the body. Even on the face, the soft chancre followed inoculation, contrary to Ricord's experience, who had always observed the indurated chancre there." (p. 22).

Dr. Danielssen, however, does not deny the possibility of affecting the system by artificial inoculation of syphilitic virus, but he believes that it can only arise when the virus is taken from an indurated chancre.

The fresh outbreaks of syphilitic disease, after syphilization has been for some time employed in certain cases, are no proof of a fresh absorption of the poison, as such symptoms frequently occur under any mode of treatment. Moreover, if Ricord's axiom be true, that constitutional syphilis can only occur once in the same individual, it is not possible that any absorption of the virus could take place. One of Dr. Danielssen's leprous patients, who had been under syphilization for upwards of two months, died of pleurisy and effusion dependent on the former malady. Dr. D. ventured upon the rash, and, to our minds, inconclusive experiment of inoculating three or four other patients with the effused matter from the pleural sac. He produced dangerous phlebitis, but no syphilis; and from hence he argues that no absorption of the syphilitic virus takes place under the ordinary process of artificial inoculation.

We now come to the grand question of immunity, on which Professor W. Boeck in his earlier writings laid so much stress. Dr. Danielssen does not believe in the constitutional change alleged to be produced previous to this effect; he regards immunity to the syphilitic virus as a simple loss of reacting power in the skin, and which it sooner or later regains. He has satisfied himself that when the integument, from external or internal causes, has lost its vitality, it is no longer sensible to the inoculation of the venereal poison, but the inoculations succeed again when the skin has recovered its vital powers. Dr. D. had long treated cases of leprosy with large doses of tartrate of antimony, and he had found that an anæmic condition of the skin was sooner or later produced by this medicine, during the prevalence of which condition the skin seemed to be insensible to the action of the syphilitic poison. Upon his restoring the natural "turgor" of the skin by iodide of potass, he has found that the supposed immunity no longer existed; the skin had, under the influence of this drug, recovered its vitality. In certain individuals with a remarkably loose and pale skin, he has remarked that the inoculation punctures produced at first no result whatsoever; but that after the lapse of two or three weeks, when the general health had been restored, these very punctures suddenly took on their characteristic action, and formed large chancres of undoubted syphilitic origin.

A remarkable instance of this kind is given at p. 34 of this essay, where a leper was inoculated with syphilitic virus, unfortunately, in this single instance, taken from an indurated chancre. In this individual, syphilization from simple chancre virus had been carried on previously to the extent of nearly 400 inoculations, and at first the virus from the indurated chancre produced no positive results. A month after, however, the cicatrix of an old chancre broke up again, and produced a regular indurated sore, and soon after the unmistakeable signs of secondary syphilis made their appearance.

Dr. Danielssen has also found the supposed immunity to be of very local character; the nates and thighs becoming insensible to the virus, while it could still be reproduced in full vigour on other parts of the body. He, however, thinks it possible that a general immunity may for a time ensue, when the vitality of the integument has been lowered by repeated inoculations.

All this therefore proves to us, that the immunity which follows syphilization is solely dependent upon the condition of the integument, and is absolutely distinct from that which results from vaccination or after the exanthemata. Nor is Dr. Danielssen at all disposed to admit that even this temporary immunity is complete to *all* syphilitic virus.

The operation of the process of syphilization must therefore be considered as strictly local, and as incapable of producing any constitutional change in the system.

> "This, however," observes Dr. Danielssen, "is of less importance in a practical point of view; for the grand feature of the process is the curative effect of the continuous inoculation of chancre virus. It is now established beyond all doubt that secondary syphilis can be healed in this manner; but whether the cure will be permanent, whether relapses will subsequently occur, time alone can show. That the operation of the treatment is strictly local, I do not regard as a reproach; on the contrary, I look upon this as inviting still more to a trial of this new method of cure, without being terrified by the many phantoms that have been conjured up against it. With the exception of the pain that occasionally results from the earlier chancres, I have not seen any untoward results from syphilization; and although my experience in regard to the mercurial treatment of syphilis does not entirely agree with that of Professor Boeck, and though I do not look upon mercury as a destructive fiend, yet I agree with him that the mercurial treatment of syphilis is so often uncertain, and that the cases are so very liable to relapse, that we should be thankful if we have now obtained another mode of treatment which, fraught with less danger, promises a more favourable result." (p. 38.)

Dr. Danielssen considers it to be an interesting pathological phenomenon, that secondary syphilis can be healed by the protracted suppuration consequent upon continuous inoculation of chancre virus; but he does not look upon this fact as absolutely standing alone in pathology. In tertiary syphilis nature herself produces deep suppurations, and if the strength holds out, all secondary symptoms disappear during the process. Of the 25 syphilitic patients treated at Bergen, 21 suffered from secondary and 4 from tertiary symptoms: 15 of these had never taken mercury, and were all speedily cured by syphilization alone. In those who had previously taken mercury, and whose system was debilitated, iodine was occasionally employed; but all were cured, and only one relapse has occurred, and this was but of temporary duration.

In no one instance did syphilization produce any favourable effect in leprous disease, the terrible malady proceeded unchecked in its career.

We look upon this pamphlet of Dr. Danielssen's as by far the most valuable essay that has yet appeared on the subject of syphilization.

While the practice of inoculation with the syphilitic virus in secondary cases has been most diligently pursued by Boeck, Sperino, and others, the theories advanced by these authors as to the *modus operandi* of syphilization have been confessedly unsatisfactory; as they involved pathological and physiological contradictions which could not and would not be received by the scientific world. The doctrine of Professor Faye relative to the depurative action of syphilization was the first to indicate the true line of operation of this treatment; but, unsupported or nearly so, as it was, by personal experience, it would have had but little influence on the profession. To Dr. Danielssen, in our opinion, is to be accorded the honour of having satisfactorily solved the problem, of having cleared up the doubts and difficulties that environed the subject, and obstructed the progress of a really valuable method of cure. Syphilization has now, for the first time, been brought fairly within the limits of modern science; it is no longer a wild empiricism, but will receive the calm consideration of the Profession.

REVIEW XI.

1. *Ueber die Wirkung warmer Sitzbäder.* Von Dr. L. LEHMANN. ('Archiv des Vereins für gemeinschaftliche Arbeiten zur Förderung der wissenschaftliche Heilkunde.' Band i. p. 521; Band ii. p. 1.)

On the Effect of Warm Sitz-baths. By Dr. L. LEHMANN.

2. *Die Sooltherme zu Bad Oeynhausen (Rehme) und das gewöhnliche Wasser.* Von Dr. L. Lehmann.—*Göttingen*, 1856.

The Saline Baths of Oeynhausen and Ordinary Water. By Dr. L. Lehmann.

3. *Das Thermalbad in Bad Oeynhausen.* Von Dr. L. Lehmann. ('Archiv des Vereins,' Band iv. p. 18.)

The Warm Bath of Oeynhausen. By Dr. Lehmann.

4. *Versuche über die physiologische Wirkung des Kochbrunnens zu Wiesbaden.* ('Archiv des Vereins fur wiss. Heilk.,' Band iii. p. 59.)

On the Physiological Action of the Saline Baths of Wiesbaden. By Dr. Neubauer.

5. *Die Bedeutung und der Werth arithmetischer Mittel mit besonderer Beziehung auf die neueren physiologischen Versuche zur Bestimmung des Einflusses gegebener Momente auf den Stoffwechsel.* Von Professor Radiche in Bonn. ('Archiv für physiologische Heilkunde.' 1858, p. 145.)

The Importance and Value of Arithmetical Mean Values in the Determination of Certain Influences exerted upon the Metamorphosis of the Tissues. By Professor Radiche.

The study of the final products of tissue change (the products derived from the substances which have played their part in the stage of life, and which emerge from the skin, the lungs, the bowels, or the kidneys) has lately undergone a great development. The new modes of examining the urine, which render feasible a rapid determination of its chief ingredients, the improved appliances for collecting the perspiration, as adopted by Schottin and Funke, the greater accuracy of the mode of determining the carbonic acid and water of the breath, and the important observations of Marcet on the intestinal excretions, seem to indicate that we are approaching a time when the physician will not only be able exactly to know what enters the body, but will be able to give an accurate numerical determination of what comes out of it.

Already, especially in Germany, we see these methods applied to physiological, therapeutical, and pathological inquiries, and in the midst of much that is unsound, untrue, and misleading, it is impossible not to recognise the fact that what is virtually a new field has been thrown open for investigation, and that important discoveries may reasonably be expected. At present it is, however, desirable to observe that no little caution and careful criticism is necessary in order to prevent a flood of crude and deceptive observations overwhelming the sterling facts of acquired science. That the experiments should be exact and careful, is of course only what we expect from all who are engaged in these inquiries. But when conscientious and exact quantitative analyses of the urine or the pulmonary excretion have been made, an incorrect way of stating the results may vitiate all the conclusions. On subjecting to a mathematical analysis some of the latest observations of the new school, Professor Radiche of Bonn* has shown that the common mode of reckoning averages is liable to grievous error, and that exacter methods of calculating results must be used if anything like mathematical certainty is to be introduced into physiologico-chemical inquiries. Some of the best conducted experiments—as those of Mosler on the action of water, Beneke on sea bathing, Kaupp on the influence of chloride of sodium on urea—are shown to be more or less vitiated by inherent errors in the mode of calculating the mean results of their experiments, so that possibly all these elaborate and apparently conclusive experiments may have to be again performed. But after careful experiments have been properly calculated, it still remains to draw well-founded conclusions; and this is a matter of such difficulty, that almost every conclusion at present come to, must be regarded as merely provisional. As an instance of our meaning, we will select one substance only, urea. Urea can now be quantitatively determined with great ease and considerable accuracy; supposing in any case that an increased amount of urea is decidedly excreted by the kidneys, such increase cannot now be held to be *per se* evidence of increased tissue change, as it has by some been considered to be; for it may result, 1st, merely from increased elimination, with or without previous retention; 2nd, from immediate formation of urea from food (for the vital question of the "luxus consumption" of Bidder and Schmidt is by no means settled,* and the possibility of this origin of urea cannot be disregarded); 3rd, from vicarious action when the skin is inactive, for if the late observations of Picard and Funke be correct, and if some quantity of urea is daily excreted by the healthy skin, we cannot reject the possibility of the skin and kidneys acting vicariously as regards urea, as they undoubtedly do as regards water and chloride of sodium; 4th, from increased tissue metamorphosis; 5th, from perverted metamorphosis, for it appears probable that a portion of the albuminous food and tissues normally runs through a series of metamorphoses which end in eliminating its nitrogen as ammonia, and this portion may conceivably vary in amount. It is obvious, therefore, that in many cases the cause of an increased excretion of urea will be determined with great difficulty, and conclusions must be drawn with the utmost caution.

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* Die Bedeutung und der Werth arithmetischer Mittel, Archiv für phys. Heilk., 1858, p. 145.

The difficulties with other excreta are scarcely less formidable, and in fact it may be said that absolutely certain results will not be arrived at until all the products passing off from all channels (skin, kidneys, lungs, and bowels) can be quantitatively determined during the same period. At present this perfection of analysis has not been attained, though we appear certainly to be approaching it.

We have made these few remarks before proceeding to bring before our readers some of the late inquiries conducted on the plan now referred to. Did we not believe that these inquiries are really of great value, in spite of the errors and inconsistencies which are obvious in them, we should not think of requesting the serious attention of our readers. But we do believe that as microscopic research has been the character of the last fifteen years, and as molecular chemistry and molecular physics (if we may use the term) will be the great object of the next generation of physicians, so an era of chemical inquiries is commencing for us, which, like the microscope, must contribute its share towards the formation of the exacter science which it is not too much to hope is dawning upon us.

On the present occasion we have selected for our subject the influence of BATHS, and we proceed to analyse the chief papers which during the last three or four years have been published on this head.

The writer who has contributed most largely to this subject is Dr. Julius Lehmann (a namesake of the great physiological chemist), an observer whose name we have had to quote several times in this journal in connexion with inquiries of a like kind. Dr. J. Lehmann is evidently not only an industrious but an accurate man, and we believe we may safely accept the record of his experiments as conscientious and true.

1. *Hip Baths.*—Lehmann's earliest experiments were with *Cold Hip* (*sitz*) *Baths*,† and were conducted as follows. During six fasting hours on eight days, the exact loss of weight of the body (caused by the constant elimination without fresh supply of food), and the exact amount of urine and fæces, were determined. The weight of the urine and fæces deducted from the total loss of weight of the body at the end of the period of experiment, gave of course the loss by "insensible perspiration."‡ During eight other days, one or two cold sitz baths (fifteen minutes) were taken during the six hours; the loss of body-weight, the amount of urine, fæces, and insensible perspiration, were again determined. The two series were then compared. We give the mean results:

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* The arguments of F. Führer and H. Ludwig (Die Quellen des Harnstoffs, Archiv fur phys. Heilk., 1855, p. 491,) seem to us very strong against the view of Bischoff and Liebig, even if they do not prove the writer's own view of the part played by the red corpuscles.

† Archiv des Verieus für wissenschaftl. Heilk., Band i. p. 521. The water in the sitz-bath covers the upper parts of the thighs and the body to just above the pelvis.

‡ The "insensible perspiration" is a term usually employed to denote the quantity of all the products passing off by the skin and lungs.

The mean loss of weight in each six hours was,

Without baths	463·5	grammes (16⅓ ozs. av.)
With	748	„ (26⅓ „)

The loss produced by the fasting alone was thus made up—

By urine	271·91	grammes.
By insensible perspiration	175·96	„
By fæces	15·63	„
Total	463·50	„

The loss produced by combined fasting and baths was—

By urine	463·1	grammes
By insensible perspiration	245·4	„
By fæces	40·3	„
Total	748·8	„

The increase in the urine was owing partly to increased flow of water (½), partly to augmented discharge of solids. The increase in the solids was owing to urea in great measure (¾), and then to chloride of sodium; the sulphuric acid was unaltered; the phosphoric acid rather lessened; the uric acid was only altered in so narrow a limit as to be within the range of error of observation.

Such, then, are the mean results; now, what are they worth? We admit at once that the experiments were exact, and that really this increase did occur. But was it owing to the baths, or to some one or more of the other influences (external temperature, movement, nervous action, &c.), which are constantly acting on the metamorphosis of tissue?

The first objection, and, to our minds, a fatal one, is the circumstance that the experiments were not made on successive days, but were scattered over an interval of nearly four months, some experiments being conducted in July, others in August, September, October, and November. Nothing can be more fallacious than such a method, as the amount of urine is subject to great and even periodical changes,* and by selecting a day here and there, we may chance to hit on one of the greatest deviations from the mean. Secondly, the number of days (eight) is not sufficient to allow a true mean to be drawn. On this point opinions have lately undergone some development, and it is now generally considered that observations must be carried on for many days (twenty or thirty at the least) before a sufficiently correct mean is arrived at, unless in cases in which, as in disease, deviations so enormous as to preclude the idea of error, occur from the mean. Thirdly, on subjecting some of Lehmann's daily observations (from which the mean is drawn) to an analysis similar to that recommended by Radiche, we find the possible errors to be so great, as to render any decided deductions hazardous.† The utmost that can be said is, that there is a *probability* (the extent of which is scarcely worth numerical expression) of the effect of the cold sitz bath being really that assigned to it. If, indeed, other observations conducted on a larger scale should accord with these, it would give them more weight.

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* A certain periodical or wave-like increase and decrease in the urinary excretion was affirmed by the writer of this Review, in the Gulstonian lectures for 1855. Radiche from his calculations, and Vierordt from independent observation, have lately stated the same fact. If an experimenter on the urine happens to take a day at the ebb, or at the full flow of the wave, how different would be the result from the real average amount.

† We have, in fact, only calculated the possible errors in one ingredient, viz., the water of the urine, as we thought it unnecessary to take further trouble. We may briefly sketch the result:

The measured quantity of urine on each of the non-bathing days amounted during the six hours in cubic centimetres to 201·85, 274·8, 226·8, 364, 194, 282·6, 290·85, 341·5. The successive means (obtained by taking the means first of the two first days, then of the three first, of the four first, and so on) are 238·075, 234·316, 266, 787, 252·19, 257·26, 262·06, 271·98. The probable mean (mean of six last successive means), may be taken as 262 (10 c. c. under the mean given by Lehmann). The mean variations from this quantity may be taken as 60 c. c.

The measured quantity of urine on each of the bath days was, during the six hours, 334·4, 674, 280, 200, 601, 364, 538·2, 663·2; the successive means are, 504·2, 427·1, 372·1, 417·7, 408·9, 433·8, 463·1. The probable mean may be taken as 431; the mean variation as 180.

The difference between 262 and 431, the received means of the two series, is very considerable, viz., 169 c. c. in favour of the bath days, and does at first sight appear to justify Lehmann's conclusion, but, owing to the great inequality of the quantities on different days, the range of error is so enormous (the variation being 60 + 180 = 240) as to render a certain conclusion quite impossible.

We will, then, pass on to further observations. Dr. J. Lehmann's second inquiry was into the effect of *warm* sitz-baths.* The experiments were made in a somewhat different manner. The amount of urine was determined every hour. As long as no food and no great exercise were taken, when nothing, in fact, disturbed the normal metamorphosis, the flow of urine was so stable, that Lehmann found the amount of any two hours multiplied by three, gave always very closely the amount for six hours. If now during such a stable excretion a fresh agent is employed, and if any great change, either in the urine or other excretions, occurs, it is but right to refer it to the action of the agent; and if in experiment after experiment the same result is found to occur, the cogency of proof becomes at last very great.

Conducted in this manner, Lehmann's experiments led him to the following conclusions: The waste of the body was increased by 15 minutes' sitz-bath of temperatures of 15° R. (65°·75 Fahr.), and of 25°—31° Reaumur (88° to 99°·5 Fahr.) and this waste was produced by an increased excretion of urinary water and of solids (urea, uric acid, fire-proof salts, and chlorine).† But the surprising fact was discovered that *baths of a medium temperature* (17°—25° R., 70°—88° Fahr.) *did not have this effect.* Excretion, therefore, according to Lehmann, is increased by sitz-baths below 70° Fahr., by sitz-baths above 88° Fahr., but not by baths between 70° and 88.°

We have already seen reason to call in question the experiments supposed to prove the first opinion, and the singular difference of effect attributed to lukewarm and hot baths, makes us very suspicious of this inquiry also. On going over the items of the experiments, however, we do not observe any obvious fallacy: six hours on five days were occupied with the hot-bath experiments; the excretion of the two first hours being taken as the standard : in all the cases the urinary flow was greatly and immediately augmented, rising from 30 or 50 c. c. per hour to 90, 100, or even 150 c. c. Nor for this great increase does there appear to have been any other cause than the baths. Yet it must be said that the experiments are not sufficiently numerous, and on this ground it is advisable to defer our judgment, and to consider the supposed eliminative effect of the hot baths as very probable, but as not proved.‡

Leaving, then, the question of cold and hot hip-baths, we will inquire what observations have lately been made on the effects of

2. *General Warm Baths of simple water.*—The indefatigable Dr. L. Lehmann s again the person who has most carefully investigated this point.§ A great num-

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* Ueber die Wirksamkeit 15—30·5 Reaumur warmer Sitz-Bäder. Archiv des Vereins für wiss. Heilk. Band ii. p. 1.

† Lehmann states (p. 19) that the insensible perspiration (skin and lungs) is also increased, but this does not appear from the table given at p. 10; the mean amount of insensible perspiration is even a little less with baths of 25—30° ·5R. than without baths.

‡ In this paper Lehmann records some careful experiments on the much debated question whether water is absorbed by the skin; and to show the discrepancies of experiment and opinion even on this comparatively simple point, we may be permitted to quote his and other late experiments. In order to avoid all the usual fallacies, Lehmann prepared two baths containing an equal quantity of weighed water; one of these only was used; the body was wiped with a weighed cloth, which was again weighed immediately; then the two baths were weighed, the loss of weight of the unused bath giving the amount of water lost by evaporation. Lehmann found that the absorption of water was extremely small; there was a mean loss of water from the bath of only 7·5 grammes (116 grains) out of 50lbs. of water, a quantity equivalent only to one three thousand three hundred and thirty-third of the entire bulk.

The late experiments of Poulet (L'Union Méd., 1856, No. 33, and Schmidt's Jahrb. 1856, Sept, p. 278) accord with those of Lehmann. No water seemed to be absorbed through the skin, although both cuticle and hair are hygroscopic, and take up some water. On the other hand, Durian (Archives Gén. de Méd.' Fév. 1856, p. 156), who distinguishes carefully between the hygroscopic swelling of the integuments and the actual absorption of water, believes that water may be absorbed by the skin under certain circumstances. There is a point, he thinks (different in different men), when elimination from, and absorption by, the skin exactly balance each other; in all warm baths, elimination by the skin surpasses absorption; in cold, the reverse is the case. Vierordt, again (Phys. Mitthielungen, Archiv für phys. Heilk., 1856, p. 575), believes that some experiments undertaken under his direction by Eichberg prove the absorption. To avoid fallacies, an *arm-bath* of distilled water only was used; the water and the towel used for drying the arm were weighed before and after the experiment, and every precaution was taken to prevent evaporation. Absorption from hot baths was held to be decidedly proved, and the amount for the whole body was calculated at 200 grammes (6¾ fld. ozs.) per hour. At low temperatures the absorption was much less, indeed trifling. These experiments will show what extraordinary diversities of opinion still exist on this point.

§ Die Soolhtherme zu Bad Oeynhausen (Rehme) und das gewöhnliche Wasser. Von Dr. L. Lehmann. Göttingen, 1856.

ber of experiments have been made, and the results of the last series have been calculated according to mathematical rules. We will quote only the last series of experiments,[*] which consisted of observations on ten days without, and on ten with, baths; all precautions were taken, as in the former experiments; fasting hours were chosen for the bath; the whole ingesta and egesta of twenty-four hours were measured, and the conditions of the external temperature and barometric pressure were noted.

The general results of the experiments were—the water of the urine was increased by the general warm bath to a very great extent (89° Fahr.; time of bath, half an hour) *for the time*—but the amount for *twenty-four hours* was not increased, so that the diuresis caused by the bath must have been compensated by lessened excretion afterwards. The figures proving this conclusion, seem to be satisfactory, if experiments for twenty days are to be considered sufficient, and after carefully considering them, we believe that Lehmann may be considered to have made out this point. The amount of temporary increase was very great, so that if the normal hourly excretion be taken as 1, after the bath it became as 3·9.

Some experiments by Nasse[†] entirely agree with the above, though they are not so free from possible error. The augmentation of the urine was for a time increased even fourfold. Poulet also found the water increased.[‡]

The effect on the solids of the urine cannot be held to be yet determined; for although many experiments were made by Lehmann, the results of which are tolerably accordant, yet in so difficult a point much more evidence is required. Lehmann, indeed, at one time believed he had proved a decided increase in the elimination of nitrogen (as urea); but in his last paper, in speaking of urea, he qualifies this, and says that "these experiments are not yet sufficiently numerous to affirm with certainty that the urea is increased after customary baths, but such an increase is probable."[§] The change in the other constituents is not more certain, and in fact it is not safe at present to come to any conclusion as to the urinary solids more definite than that given above by Lehmann.

The acid reaction of the urine is said by Homolle[‖] to be diminished after all but very hot baths, and other observers have noted the same thing. It is very desirable that a repetition of experiments should decide this point, as it is not possible to determine the effect on the acidity of the urine of medicinal baths until that of simple water is known.

The condition of the insensible perspiration must of course be considered undecided, but it is probably increased.

Whatever may be the immediate effect of the bath in increasing the loss of body-weight during fasting, from increased discharge of urinary water, and perhaps of solids, and from increased cutaneous transpiration, it does not cause the body to lose weight in a cycle of twenty-four hours. When on a regulated weighed diet, Lehmann lost during ten days 1235 grammes (43½ ozs. av.); with warm baths he lost during an equal time, and with the equal diet, only 805 grammes (28½ ozs. av.)[¶]

General Cold Baths, as far as is at present known, have the same effect as warm baths—*i. e.*, they temporarily increase the flow of urinary water, and perhaps the excretion of solids;[**] they are said, however, to differ from cold sitz-baths in lessening instead of increasing the "insensible perspiration" (skin and lung excretions). The evidence on this point is, however, decidedly incomplete.[**]

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[*] Archiv des Vereins für wiss. Heilk. 1858, Band iv. p. 18. For some objections to Lehmann, and his reply, see Deutsche Klinik, 1857, Nos. 10, 11, 22, 24, 25.

[†] Einige Beobachtungen über die Wirkung warmer Bäder auf die Harnabsonderung von H. Nasse; Archiv des Vereins für wiss. Heilk., Band ii. p. 265.

[‡] Falck did not, however, find the same effect, but as his experiments are unknown to us, and as, indeed, we believe the details have not been published, we shall not place them in opposition to Lehmann's strong testimony.

[§] Archiv des Vereins, Band iv. p. 30.

[‖] L'Union Méd., 1858, p. 177.

[¶] Archiv des Vereins, Band iv. p. 35.

[**] Lehmann in his earlier work (Bad Oeynhausen, p. 29), found the urea, uric acid, sulphuric acid, phosphoric acid, chlorine, and the alkalies all increased by the general cold bath; the extractives and pigment were lessened. There was increased loss of body-weight from the use of the bath. Further experiments, however, are wanted before these statements can be accepted as basis for reasoning.

[‡] In this stage of the inquiry we do not think it worth while to discuss the modus operandi of baths; Lehmann,

3. *Natural Saline Baths.*—The effect of *natural saline baths* has been investigated by Neubauer[*] and by Lehmann. Neubauer's experiments were made with the warm saline baths of Wiesbaden,[†] and were conducted on himself and on Dr. Genth. The results of the experiments on the two persons differed greatly. In Neubauer's own case the bath for half an hour increased the water, the urea, the uric acid, and the free acidity; and in a much less degree the sulphuric and phosphoric acids, the lime, magnesia, and ammoniacal compounds in twenty-four hours.[‡] When in addition to the bath the saline water was drunk (500 c. c., equal to 17 fl. ozs. nearly), the excretion of the solids was still more increased.[§] The effect, therefore, of the Wiesbaden baths was, in Neubauer's case, apparently a highly eliminative one, if the experiments can be trusted. We shall not enter into a critical examination of them, but content ourselves with the statement, that they appear to us to be too few in number to warrant us in at once receiving them. We are the more confirmed in this, because the experiments on Dr. Genth were not accordant with those on Neubauer himself. The bathing, in Genth's case, lessened, instead of increasing the urea and the sulphuric acid, and augmented only very slightly and inconsiderably the water, the uric acid, the phosphoric acid, and the ammonia. Drinking the water with the baths increased of course the water of the urine; but the urea and the sulphuric acid remained still below their healthy amount, though their amount was greater than with the baths alone; the uric acid, which was slightly increased by the baths alone, was actually lessened when the water was also drunk.

It is therefore evident that the apparently disintegrating or simply eliminative effect of the baths, as shown by the experiments on Neubauer himself, were either attributable to other causes, or that an individual peculiarity of constitution in Genth prevented these effects from being manifested in him. It may certainly be admitted that the resistance (so to speak) offered by the system to the action of remedies is very different in different persons, and that in some, chemical stability is greater than in others, so that remedies produce less chemical action and of course a less marked effect. We should be sorry to admit, however, without cogent evidence, that this has any very great influence; were it so, it would be almost hopeless to attempt the elucidation of the chemical action of agents on the body.

The action of the Sooltherme (chloride of sodium chiefly) of Oeynhausen has been examined by Julius Lehmann;[‖] the result accords rather with the experiments on Genth than with those of Neubauer. When a single bath was taken, the urinary water and solids were increased, but in a much less degree than by simple water; and when the baths were continued for nine days regularly, the elimination was found to be actually lessened by the bath. During the non-bathing period the daily amount of ingesta was 2038 grammes (70½ ozs. av.), of egesta 2147 grammes (76 ozs. av.); during the bathing period the mean daily ingesta were 2079 grammes (73 ozs. av.), of egesta 2072 grammes (73 ozs. av.) Thus, from some unexplained cause, there was during the non-bathing period a daily loss of weight arising from an excess of excretion over ingesta, which excess was *arrested by the bath*, and this arrest was found to be owing to very considerable lessening in the

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in his first paper, attributed the (presumed) eliminative effects of *cold* sitz-baths to the abstraction of heat, which caused an increased activity of the chemical processes as compensation. But when he found a similar effect produced by *hot* sitz-baths, he was obliged to give up this hypothesis, and fell back on a commonly received opinion, that the effect is produced by "irritation" of the cutaneous nerves. Nor do we think it necessary to discuss at present whether the increase of urea is owing to augmented metamorphosis, or merely to augmented elimination. Further proof must first be given of the fact.

[*] Archiv des Vereins für wiss. Heilk., Band iii. p. 59.

[†] These are chiefly chloride of sodium baths (6·83565 parts in 1000 parts), but the water also contains a little chloride of potassium, chloride of lithium, of ammonium, of calcium, of magnesium; a little bromide of magnesium, and traces of iodide; they contain also carbonates of lime, magnesia, baryta, strontia, iron, and manganese, dissolved by carbonic acid; there is also some free carbonic acid, and a little nitrogen.

[‡] The exact mean increase in twenty-four hours was—

Water	294 c. c.
Solids	10·928 grammes.
Urea	6·570 "

[§] The internal use of the water, with coincident exercise, increased in Neubauer the excretion of the skin and lungs considerably.

[‖] Die Sooltherme zu Bad Oeynhausen (Rehme) und das gewohnliche Wasser. Gött. 1856.

urinary excretion,[*] for the insensible perspiration (skin and lungs) was increased 12 per cent., and the intestinal excreta were augmented from 149 (5 ozs. av. nearly) to 195 grammes ($6\frac{7}{8}$ ozs. av.) daily.

The effect, then, of this bathing was to augment three excretions, to lessen one; and the lessening of this one was sufficient to reduce the amount of the egesta below that of the ingesta, and to cause the body to gain weight. Lehmann believes that the assimilative and formative processes were increased in energy, and that many of the eliminative processes were lessened by the use of the bath. Whether this be so, or whether disintegration or excretion were merely checked without augmentation of formation of tissue, cannot yet be known. After the bath was left off, the urinary solids increased above the average even of the ante-bath period; this did not depend on great lessening of the cutaneous excretion, for this remained still somewhat increased, and the explanation of the fact is not easy,[†] unless the bath had merely checked excretion for the time.

It remains, of course, to inquire what credit is to be given to these elaborate experiments of Lehmann, of which our space compels us merely to give an outline. The most obvious objection is, that before the experiments the body was not in a very fit condition—it was losing weight considerably, so that something was amiss either with assimilation or disintegration. Otherwise, the experiments seem numerous enough to permit us to attach considerable weight to them.

On the whole, perhaps, it may be safely concluded that the natural saline warm baths differ from simple warm-water baths in causing a much greater diaphoretic, and a much less diuretic effect. They have not apparently the eliminative action over the urea which warm simple-water baths *probably* possess; but, on the contrary, they rather seem to aid assimilation and to lessen the exit of nitrogen from the system. If these conclusions be correct, therapeutical rules for the use of these baths at once arise, and we trust that confirmation, or the reverse, of these interesting experiments will soon be given us.

4. *Sea-bathing.*—The effect of *sea-baths* (and sea air) has been investigated by Beneke, and has been already reviewed by us.[‡] We notice it here merely to remark that Radiche has shown some of Beneke's conclusions to be not warranted by his own figures, when these are subjected to a rigid analysis. No doubt Beneke will, as soon as he can, repeat his experiments.

5. *Cold wet sheet.*—The result of the application of the *cold wet sheet* has been inquired into by Wundt.[§] Two women who were being treated for slight hysteria in the clinic of Hasse at Heidelburg were selected for the experiments. The wet sheet was applied for four hours; the body was found to lose weight faster with, than without, the sheet; the excreta were in fact almost doubled, and this was chiefly shown in the water, the urea, and the chloride of sodium of the urine. The cutaneous exhalation was sometimes slightly increased, sometimes lessened, and Wundt believes that water was absorbed through the skin. The effect on the skin was much less marked than the previous statements of the effect of the sheet would have led us to anticipate.

When the sheet was continued for six hours, the effect was comparatively less marked, as if the first application of the sheet was followed by more energetic action.

Until these experiments have been confirmed, it would be quite premature to speculate on the mode of action of the wet sheet.

6. *Medicinal Baths.*—No one has yet investigated, to our knowledge, the quantitative influence on the excreta of *medicinal water baths*. Nasse states that a warm bath containing carbonate of potash increased the urinary flow to double; but as this effect is also produced by simple water, it does not follow that the carbonate of potash had any effect.

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[*] The water of the urine was lessened by 163 c. c. (5 fld. ozs.), and the solids by 8 grammes ($46\frac{1}{3}$ grains) daily.

[†] One curious fact was noticed, that during the bath the alkaline phosphates of the urine were increased, but the phosphate of lime was much lessened. As the phosphate of lime is supposed by many to be especially connected with increased tissue-building, Lehmann supposes that this fact strengthens the notion that assimilation was improved.

[‡] Vol. xvii.

[§] Archiv des Vereins für wiss. Heilk., Band iii. p. 85.

It seems clear that very few, if any, medicinal substances are absorbed through the skin when applied in the form of baths. To refer only to recent experiments: Homolle could detect no trace of iodine after baths of iodide of potassium, or of cyanurets or hydrocyanic acid, after baths of ferrocyanide of potassium. He states, however, that potash increases in the urine after baths of carbonate of potash. The urinary chlorides were not increased, but the reverse after a bath of chloride of sodium. Kletzinsky[*] could not detect in the urine any of the following substances after bathing in solutions—iodide of potassium, ferrocyanide of potassium, sulphate of magnesia, borax, acetate of lead, nitric acid, sulphuret of potassium.

Durian[†] could not detect the entrance of iodide of potassium, carbonate of potash, ferrocyanide of potassium, rock salt, nitre, alum, sulphate of magnesia, sulphate of quinine, infusion of digitalis, and infusion of belladonna.[‡]

Braune[§] could detect no iodine in the urine after foot-baths of iodide of potassium and of iodine, when the evaporation of iodine and absorption through the lungs were prevented by a layer of oil on the water of the bath.

Although all these recent experiments agree in the fact that no absorption of these substances can be proved, it does not follow that no effect is produced on the urine. It is a curious fact, that the acidity of the urine is often very much altered.

Durand Fardel[‖] found the urine to be alkaline during and after the use of the Vichy baths (bicarbonate of soda), and Musset has observed the same fact from the Plombières water.[¶]

It might be supposed that this was owing to actual absorption of those alkaline waters; but the same fact was noticed by Homolle after baths of chloride of sodium, iodide of potassium, and ferrocyanide of potassium. The alkalinity was even sometimes greater than after an alkaline bath.

Poulet has indeed noticed the production of alkaline urine after *acid baths*, so that we must look to some other cause, probably to some special action on the skin, rather than to absorption of alkali.

The decrease of acidity (though not alkalinity) was noticed by Spengler[**] from the baths of Ems. Absolute alkalinity of the urine has been found by Durian after baths of iodide of potassium (200 grammes in cold and warm baths for two hours); of carbonate of soda (230 grammes); of carbonate of potash (230 grammes); of ferrocyanide of potassium, of chloride of sodium, of nitre, of sulphate of magnesia, alum, nitric acid (200 grammes in bath); sulphate of quinine and infusion of belladonna (2 lbs. of the leaves).

On the other hand, sea bathing increases the acidity (Beneke), and so do also the Wiesbaden saline baths (Neubauer). The Oeynhausen baths produce little effect, but rather tend to increase the acidity than otherwise (L. Lehmann). The action of these natural baths contrasts therefore in this respect with medicinal baths of chloride of sodium.

The effect of these various substances on the solids of the urine, and on the cutaneous, pulmonary, and intestinal excreta, has not, to our knowledge, been yet made out.

We have brought these fragmentary contributions to the important subject of Balneology before our readers, not with the hope that by their means we can build up a complete theory of the influence of the baths, nor with the idea that it would be wise at present to attempt such a thing. But as there seems to be a disposition to prosecute these inquiries, and as the experiments are constantly increasing in accuracy and extent, we have thought that it might be interesting merely to record what has hitherto been done, without seeking to enunciate anything like decided conclusions, so that at a future time we may be able to refer to this article, and to use or correct its details, without having the necessity of again stating them. We have not alluded to the influence of baths on the pulse and respiration—an important point, which has been investigated by Sieveking, Beneke, Wiedasch and others—as this also will be considered with greater propriety when we are in a position to speak positively as to the chemical results of baths on the various excreta.

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[*] Canstatt's Jahresberichte for 1858. Löschner's Report on Mineral Waters, p. 96.

[†] Archives Gén., Fév. 1856.

[‡] Archiv für path. Anat., Band xi. p. 295.

[§] As noticed by Meissner, however, all these experiments were made at temperatures which Durian himself considers unfavourable for absorption.

[‖] De l'alcalisation de l'urine, chez les malades au traitement thermal de Vichy—L'Union Méd., 1858, Nos. 18-20.

[¶] L'Union Méd., 1858, No. 34.

[**] Ueber die Absorption in den Bädern zu Ems, Deutsche Klinik, 1854. No. 22.

We may be permitted, however, to make a few statements, which these late investigations into the use of baths seems to warrant.

Sitz Baths:—

1. It is probable that cold and hot hip-baths of simple water increase for the time the flow of urine; but it would appear that baths of a medium temperature (70°—88° Fahr.) do not have this effect. The effect apparently does not arise from absorption of water or impairment of cutaneous transpiration.
2. It is probable that the cold and hot-hip baths augment the elimination of urea for the time, and perhaps the uric acid, the chloride of sodium; but the experiments, though numerous, and accordant, require to be repeated, and the amount of the other urinary constituents cannot be held to be at all determined.
3. The experiments are not sufficiently precise to prove the effect of the baths on the "insensible perspiration" (skin and lungs): as far as they go they indicate an increase.
4. The effect on the intestinal excretion is also uncertain, but there would appear to be here also an augmentation.

Altogether, therefore, it would seem probable that these baths have an eliminative and (as all the excretions are increased) disintegrating influence.

General Simple Water Baths:—

1. The water of the urine is temporarily increased by cold and hot baths.
2. The acidity of the urine is diminished.
3. An alteration in the excretion of urinary solids is doubtful, but it is *probable* that the urea is increased; the amount of the other solids is uncertain.
4. The effect on the "insensible perspiration" is doubtful, but it is probably somewhat increased by the warm baths (increase of cutaneous transpiration).
5. The intestinal excreta are not very much affected.

Altogether, the effect of the general hot and cold baths is very similar to that of the hip-baths. As far as can at present be seen, it is eliminative, and perhaps disintegrating.

Natural Saline Warm Baths (Wiesbaden and Oeynhausen; chiefly chloride of sodium):—

1. The water and solids of the urine are probably not increased; are indeed in some instances decidedly lessened.
2. The acidity is increased (Wiesbaden) or unaffected (Oeyenhausen).
3. The solids of the urine are lessened in amount.
4. The "insensible perspiration" is greatly augmented.
5. The intestinal excretion is increased.

These baths check urinary, but augment cutaneous and intestinal, excretions.

Medicinal Baths:—

1. The substances dissolved in the baths are not absorbed through the skin.
2. The acidity of the urine is lessened as a rule; often, complete alkalinity is produced; and this is not apparently attributable to the alkalinity of the baths.
3. The effect on the constituent parts of the various excreta is not known.

The chief point which seems to flow from these general conclusions is, the antagonism between warm baths of simple water, and the natural saline (chloride of sodium) water baths. The former are probably diuretic, and moderately only diaphoretic. The latter (if Lehmann's experiments may be received as the rule, and Neubauer's experiments be considered exceptional) are especially diaphoretic, and not at all diuretic, but even the reverse. As far, therefore, as the experiments enable us to see, the simple warm water baths are chiefly eliminative by the urine, the saline baths by the skin; and the effect of the former is sufficient to cause for the time increased wasting of the body, while that of the latter may, in spite of the diaphoresis, check urinary excretion sufficiently to arrest permanently a loss of weight going on in the system. The one set of baths are, therefore, weakening; the others strengthening. Further than this, facts do not warrant our proceeding, and we must leave for future and more rigorous investigation an exact determination of the effect produced on the individual ingredients of the various excreta.

In conclusion, we would direct the attention of all those engaged in these inquiries to Professor Radiche's most important paper. Unless calculations are properly made, the most accurate experiments will only lead to fallacies. We can perhaps never reach mathematical certainty in such inquiries, but we can give to them a much higher degree of probability than they have yet possessed, by a rigid analysis of the numerical results.

REVIEW XII.

Nutrition in Health and Disease. By JAMES HENRY BENNET, M.D., Licentiate of the Royal College of Physicians, Physician Accoucheur to the Royal Free Hospital, &c.—*London.* 8vo, pp. 220.

THERE is a class of publications, very numerous in the present day, in which physicians take occasion to edify the laity with a little physiology and a few pathological and practical views not usually of a very original kind—the real practical view being that the said laity, struck with what appears to them as a very clever performance, should consult the writer whenever they think that their own cases come within the scope of his observations. We should have been inclined to refer the book before us to this department of medical literature, had not the author announced what he seems to consider as an especial and important object of its publication. At the beginning of the preface he says:—

> "My object in writing the following work has been forcibly to draw attention to the fact, often overlooked, that the imperfect performance of the digestive and nutritive functions leads slowly, but surely, to ill health, to disease, and to death."

Further on, he adds:

> "I trust that I shall not be considered presumptuous if I express the hope that this little work may contribute to convince my medical brethren of the imperative necessity of studying dietetics in connexion with chemistry and physiology."

Now, if Dr. Bennet could keep his countenance while he was writing these passages, it was more than we have been able to do while reading them.

We should have supposed that no person, whether of the medical profession or not, who was sufficiently educated to understand the general meaning of the words "digestion" and "nutrition," could be so stupid as not to perceive the consequences here ascribed to the imperfect performance of these functions.

Again, what benighted portion of our fraternity are studying dietetics otherwise than in connexion with chemistry and physiology?

Our author seems to have a wonderful propensity to create errors for the purpose of demolishing them. Thus he tells us:

> "The general impression, not only with the public, but with many members of the medical profession, appears to be that nitrogenous food and stimulants are synonymous with assimilation and strength. The undeniable fact that between the two lies a gulph, occupied by all the varied digestive processes, the imperfection of any one of which neutralizes the result—healthy nutrition—is thus overlooked." (pp. 60, 61.)

We submit that this is not the impression with many members of the medical profession, nor with any members of the same who have not taken leave of their wits. Neither is it the impression with the public. Did Dr. Bennet never hear the popular allusion to food going into an "ill skin," where the recipient remained lean, and poor in condition, notwithstanding the ingestion of all manner of good things? Now this "ill skin" is merely a homely kind of metaphor, expressive of defect in the digestive and assimilative functions.

We may now proceed to some general notice of the contents of Dr. Bennet's book. The first chapter is on "Digestion and Nutrition in Health," and contains a fair view of the commonplaces of the subject; but neither this nor the following, "on Nutrition, considered generally," presents anything requiring especial comment. Chapter III. is on the "Nutritive Requirements in Man, considered with reference to Temperature, Climate, and Social Occupation." Here also the scientific physiologist will not meet with much to detain him, though the popular reader may find some information. There is one passage of startling interest to ladies who have attained to "what certain people call a certain age," with an accompanying degree of *embonpoint*. In their case the fat

> "Stretches the yielding skin, and thus conceals the ravages of time, the results of diminishing nutritive power. To many women this change constitutes a second youth, and may even impart to them a charm and loveliness which they never presented in their earlier age." (p. 66.)

So far, so good—but evil days are to come:

> "The deposit of fat often continues, especially when the tendency is constitutional, or the diet liberal, and the habits of life indolent. In such cases it may increase by degrees, until the abdomen becomes protuberant, the hips massive, and until the chin, neck, and shoulders blend into one." (p. 66.)

A sad state of affairs indeed, in which the human shows an alarming tendency to retrograde into the arachnoid type!

Chapter IV. is on "Defective Nutrition," and the section of it which most claims our attention is that on *Urinary Deposits*. On this subject Dr. Bennet's views are rather peculiar in more ways than one; and we shall here notice them as they occur throughout the work, without confining ourselves to this particular chapter and section. He has arrived, it appears, at a general conclusion, which may, perhaps, be best enunciated by an extract from the preface:

> "There is one point on which I join issue with many who have specially treated of urinary deposits. I believe that too much importance has been attached to the *differential diagnosis* of the different morbid salts which are found in the urine as a result of disordered digestion and nutrition. I attribute even more importance to the presence of these deposits, as evidences of perturbation of the digestive and nutritive processes, than is usually attributed to them; but after many years' research, I have not been able to establish to my own satisfaction that the different morbid salts have always, or indeed generally, a different pathological meaning. It appears to me that, in disordered nutritive states, all, or nearly all, may occur, and constantly do occur, under the same circumstances."

Our author here speaks of "many years of research," and in other places he alludes to his own "experiments" and "observations" on this subject; but he has nowhere condescended to enter into any particulars respecting these researches, experiments, and observations: and really, when a man differs on such very general grounds, from high authorities, who are backed by elaborate and detailed experiments and observations, we must be excused from attaching any importance to his opinions. There is a way of simplifying any subject, by denying all that has been said upon it; but such a procedure would soon reduce all science to a negation.

Another important point on which Dr. Bennet entertains peculiar views is the amount of scientific knowledge requisite for the examination of the urine in reference to pathological inquiries; and here also his tendencies are in the negative direction.—"Divested of minute scientific development, that examination, as we have seen, is a most simple matter, and does not require any very extensive knowledge either of chemistry or of the microscope." (p. 126.)

But, as if startled at his own assertion, he endeavours to modify it in some degree by the following truism—"The greater the knowledge possessed, however, the more certain are the results obtained; and there is therefore every inducement to the student and the practitioner to pursue their researches."

At page 117, he has the temerity to affirm that a practical acquaintance with the principal salts and morbid products revealed by the microscope may be acquired in a few hours!

We have always regarded the analysis of urinary deposits as requiring extensive and accurate chemical knowledge. Again, we believe that the very first requisite for *any* effective use of the microscope is a thorough practical acquaintance with the instrument—its construction, powers, and management, and we are convinced that, in the absence of such knowledge, the results obtained are very likely to be a series of blunders, often of a ludicrous description. We must therefore express our entire mistrust of all examinations of the urine "divested of minute scientific development;" and we cannot help thinking that if Dr. Bennet will bring to his aid a little more chemical and microscopical science, he may arrive at conclusions more satisfactory (to the profession, at least) than the generalities with which he appears at present to be so well contented.

Such, however, being the small amount of scientific acquirement which our author deems essential to the investigation of urinary deposits, we do not wonder at his being occasionally disposed to delegate the inquiry into the patient's own hands. After occupying some pages with remarks on the variety of constitution in different individuals, respecting the kinds of food which agree best, the frequency of meals, and the time of day at which these may be most advantageously taken, and after stating that the inspection of the urine will throw important light on such points, and that the presence or absence of urate of ammonia in this fluid is the most delicate test of good or bad digestion, he proceeds as follows:—

> "The above facts, which are deduced from the careful investigation, during many years, of the urine of large numbers of dyspeptic patients, show clearly that it is impossible to lay down general dietetic rules: each case must be studied by itself, and the advice given must be modified according to the results of the study of each individual. The existence of urate of ammonia in the urine a few hours after the ingestion of food, presents, however, in most cases of disordered digestion, an easy means of arriving at the required knowledge. The circumstance of this salt rendering the urine turbid when it cools, makes it quite possible for the patient himself to carry on the investigation, once the physician has ascertained that the turbidity is owing to the presence of a lithatic deposit, and not to pus, &c. Thus an Ariadne thread is placed in the hands of the dyspeptic patient, which may enable him, with some slight assistance from his medical attendant, in the way of explanation and direction, to guide his own path though the dietetic labyrinth. He may thus learn to a certain extent what kind of food suits him individually, what amount he can take, and at what intervals and hours it is required in his own particular case: should, however, uric acid, oxalate of lime, or the triple phosphates be present without the urate of ammonia, this information could of course only be obtained by microscopic examination. The same mode of study may be applied to beverages, and their influence on digestion. If alcoholic stimulants are beneficial, they will not disturb digestion, and the urine will remain as clear, as free from morbid deposits, as if they had not been taken; but if, on the contrary—as sometimes occurs, even with the healthy and very frequently with the weak and dyspeptic—beer or spirits, even when taken in moderation, render the urine turbid and lithatic, evidently disturbing digestion, they assuredly can do no good: indeed, far from doing good, they are a snare and a delusion, owing to the temporary feelings of strength and comfort which they give rise to at the very time that they are in reality poisoning the economy." (pp. 153—155.)

We consider this as a very first-rate specimen of *ad captandum* writing. We fancy we see some elderly gentlewoman anxiously poring over the oracular fluid—calling in the aid of the microscope, the management of which has been represented as so simple and easy—and, when she has puzzled and frightened herself nearly into fits, sending for her medical attendant to give some slight assistance. Of course, such assistance in making out the urinary deposits will be accompanied with the deposit of a fee in the palm of the obliging gentleman who renders it.

By the way, a Greek motto is an imposing sort of thing; we will suggest one to our author for the title-page of his next edition, which aptly associates the pleasures of the table with the means of inquiry into their effects on the system:

Ἔγχει πιεῖν μοι· καὶ τὰ πέρδικος σκέλος,
Ἀμιδα δότω τὶς·*

Jesting apart, we would earnestly recommend Dr. Bennet, and all who profess to have the honour of the profession and the welfare of the public at heart, neither to instigate nor to encourage the laity to dabble in medicine, always excepting that useful common-sense kind of medicine which every good housewife knows how to administer without any lessons from the doctor.

The extract just made, though germane to the matter of the fourth chapter, is taken from the fifth, which is headed "Practical Deductions." On the contents of this chapter we have no particular remarks to make; neither does the sixth and concluding chapter, on "Confirmed Dyspepsia," demand any especial notice; the topics commented on are those to be found in most treatises of a similar character to that now before us, and the views inculcated do not differ from those generally received.

In some parts of this book the writer falls into inaccuracies from which a moment's reflection would have preserved him, and makes statements which are at variance with common observation. For example, at p. 33, we are told that "The animal creation are satisfied with water; indeed they show dislike and repugnance to all other beverages." Whereas many horses will drink wine and malt liquors with avidity, some monkeys delight in strong drinks, and a cat will seldom take water when it can get milk. The work abounds also with defects of style—the misuse of some words, and the employment of others which have no existence save in the writer's own vocabulary. Thus, at page 64, we meet with "*media* obtained by *acting* on numbers." These words, as they stand, are entirely destitute of meaning; but the writer evidently intends to express "averages obtained by the use of numbers." In several places he calls starch *amydon* instead of *amylum*—a mistake which shows an entire unacquaintance with the derivation of the word. The word *dietary* is continually used in the sense of *diet.* Nor do we approve of the employment of so obsolete and certainly strange-sounding a word as "activate," for which the author has a special penchant. We wonder the more at the occurrence of such strange mistakes as these, because many passages in the book are written in a correct, easy, and agreeable style.

On the whole, Dr. Bennet's work may afford some interesting information to the general reader, for whom it appears to be chiefly intended. If we have treated it for the most part in rather a jocose manner, this has been in order to avoid the censure which a graver criticism might have called upon us to pronounce.

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* Athenæus Deipnosophist, lib. i.

Review XIII.

1. *A Treatise on the Pathology of the Urine, including a Complete Guide to its Analysis.* By J. L. W. Thudichum, M.D.—*London*, 1858.
2. *Urinary Deposits—their Diagnosis, Pathology, and Therapeutical Indications.* By Golding Bird, M.D., F.R.S. Fifth Edition. Edited by Edmund Lloyd Birkett, M.D., Fellow of the Royal College of Physicians, &c.—*London*, 1857.

Some months ago we purposed drawing up a notice of several theses which had recently been published on various points connected with the physiology and pathology of the urine, and we had begun to arrange our scattered materials when we received the volume which stands first at the head of the present article. A perusal of Dr. Thudichum's volume at once showed us that he had been so carefully over the ground that we had intended to occupy, that there were little, if any, gleanings left us, and we have consequently abandoned our original intention, and shall content ourselves with the easier task of culling freely from his rich harvest.

His work, which extends over considerably more than four hundred pages, is divided into no less than forty-six chapters, the headings of which we shall briefly give, for the sake of affording our readers some idea of the extensive plan which the author has proposed for himself.

1. General characters of urine.
2. Quantity of urine and ingredients.
3. Urea.
4. Uric acid.
5. Creatine and creatinine.
6. Colouring matter.
7. Hippuric acid.
8. Chlorine and chlorides.
9. Sulphuric acid and sulphates.
10. Phosphoric acid and phosphates.
11. Free acid of the urine.
12. Potash and soda.
13. Lime and magnesia.
14. Iron.
15. Ammonia.
16. Carbonic acid.
17. Blood and its anatomical elements.
18. Hæmatine or hæmato-globuline.
19. Fibrine. Chylous urine.
20. Casts of uriniferous tubes.
21. Albumen.
22. Pus.
23. Mucus.
24. Fat and oil.
25. Cancer cells and tubercular matter.
26. Echinococcus hominis.
27. Spermatozoa.
28. Bile and biliary matters.
29. Leucine and tyrosine.
30. Xanthine.
31. Hypoxanthine.
32. Sarcine or sarkine (carnine).
33. Cystine.
34. Allantoine.
35. Grape sugar.
36. Acetone.
37. Inosite.
38. Urerythrine or purpurine.
39. Uroxanthine or Indican. Uroglaucine or indigo blue. Urrhodine or indigo red.
40. Phenylic or carbolic acid.
41. Damaluric acid.
42. Oxalic acid.
43. Lactic acid.
44. Urophanic organic acids.
45. Urophanic organic bases.
46. Urophanic inorganic substances.

In the "General Characters of the Urine" we have a full description of the various colours and tints that this fluid may assume, from which we extract the following remarks:

> "The urine may be very deeply tinted by the colouring principle of coffee, when a tolerably strong infusion of the unadulterated roasted berry is taken even in moderate quantity. The colouring matters of several drugs, such as the chimaphila or pyrola, hæmatoxylum, senna, rhubarb, enter the urine very readily, and [in] a short time after having been taken into the stomach. Urine coloured by rhubarb is sometimes mistaken for bilious urine. The error can be at once detected by the addition of liquor ammoniæ, which converts the dark orange into

a crimson colour. Vogel recommends the addition of mineral acids to the urine in cases where the presence of the pigments of either rhubarb or senna is suspected. The acids change the brownish or dark-red colour into a bright yellow; while uræmatine, if changed at all, is rather darkened by their influence. Black or blackish urine has several times been observed after the internal use of creasote and the inunction of tar over the whole surface of the body."*

Greenish or even grass-green urine has occasionally been found in cases of cystitis and Bright's disease, and blue or violet-coloured urine has more frequently been observed in the same diseases.

The odour of the urine is due to the presence of minute quantities of certain volatile acids, such as phenylic (or carbolic), taurylic, damaluric, and damolic acids, which have been detected by Städeler in the urinary secretion of man and cattle. "The odorous acids (occurring in the urine) develope their flavour on the tongue to great advantage!"—a statement that we are quite willing to take Dr. Thudichum's word for, without repeating the experiment.

The following points are of importance in connexion with alkaline urine. Ammoniacal urine is always fœtid, pale, and turbid, from the precipitation of triple phosphate and phosphate of lime. The smell, and the presence of the crystals of ammonio-phosphate, easily distinguish it from urine which is only turbid from alkalinity from fixed alkali, and contains a precipitate of phosphate of lime, or phosphate of lime and magnesia. Urine which is alkaline from the presence of bicarbonates (after Vichy water), is mostly clear, the earthy phosphates being soluble in the second equivalent of carbonic acid, which easily separates from the carbonate. A neutral or alkaline condition is frequently associated with the pale urine which is discharged in anæmia. The reason of the absence of acidity in these cases is not very well known, but it seems beyond all doubt that a constantly alkaline pale urine requires the employment of tonics, and especially of preparations of iron.

Between thirty and forty pages are devoted to the consideration of urea, all the best methods of testing for this substance being very fully noticed. We can only notice his remarks on an excess or deficiency of urea in disease.

Taking the normal daily quantity of urea at from 30 to 40 grammes (about 460 to 620 grains), we may observe that there is an excess in the *stadium incrementi* of all acute febrile diseases, such as typhus, pneumonia, &c.; while there is a diminution in diseases which are chronic, and accompanied by impaired nutrition. The lowest quantity which Dr. Thudichum ever observed to be discharged by a patient in twenty-four hours was 75 grains, in 200 fluid drachms of pale, faintly alkaline urine. The patient in question was a lady with ovarian tumour, who had become anæmiated after salivation. So low an amount as this generally only occurs towards the end of fatal diseases, when there is not only a diminished formation of urea in the system, but also a failure of the excretory powers of the kidneys.

In his remarks on the determination of uric acid in urine, he recommends that if the secretion be normal, and contain no albumen, nitric acid should be used in preference to hydrochloric acid. A temperature of 98° Fahr. has the advantage of not admitting the precipitation of any urates; and, further, crystals formed at this temperature are much larger and more easily collected than those produced at the ordinary temperature. He disapproves of the use of hydrochloric acid, because, 1st, uric acid is relatively soluble in it, and the accuracy of the result is thus affected; and because, 2ndly, hydrochloric acid favours the acid fermentation and the development of certain confervoid growths, which act as yeast-cells on the urates, and decompose them with great rapidity. If the urine contain albumen, acetic acid, or the common phosphoric acid, should be used.

The following form of urinary sediment will probably be new to most of our readers:

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* Thudichum's Treatise, &c., p. 4.

> "When appearing as a precipitate in alkaline (ammoniacal) urine, urate of ammonia forms very slender dumb-bells. This form I have met with in perfectly black alkaline urine from a child suffering from dropsy after scarlatina. It was mixed with rosettes and hexagonal plates of urate of soda, the deposit being perfectly black after filtration."*

The daily quantity of uric acid is liable to great variations, even in a state of perfect health. Our author gives 1·0 gramme as the maximum, 0·02 of a gramme as the minimum, and 0·5 of a gramme (or 7·72 grains) as the mean.

Patients are often unnecessarily alarmed by observing a deposit of urates in the urine. The indication of such a deposit, however, only becomes important when we take into consideration the total quantity of urine and of dissolved urates discharged in twenty-four hours. There are two cases to be considered.

> (*a*) "If the bulk of the urine for twenty-four hours is the normal average, and if a sediment of urates continues to exist in that urine, it is tolerably certain that an excess of urates is present."
>
> (*b*) "If, on the other hand, the urine for twenty-four hours is below the average, a deposit may possibly be, and in most cases is, due to saturation only. The easiest process of ascertaining this, for ordinary practical purposes at least, is to dilute the urine with water to its average bulk, and to shake it well. If the deposit does not entirely dissolve, an excess of urates is present."

Dr. Thudichum lays down the following therapeutic rule in connexion with the deposition of urates: The individual whose urine has deposited the urates does not drink water enough, and must drink more; and must drink so much that the urine (at the ordinary temperature of the air) shall remain clear.

As far as we know, our author is the first chemist who has determined the quantity of creatine and creatinine in the urine.

In the following table A and B represent two healthy persons (men), aged twenty-eight years, and weighing 154 lbs. and 159 lbs. respectively.

	No. of observations.	No. of days observed.	Mean daily creatinine in grammes.	Mean daily creatine, in grammes.
A	1	5	9·66	6·32
	2	4	5·61	4·68
	6	5	6·00	3·67
B	3	2	6·31	4·77
	4	5	3·66	3·45
	5	3	8·76	4·36

The statement in p. 120, that creatine is absent in the brain, is incorrect, if we are to trust the statement of Lerch, who asserts that he has obtained it from this organ.

The chapter on Hippuric Acid is confessedly unsatisfactory. It contains all, or nearly all, that is known on the subject, but leaves all the physiological and pathological difficulties untouched. Why, for instance, does hippuric acid occur in the urine of Herbivora and not of Carnivora? What is the source of the hippuric acid in the urine of the former? Why is it almost always in excess in the urine of diabetic patients, and in the very acid urine passed in some forms of fever? Why are no special symptoms associated with the presence of an excess of this constituent in the urine?

The only investigations of any importance on this subject which our author has omitted to notice, are those of Weismann, and those of Kühne and Hallwachs;† the two latter observers seem by their experiments to have established the view (originally propounded, we believe, by Duchek), that it is the glycine (glycocoll of some chemists) of the bile or of the liver which is converted, either within the liver or in the blood, into hippuric acid. Hallwachs‡ has subsequently published a Prize Thesis 'On the Origin of the Hippuric Acid in the Urine of the Herbivora,' which, however, is chiefly valuable for its negative results. He distinctly proves by a long series of carefully-conducted experiments and analyses, that the natural food of the Herbivora contains no benzoyl-compound (in the ordinary acceptation of the term), which would account for the formation of the hippuric acid, and thus establishes (as the adjudicators of the prize believe) the fact that the production of this acid is due not to any peculiarity of the food, but to some special mode to which their metamorphosis of tissue proceeds, probably to the slowness of this action as compared with the rapid destruction of the tissues in the carnivora.

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* Op. cit. pp. 88–89.

† Archiv für pathol. Anat. und Physiol., Band xii., p. 386.

‡ Ueber den Ursprung der Hippursäure im Harn der Pflanzenfresser. Preisschrift. Göttingen, 1857.

Weismann,* who competed with Hallwachs for the prize offered by the Medical Faculty of Georgia Augusta, and to whom a prize was likewise awarded, has also published his Thesis. The main point of his essay is to show that the formation of hippuric acid is due to the nature of the food, and that lignin is its most probable source, a view to which little weight can, we think, be attached. We extract the following remarks on the influence of the food on the amount of hippuric acid excreted daily by himself:

> "On a mixed diet my urine contained on an average (the mean of twenty-six observations) 0·145 per cent. of hippuric acid, and the quantity excreted in twenty-four hours averaged 2·473 grammes.
>
> "On a purely animal diet my urine contained on an average (mean of eight observations) 0·080 per cent. of hippuric acid; the mean daily quantity was only 0·765 of a gramme, and the amount fell after one day's animal diet to 0·798 of a gramme, the quantity on the previous day having been 1·808 grammes; and during the three days on which the purely animal diet was maintained the numbers were 0·798 of a gramme (on the first day), 0·723 of a gramme (on the second day), and 0·775 of a gramme (on the third day), so that there was no reason to believe that they would further diminish if the animal diet were longer continued.
>
> "On a bread diet, on which I subsisted for seven days, taking only bread and water, my urine contained on an average 0·070 per cent. of hippuric acid, and here also soon sunk to a fixed limit, beyond which it did not fall during the continuance of this diet.
>
> "In order to convince myself with certainty that no constituents of vegetable food retained in the body could have given rise to the formation of the hippuric acid during the above experiments on a purely animal and a purely bread diet, I frequently analysed the urine of typhus-patients, who for two to four weeks had taken nothing but milk and *bouillon*, and invariably found it to contain hippuric acid. The urine of these patients contained on an average (the mean of twelve observations on seven patients), 0·050 per cent. of hippuric acid."

Dr. Thudichum's next chapter is devoted to the consideration of chlorine and the chlorides. We extract the following remarks on the quantity of chlorine that is daily excreted by healthy persons under different physiological conditions:—

> "That the amount of chlorine discharged during twenty-four hours varies in different individuals, undoubtedly depends mainly upon the fact, that unequal amounts of chloride of sodium are ingested with the food of different persons. Sailors who have lived on salt rations for the greater part of their life spent afloat, will discharge an extraordinary amount of chlorine in their urine, because the ordinary food of our kitchen is insipid to them without the addition of an amount of salt that would make any ordinary person ill . . . . The amount of chlorine discharged by an individual varies on different days according to and corresponding with the amount of chloride of sodium taken with his food. When Falck ate strongly salt food on three successive days, he discharged the following respective amounts of chlorine—viz., 6·0, 7·8, and 10·3 grammes during twenty-four hours. But when he partook of food containing no addition of salt, he discharged 2·5, 1·6, and 0·9 grammes of chlorine on the three respective days succeeding the experiment. Professor Vogel observed the amount of chlorine discharged per hour by several individuals who had taken kitchen salt in doses not sufficiently large to purge them. In all the amount of chlorine discharged per hour was increased and rose from 0·4 to 1·0, nay, even 1·8 grammes."

As a general rule, it appears, from the researches of Hegar and others, that the urine is richest in chlorine a few hours after the largest meal of the day, and poor-est during the period of sleep; and further, that mental and bodily activity will increase the secretory activity for chlorine of the kidneys at any time during the day or night. Vogel has, however, shown, that by the ingestion into the system of large quantities of water, the amount of chlorine (like that of urine and urea) is increased.

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* Ueber den Ursprung der Hippursäure im Harn der Pflanzenfresser. Preisschrift. Göttingen, 1857.

Since the time of Redtenbacher's well-known discovery of the temporary disappearance of the chlorides in pneumonia, many observers have carefully noted the variations of these salts in various forms of disease.

> "The result (says our author) of many observations of Vogel and others, last of myself, is, that in all acute febrile diseases the amount of chlorine discharged in the urine sinks rapidly to a minimum, say one-hundredth part of the quantity normal to the individual, until at last in certain cases it disappears entirely for a short time. When the diseased action is abating, the amount of the chlorides rises during convalescence, sometimes above the normal average. We have already seen that the total quantity of urine has a similar relation to the stages of acute febrile diseases. But it is the reverse with the colouring matter or uræmatine, the amount of which rises and falls in the inverse ratio of the chlorine; so that when the latter is entirely absent, the former is discharged in the largest quantity. Urea, on the other hand, though rising at first in amount inversely to the sinking of the amount of chlorine, afterwards sinks below the healthy average, and during convalescence rises parallel with the amount of chlorine."

The following instance will show what variations may be expected in similar cases. In a man with severe pleuro-pneumonia, Vogel found that the total quantity of chlorine sunk to 0·6 gramme on the third day, to 0·3 gramme on the fourth, and on the fifth to almost nothing. From this date an improvement took place, and the chlorine rose on the succeeding days to 0·4, 1·8, 2·6, 5·5, 9·0, and 10·7 grammes, the latter being about the normal average.

Bronchitis, typhus, acute rheumatism, pyæmia, and pleurisy, are, in addition to pneumonia, diseases in which this variation of the chlorides has been especially noticed. In chronic diseases the excretion of chlorine is generally diminished, as might be expected from the bad nutrition, and generally poor appetite of patients of this class. Diabetes insipidus forms, however, an exception to this rule. In a case of this nature, Vogel found the amount of chlorine discharged by the urine to be occasionally increased to 29·0 grammes. The same excellent observer found that dropsical patients, under the influence of diuretics, discharged an increased quantity of chlorine, which must have passed (dissolved in the exudations, &c.) into the tissues and cavities. In dropsical or hydræmic cases an increase of the chlorides in the urine is a favourable symptom.

There is nothing in the chapter on sulphuric acid and the sulphates that need detain us, and we pass on to the consideration of phosphoric acid and the phosphates.

Taking the mean results of the analyses of Breed, Winter, Mosler, Neubauer, and Aubert, our author finds that the average amount of phosphoric acid discharged by an adult male in twenty-four hours is 3·66 grammes; and further, that there is a regular rise and fall in the hourly amount of phosphoric acid, the rise invariably taking place soon after the principal meal of the day. The maximum secretion was observed during the hours of the evening, the quantity falling during the night, and being at the minimum in the morning.

As the internal use of sulphur, sulphurets, sulphuric acid, and sulphates increases the quantity of sulphuric acid in the urine, so we find that the introduction into the body of phosphorus, either in the form of the acid, or of phosphates, or in combination with albuminous substances, gave rise to an increase of the phosphoric acid in the urine. It has been ascertained that total abstinence from food, or from food containing phosphorus, diminishes the amount of phosphoric acid, but does not cause its entire disappearance, the small persistent portion being probably due to the continuous oxidation of albuminous tissues.

The results regarding the quantity of phosphoric acid in the urine in disease, as determined by Vogel from more than one thousand observations, may be summed up in the following sentences:

> "In acute but not very severe diseases the amount of phosphoric acid in the urine decreases at first most probably in consequence of the low diet, and afterwards rises again with a more liberal allowance of food. During convalescence, the normal amount is sometimes exceeded in consequence of an increased quantity of food.
>
> "If the illness, though combined with violent fever, only lasts for a short time, the decrease of the phosphoric acid is sometimes very slight, and scarcely perceptible.
>
> "When the diseases are of a more severe nature, so as to cause a long abstinence from food, or to take a fatal turn, the decrease of the phosphoric acid in the urine becomes much more considerable.
>
> "In some exceptional cases the amount of phosphoric acid discharged during the height of acute diseases may considerably exceed the amount discharged during health.
>
> "In chronic diseases the excretion of phosphoric acid takes a very irregular course, and though remaining mostly below the normal average, may sometimes considerably exceed it."

There is nothing calling for any special remark in the two succeeding chapters on "The Free Acid of the Urine," and on "Potash and Soda."

The next chapter, on "Lime and Magnesia," includes the consideration of Deposits of Earthy Phosphates. We extract the following sentences in reference to this important subject:

> "As a rule, deposits of earthy phosphates can exist only in urines exerting an alkaline reaction upon test-paper. There is only one (questionable) case in which a deposit of an earthy phosphate is compatible with an acid reaction of the urine—namely, when urine containing little or no free acid exerts an acid reaction from the presence of chloride of ammonium. In this case a deposit of phosphate of magnesia may perhaps exist, for the salt is little or not soluble in chloride of ammonium. But phosphate of lime is so soluble in the latter salt that it could not exist as a deposit so long as any acidity of the chloride of ammonium is not neutralized. The observations which are said to have been made of urine having an acid reaction and yet containing a permanent deposit of phosphates (see Dr. G. Bird's 'Urinary Deposits,' p. 260, § 261), if they cannot be explained in the way just detailed, must be considered as fallacious. They are contrary to the commonest law of chemistry. I have made some observations which may serve to explain the manner in which such statements have come to be called observations. Clear acid urine was allowed to stand for three hours, when a pellicle of phosphates was observed on the surface. Blue test-paper, immersed an inch deep into the fluid, on being withdrawn, had become red. Another piece of the blue test-paper was now laid flat upon the surface of the fluid, when no reaction took place. The upper stratum of the urine had evidently become alkaline under the influence of the air, while the lower strata had retained their acidity."

The quantity of earthy phosphates normally discharged by the urine in twenty-four hours, has been determined by several observers. Beneke fixes it at 1·2 gramme for a healthy man; while Lehmann found that he discharged 1·09, and Böcker 1·48 grammes. We cannot, however, give any fixed average, as the quantity depends upon the amount of earthy matter taken in the food, and not discharged with the fæces.

Our author lays down the following general rules regarding the pathological indications afforded by the presence of these deposits:

> 1. "The presence of earthy phosphates in the urine is indicative of the alkaline condition of that fluid."
> 2. "If the precipitate of earthy phosphates is entirely amorphous, we may conclude that the alkali which formed it was not ammonia."
> 3. "If, however, the precipitate contains crystals of triple phosphate, it indicates the presence of ammonia, arising most probably from the decomposition of urea."

It has been long known that animal diet has a tendency to increase the acidity of the urine, while vegetable diet acts in the opposite manner. Dr. Thudichum lays great stress on this fact. The alkaline urine often noticed in aged paupers is, or may be, the consequence of a deficient supply of animal food, and we can very generally render their urine again acid by a proper allowance of meat. A similar treatment may often be successfully adopted in anæmia—a disease in which we usually find a deficiency of the free acid of the urine.

Passing over several chapters, we arrive at the consideration of albumen. Our author observes, that "when albumen is digested with permanganate of potash, urea is obtained as one of the products of oxidation;" and in his chapter on fibrine, he observes, that "Béchamp has produced urea from fibrin by oxidising agents, so that the ultimate fate of fibrine appears to be determined." We are sorry to be compelled to state that Städeler has very recently overthrown Béchamp's supposed discovery, and has shown (we fear, beyond a doubt) that he mistook benzoic acid, which would naturally be formed during the experiments, for urea. We regret that our limited space compels us to omit any notice of the section on the "pathological indications of albumen," which concludes this chapter.

The chapter on Grape-sugar is followed by one on Acetone, which has lately been discovered in the urine, blood, and most parts of the body of a diabetic patient. It is possible that the peculiar odour so frequently noticed in diabetic urine is due to the presence of small quantities of this substance. The suggestion that the odorous substance in question was acetone, is due to Dr. Lerch; the determination of it was made by Petters.

Many of our readers are probably not aware that indican, a gum-like vegetable substance, which when boiled with acids yields indigo blue, seems from the researches of Schunck and others to be generally present in small quantity in healthy human urine. The urines of forty different individuals, all of whom were apparently in a good state of health, yielded, with a single exception, more or less indigo blue. The persons in question were of both sexes, and their ages varied from seven to fifty-five; and diet seemed to produce no definite effect upon the quantity of indigo blue that occurred. The urine of the horse and cow (especially of the horse) gave comparatively very large quantities of this substance. It does not seem (from the recent investigations of Virchow) that the presence of this substance even in some excess, is indicative of any peculiar morbid state; concentration being apparently the main condition upon which its detection depends. It was formerly believed to be specially associated with albuminuria.

The chapter on "Oxalic acid" contains many original observations on various points connected with the physical and chemical characters of the different crystalline forms which we generally consider to pertain to oxalate of lime, and is well worthy of a careful perusal.

The volume concludes with a full consideration of what our author terms *urophanic substances*—those substances, namely, which pass through the body and reappear in the urine without undergoing decomposition. We shall take as our final extract, our author's remarks on the passage of strychnine into the urine, and the method to be employed for its detection.

> "Strychnine when introduced into the animal economy in any notable quantity—for example, in the ordinary medical doses at from one-tenth to one-twentieth of a grain, repeated at intervals, reappears in the urine. . . . In order to obtain strychnine from urine, it is only necessary to evaporate the fluid to the consistence of a thin syrup, to make it strongly alkaline by caustic potassa, and to shake it with large and repeated quantities of ether. The ethereal solution, which is only obtained after some standing, and sometimes only after the addition of some alcohol, is evaporated, and the residue treated with concentrated sulphuric acid on the water-bath. After several hours' digestion, the acid is neutralized by carbonate of soda, the fluid is then made alkaline, and again extracted with ether, which after evaporation will leave strychnine, to be tested by the taste, and by the reaction with bichromate of potassa and sulphuric acid. The latter reaction is best effected in the following manner. The solution in water of the supposed alkaloid is placed in a small china dish, and after evaporation to dryness at a low temperature, is dissolved in a drop or several drops of sulphuric acid. The solution is now spread over the space of about a fourpenny piece. A small granule of bichromate of potash is now dropped into the solution. On moving the fluid by giving the china dish different inclinations, violet streaks are perceived to flow from the granule of bichromate, and on moving the crystals to and fro in the fluid by means of a glass rod, the entire solution soon assumes a fine purple colour (Otto).

"One grain of a solution of strychnine, containing one forty-thousandth part of a grain of solid strychnine, yielded this test quite clearly. Five drops of the same solution brought upon the tongue, had a decidedly bitter taste; on some occasions, two or three drops would permit the bitterness to be recognised."

We ought to mention that the volumetric methods of testing for the various substances occurring in the urine, are very fully given in this volume: they are as yet little known or practised in this country, although in many cases they present great advantages over the ordinary modes of analysis.

When a new edition is called for, which we doubt not will soon be the case, we would suggest a revision of the references given in the foot-notes. Thus in pp. 15 and 77, there are notes referring to previously quoted works of Vogel and Gerhardt, yet where these works are previously quoted we cannot ascertain. There are also a few words occasionally met with which have as yet hardly established themselves in the English language, as for instance *hydrothion*, in p. 299, a substance familiar to our readers under the term of sulphuretted hydrogen. These are, however, very slight drawbacks, and upon the whole we may award our author great praise for his very painstaking and elaborate work. It is a perfect encyclopædia of all matters connected with the urine.

We regret that we cannot speak in equally favourable terms of the new edition of Dr. Golding Bird's "Urinary Deposits," edited by Dr. Birkett. While there is a considerable amount of new matter in this edition, there is a great deal of equally important matter of which no notice is taken, while a good deal that had better have been removed is retained.

A considerable amount of trouble and inconvenience arises from the fact that the reader has no means of deciding to whom the authorship of any particular part of the volume is due. By the use of brackets or initials, Dr. Birkett might easily have indicated what his additions really are. Let us turn, by way of illustration, to p. 47. Who is the "old pupil" who bears his humble testimony to the elegant scholarship of Dr. Keate? Is it Dr. Bird or Dr. Birkett? Is it, we ask, fair to leave the reader in doubt on so interesting and important a question?

In pp. 94, 95, it is almost impossible to know whose views are being expressed. We seem to have an inexplicable admixture of incongruous views. In the first place, we have a series of analyses of urinary sediments by Dr. Letheby. These analyses are succeeded by the natural remark, "that the deposits usually considered to consist principally of urate of ammonia, are in reality made up of urates of lime, potash, and soda. . . . As these views are ingenious" [they are better than ingenious, they have just been proved to be true in the preceding paragraphs], "and are still supported by some chemists, I have hesitated to remove them, though myself persuaded of their fallacy. I believe that the explanation of the proximate formation of these deposits is to be found in the action of uric acid on the microcosmic salt or double phosphate of soda and ammonia." If our readers can extract any clear ideas as to *who* is persuaded of the fallacy alluded to in the foregoing sentence, and why any one should hold views directly based on Dr. Letheby's analyses to be fallacious, they will deserve credit for their ingenuity.

Were we inclined to be hypercritical, we might object to the editor's allowing Dr. Bird's remark in p. 105, that lactic acid exists in the sweat, to remain uncorrected. Schottin, who worked under Lehmann's direct observation, ascertained that no lactic acid occurred in that excretion either in health or in disease. And why is Pelouze's fallacious test for lactic acid still given? Ten years ago, or more, Pelouze himself showed that it was not to be depended on. Again, in p. 112, we find Dr. Kemp's analysis of the organic material of human bile quoted and contrasted with analyses of hippuric acid and urinary colouring matter. Does not the discovery (and it is by no means a recent discovery) of sulphur in the bile altogether do away with the value of such comparisons?

In our perusal of the fourth chapter, On the Pathology of Uric Acid, and its Combinations, we made the singular discovery that the great drainage-system which is spreading itself so widely and rapidly over all our agricultural districts, is making marvellous advances in the human system. In p. 144 of the fourth edition (published in 1853), we read as follows:—

> "Surely such an amount of *drainage* as 73 feet in every square inch of skin, assuming this to be the average of the whole body, is something wonderful, and the thought naturally intrudes itself—what if this *drainage* were obstructed? The number of square inches of surface in a man of ordinary height and bulk, is about 2500; the number of pores, therefore, is 700,000, and the number of inches of perspiratory tube 1,750,000—that is, 145,833 feet, or 48,600 yards, or nearly 28 miles."

In p. 154 of the present edition (published in 1857), we find that the number of inches of perspiratory tube has been increased to 2,205,000 inches, or nearly 34 miles; so that if the above data are trustworthy, about six miles of tubing have been laid down in "a man of ordinary height and bulk" in the course of four years!

In the seventh chapter, On the Chemical Pathology of Cystine, Dr. Bird, after comparing the per-centage compositions of this substance and taurine, observes, that "it appears by no means unlikely that the excretion of cystine may be a means under certain circumstances of compensating for deficient action of the liver *quoad* the excretion of sulphur." The editor might have appropriately strengthened this view by a reference to the recent observations of Virchow and Cloëtta, from which it appears that cystine may sometimes be found in the liver, and that both taurine and cystine (sometimes one and sometimes the other) may be detected in the kidneys.

In minor points, as, for instance, in the correction of the press, we find a similar want of careful editorial superintendence. We have seldom seen proper names so cruelly maltreated. Thus we find Donne for Donné (p. 376); Erischen for Erichsen (p. 431); Grullot and Leblance for Guillot and Leblanc (p. 407); Kolliker for Kölliker (p. 373); Krahmer for Kramer (p. 451), or *vice-versâ*, both spellings being given on the same page; Lallelard for Lallemand (p. 378); Mülder for Mulder (pp. 344, 350, 357, &c.); Pellischer for Pillischer (p. 281); Städder for Städeler (p. 17); and Wohler for Wöhler (pp. 123, 244, &c.). The word hyoscyamus is frequently spelt hyosciamus; several chemical formulæ contain serious errors (see pp. 41, 201, and 332); we read of urates in one place, while in another the same salts are termed bi-urates; while in one part of the book phosphoric acid is symbolically designed as $P_2 O$, in another page we find the same acid written $Ph.\ O^5$; and finally, in p. 396, we find a reference to the 'Archiv fur Physiologysche Chimie,' the first word of this title being the only one that does not contain a gross blunder.

In conclusion, we would venture to recommend that if another edition should be called for, Dr. Birkett will best discharge his editorial duties by removing with no sparing hand those "passages and plates that have lost a portion of their value from the recent progress of scientific inquiry" to which he alludes in his preface; by condensing many portions of the volume which belong rather to physiology generally than to the special object to which the work is nominally devoted (for instance, the first three chapters might advantageously be condensed into one); by omitting a good many of the cases; and finally, by only introducing such additions as have a practical value in relation to the diagnosis and treatment of "Urinary Deposits."

REVIEW XIV.

Guy's Hospital Reports. Edited by SAMUEL WILKS, M.D., and ALFRED POLAND. Third Series. Vol. IV.—*London*, 1858. 8vo, pp. 371.

WE proceed, as usual, to give a brief analysis of this eminently useful serial. When, year after year, we behold the immense number of valuable facts which by these 'Reports' are added to the stores of medical science, we cannot avoid feeling regret that the example of Guy's is not followed by others of our large metropolitan and provincial hospitals. What a vast amount of clinical knowledge is annually lost for want of some such medium of publicity! While we deprecate the modern tendency to multiply the periodical sources of medical literature, we believe that the place of many of the existing medical journals might be well supplied by hospital reports. No better model could be selected than that of the Reports which the medical officers of Guy's Hospital have been issuing for the last twenty-two years. The hospital case-books of many of our physicians and surgeons, if carefully analysed after a series of years, might be found to contain facts which could have no small influence over the existing medical practice, and in many instances would effect a complete revolution. It is true that many of our hospitals have not the means and resources possessed by Guy's; but this difficulty might be got over by two or more joining together for the object in question. But whatever the plan adopted, we are convinced that a more extended application of the materials furnished by hospital practice would do far more to advance the science and practice of medicine, than any amount of speculative writing unsupported by a sufficient number of well-observed facts.

The present number of the 'Guy's Hospital Reports' contains fourteen original communications, which are illustrated by fourteen lithographic plates and eight woodcuts. The first two communications have reference to the same case.

I. *Case of Epithelial Cancer of the Œsophagus, in which Gastrotomy was performed.* By S. O. Habershon, M.D.

II. *Description of the Operation of Gastrotomy.* By J. Cooper Forster.—The subject of this case was a man, aged forty-seven, who was admitted into Guy's Hospital, October 8th, 1857, with considerable dyspnœa and feeble voice, but no marked physical signs of mischief in the chest. After he had been in hospital for some time, he began to suffer from severe pain in the throat on coughing, and subsequently on swallowing. Deglutition became more and more difficult and painful, and towards the end of February, 1858, the commencement of the œsophagus was found to be obstructed by a rounded tumour situated below the epiglottis. On March 2nd, the dyspnœa was so extreme that tracheotomy was performed, but without much benefit. In the meantime, the patient had become emaciated to an extreme degree, and suffered from most distressing thirst, and a fearful sense of starvation, while the rectum ejected nutritious enemata almost at once. It was obvious that the patient was dying from pure inanition, and there seemed no reason to doubt that life might be prolonged for a brief period if any means could be devised for introducing nutriment into the system. Three modes of relief suggested themselves: 1st, the forcible introduction of an œsophageal tube; 2nd, opening the œsophagus in the neck; and, 3rd, opening the stomach. The nature of the disease rendered the first of these modes a very hazardous one. A similar case is alluded to, in which an eminent surgeon passed a tube through the diseased mass, and injected half a pint of beef-tea into the pleural cavity. The situation of the disease would have rendered the opening of the œsophagus formidable, dangerous, and useless. Gastrotomy alone appeared to be the operation which could possibly relieve the patient, while the cases recorded and collected by Dr. Murchison showed that wounds of the stomach might become converted into permanent fistulæ, and life be prolonged for many years.*

After due deliberation, therefore, this operation was performed by Mr. Cooper Forster on March 26th. Chloroform was not administered, as the patient was remarkably cool and collected, and vomiting above all things was to be avoided. The external incision was made in the left *linea semilunaris*, and the cardiac portion of the stomach was that which was opened. None of the contents of the stomach escaped into the peritoneum, and the margins of the opening in this viscus, which measured about an inch, were carefully attached by sutures to the abdominal parietes. The patient's sufferings were evidently mitigated, and the horrors of a death from thirst and starvation averted; while there was reason to believe that, had the relief been afforded at an earlier period, it might have been more permanent. After the operation, nutriment and stimulants were freely introduced into the stomach; but on the night of the 27th faintness came on, and the patient gradually sank, and died at the end of rather more than forty-four hours. After death, the peritoneum was found healthy; no inflammation, effusion of lymph or serum, or diminution of the normal smoothness of this membrane could be detected; while the opposed serous surfaces round the margins of the opening were found slightly adhering. The growth in the œsophagus appeared to take its origin from the posterior surface of the cricoid cartilage, and the seat of greatest constriction was opposite the upper margin of the sternum. The œsophagus above this was much ulcerated, and there was a fistulous communication with the trachea. There was no cancerous disease in any other organ except the œsophagus. The two papers are accompanied by four plates, illustrating the operation and the post-mortem appearances.

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* See Memoir on Gastro-Cutaneous Fistulæ, in the Forty-First Volume of the Medico-Chirurgical Transactions.

III. *Pathological Observations.* By Samuel Wilks, M.D.—1. *On Cancer and New Growths.*—We here meet with a further development of Dr. Wilks's views of the pathology of morbid growths, to which we have already* had occasion to allude, and with which we in the main agree. Dr. Wilks observes:—

> "We will state at the outset our firm conviction that cancer, in the commonly received acceptance of the term, has no peculiarities which can always distinguish it from other morbid growths, or even from many healthy structures. Some experience in testing this debateable question has satisfactorily proved the truth of the assertion, having seen, for example, well-marked encephaloid disease, which displayed merely small nuclei by the microscope, offering nothing peculiar to the eye of those well versed in the use of the instrument; and, on the other hand, we have known healthy cells from parts of the alimentary canal, from the kidney, from the Pacchionian bodies, &c., considered as cancerous, when this idea of their nature was suggested to the observer." (p. 19.)

But even those growths which are "homologous" in their structure, not unfrequently put on many of the characters which for long were attributed to cancer only—such as rapidity of growth, tendency to spread, &c.; so that, if we were to arrange tumours according to the degree of their so-called "malignancy," those at the top of the scale may be called cancerous; but we can draw no boundary line between the last on the scale, which we would style cancer, and the first, which we would designate by some other name. The question, then, to be asked with reference to a new growth is not so much—is it malignant or not? as—what degree of malignancy has it? Dr. Wilks observes, that when from any cause a blastema is thrown out, the natural tendency is for it to be developed into tissues similar to those in its vicinity; but if there is any "vice in the constitution," the disposition in the new formation to ally itself to the healthy tissue will be lessened. Thus he explains the origin of cancer, and we are therefore somewhat at a loss to explain why, at another place, he speaks of the "cachectic condition spoken of in cancer," as only a result of the local disease, unless he considers a vice in the constitution, and a cachexia, as two very different conditions, the distinction between which we can scarcely appreciate. The author's observations are accompanied by the records of a number of cases which have come under his observation at Guy's Hospital.

In reference to *melanosis*, he holds the view that the colouring matter is merely adventitious, and may occur in any form of growth. Hence he accounts for the difference of opinions as to the malignancy of the disease. "A simple fibrous tumour, for example, near the eye, may contain pigment, and not return. A cancer, however, in the same spot, may also be black, and that shall return." (p. 33.) Several cases corroborative of this opinion are alluded to.

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* Brit. and For. Med. Chir. Rev., April, 1858.

2. *Acute and Chronic Disease.*—Dr. Wilks objects to the commonly received opinion, that chronic diseases are very frequently the sequelæ of acute; and endeavours to show that an opposite doctrine is much nearer the truth, or that disease is mostly chronic, and if not arrested will become acute. One remark under this head is at the present moment of peculiar interest:—

> "Although there are constantly cases of pneumonia treated in the hospital, post-mortem inspection has not revealed a single idiopathic case for more than a year past; and the same may be said of pleurisy, though to a less extent." (p. 52.)

3. *On the Relative Importance of Disease of the Aortic and Mitral Valves of the Heart.*—If we are to judge from the duration of illness or loss of health in the two cases, the author believes that the general opinion is correct as to the greater severity of the disease which has its origin in the mitral valve; but if we date from the probable time at which the respective maladies commenced, he thinks we may readily arrive at an opposite conclusion. The records of Guy's Hospital fully corroborate the view, that, as a general rule, the disease of the mitral orifice has its origin in rheumatic endocarditis; whereas disease of the aortic orifice is due to a strain on the vessel or valves, occurring for the most part in men who are accustomed to use strenuous exertions with their arms.

4. *Contre-Coup.*—The records of Guy's Hospital lend no support to the prevalent error, that when a blow is inflicted at one point of the cranium, a fracture and extravasation of the blood may take place in an opposite direction. The base may certainly be fractured from a fall on the vertex; but this is by direct continuance of the force, and not by contre-coup. The cerebral substance also on the other side opposite to the fracture may become torn, and extravasation ensue. Yet in such a case the injury to the brain is of greater importance than the effused blood, and could not be benefited by any surgical interference. During many years, the author has never seen a case on the post-mortem table, where trephining on the side opposite to that injured could possibly have given any relief.

5. *Chronic Rheumatic Arthritis.*—The characters of this affection are detailed with much accuracy, and Dr. Wilks agrees with Dr. Adams in considering it as distinct from both rheumatism and gout.

IV. *On the Operation of Opening the Urethra in the Perinœum.* By Thomas Bryant.—Mr. Bryant enters upon the discussion of this much-vexed question apart from all personal feeling. His remarks may be regarded as coming from one entirely unprejudiced, and merit the attentive perusal of the surgeon. He justly draws a distinction between the opening of the urethra without a staff, which he denominates "perineal section," and "the external division," or Syme's Operation. In the discussions which have taken place upon this subject, these two operations have too frequently been confounded.

The paper is accompanied by the histories of eighteen cases in which one or other of these operations was performed. We are glad to find Mr. Bryant speak so favourably of his experience of Mr. Syme's operation, and that he has failed to observe those frightful consequences which its opponents have attributed to it. His opinion regarding the operation may be summed up in his own words:

> "Cases of stricture do occur occasionally which are so exquisitely sensitive, and in which the passage of a catheter, however skilfully performed, is followed by such severe constitutional and local disturbance as to produce more harm than good; and others, which are relieved by means of a catheter, and are even fully dilated, but which have a tendency to contract again immediately upon the omission of the treatment. In such cases, the operation of 'external division' is most valuable." (p. 80.)

Such are the only cases, and only such, for which Mr. Syme has recommended the operation.

V. *On Ophthalmostasis, with an Account of an Improved Method of Extraction of the Cataract.* By John F. France.—Mr. France gives an account of the various means which have been recommended for steadying the globe in the operation for extraction of cataract, and he recommends the ordinary artery forceps applied immediately beneath the inferior-margin of the cornea, as infinitely preferable to them all. The histories are given of twenty cases in which the operation was performed in the manner recommended.

VI. *On the Existence of Copper in Organic Tissues.* By William Odling, M.B., F.C.S., and August Dupré, Ph. D.—The analyses and observations of the authors show that copper is a natural and constant constituent of living organisms, or, at any rate, that it has a very extensive distribution in the organic kingdom. Of twenty-five samples of bread, seven yielded distinct traces of copper; and of fifteen other specimens, of which larger quantities of materials were employed, in one only could no trace of the metal be detected. Of twenty samples of wheat-flour, traces of copper were discovered in every specimen, and it was likewise found in every one of twenty-nine specimens of grain. Copper also was found in several other vegetable products, and in many animal tissues. Dr. Odling's analyses render it highly probable that the poisonous effects which have frequently followed upon eating mussels and other shell-fish, have been due to their containing copper. The tissues of the higher animals, particularly those of the liver and kidneys, usually contained copper in notable quantity, whereas the blood in general contained but very minute traces of it. The authors are scarcely prepared to maintain that copper is a necessary and invariable constituent of living organisms, yet they agree with Dr. Christison in thinking that its possible presence must not be overlooked in medico-legal researches.

VII. *A Collection of several Cases of Contusions of the Abdomen, accompanied with Injury to the Stomach and Intestines.* By Alfred Poland.—This is a very elaborate paper, containing the histories of upwards of seventy cases in which accidents of divers sorts have ensued upon blows over the abdomen. These have been arranged under the following heads:

A. Contusion attended with injury to the stomach:
  I. Traumatic gastritis.
  II. Chronic inflammation, ending in abscess and external fistulous openings.
  III. Rupture of the coats of the stomach.
B. Contusion, attended with injury to the small and large intestines:
  I. Traumatic enteritis.
  II. Chronic inflammation and stricture.
  III. Contusion of the bowel, with subsequent sloughing and ulceration.
  IV. Ruptured coats of intestine.
    1. Ruptured duodenum.
    2. Ruptured jejunum.
    3. Ruptured ileum.
    4. Cases of ruptured bowel occurring in persons the subject of hernia, but independent of any hernial mischief.
    5. Contusions of small intestine while in a hernial sac.
    6. Rupture of the large intestines.

The illustrative cases have partly been under observation at Guy's Hospital, but for the most part have been collected from the published archives of Medicine and Surgery. Our space will not permit us to do more than call attention to these cases, all of which are of much rarity and interest. We would only mention the two cases which have been cited as examples of chronic inflammation followed by abscess and external fistulous openings. Both of these were probably examples of simple ulcer of the stomach, and it may be doubted how far the local injuries to which they were attributed had to do with their origin. The abscess in the young woman forming the subject of Etmüller's case, did not appear until ten years after she had received the blow in the epigastrium from the carriage pole. The other case, quoted from Richerand's 'Physiology,' was evidently that of the celebrated Magdelaine Goré, who was a patient in La Charité at the commencement of the present century. This woman, at the age of twenty, received a blow over the stomach from a fall on a door-step; but the abscess, which ended in fistula, did not make its appearance until eighteen years afterwards. This abscess opened externally on the twenty-first day from its first appearance, and the resulting fistula continued until death, eight and a half years later.*

Mr. Poland makes some valuable remarks upon the signs and symptoms which indicate that rupture of some portion of the bowel has taken place, and also upon the best treatment to be adopted in such cases. The prognosis in cases of ruptured bowel must always be very unfavourable; out of sixty-six collected by the author, sixty-four proved fatal; and in the remaining two, it was questionable if any rupture had taken place. The author, however, believes, that some cases do recover, and that many of the cases of injury of the abdomen followed by peritonitis, are really examples of ruptured bowel, of which no evidence could be satisfactorily offered.

VIII. *Cases of Paraplegia.* Second Series. By WILLIAM GULL, M.D.—An analysis of the first series of Dr. Gull's cases of paraplegia will be found in a previous number of this 'Review.'† The present essay contains an account of sixteen additional cases, which are illustrated by very beautiful lithographic drawings. Several of the cases are of exceeding interest. Case XVII. goes far to establish an important point, namely, that the spinal centres may be paralysed without *anatomical* change of their structure. We have ourselves observed one or two cases of complete hemiplegia in which even the microscope failed to reveal any lesion in the suspected locality. It is very desirable that we extend our observations as to other means of investigating the changes which take place in nervous tissue.

Case XVIII. presented a not uncommon history of chronic inflammatory degeneration of the columns of the cord almost latent up to a certain point, and then accompanied by a sudden aggravation of the symptoms.

Case XIX. was a remarkable instance of limitation of the disease to the posterior columns ("chronic inflammatory degeneration.") There was no paralysis, but a want of controlling power; sensation was but slightly affected. The symptoms confirmed Dr. Todd's theory, that the posterior columns of the cord are the channels through which the voluntary movements are co-ordinated.

Cases XXII., XXIII., and XXIV., show that the substance of the cord may be damaged by a violent exertion, without any affection of the bones, ligaments, or membranes of the spine.

Case XXIX. was remarkable for the obscurity of its clinical history. It was an example of chronic thickening of the spinal membranes implicating and destroying the posterior roots of the nerves of the brachial plexus. The disease was for some time so limited as to produce no other symptom than anæsthesia of the left arm. Duchenne's galvanism test proved entirely fallacious. All the conditions which he mentions as characteristic of hysterical paralysis—viz., normal electro-muscular contractility, great diminution of electro-sensibility, and no impairment of voluntary motion—were present, and yet the case was one of serious organic lesion.

IX. *A Case of Pharyngotomy for the Extraction of a Foreign Body; with*

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* For the original description of this case, see Journ. de Méd. et Chir. Par Roux et Corvisart, tome iii. p. 407.

† Brit. and For. Med.-Chir. Rev., July, 1857.

some Remarks. By Edward Cock.—The patient was a gentleman, whose age is not stated. The foreign body was a metallic tooth plate, which had been swallowed, and retained at the termination of the pharynx. The operation was performed on January 21st, 1856; the patient made an excellent recovery, and is still alive and well.

Mr. Cock has collected the histories of seven other cases in which the gullet had been opened for the extraction of a foreign body. All the cases recovered save two. In one of these death was attributable to pneumonia, which existed at the time of the operation; and in the other, the fatal result was brought about, not by the operation, but by the severe and somewhat extraordinary means which were previously used to dislodge the foreign body.

X. *Contributions to the Practical Surgery of New Growths or Tumours.* Series II. *Fibro-plastic Growths.* By John Birkett.—In this paper, Mr. Birkett records fourteen examples of that class of tumours to which Lebert first applied the appellation "Fibro-plastic." Some of these are remarkable for their returning repeatedly after removal, constituting the "Recurrent Fibrous Tumours" of some writers. Case X., for example, a fibro-plastic growth in the leg, was removed three times, and as often recurred. Ultimately, amputation through the femur was performed; but the growth re-appeared in the stump, and the patient died. The following are some of the conclusions which the author arrives at concerning "Fibro-plastic tumours:"

1. The elementary tissues comprising them differ from those entering into the composition of the tumours called carcinoma.
2. Their progress is slower.
3. They may recur at the primary site of the new growth, or in its immediate neighbourhood.
4. Unlike carcinoma, there does not appear to be a disposition to their production in any of the viscera of the chest or abdomen. (Dr. Wilks in his paper mentions a case of "Recurrent fibroid tumour of the lung.")
5. The lymphatic glands do not become secondarily involved, as in carcinoma.
6. Excision of a primary fibro-plastic growth may be undertaken with a better chance of the eradication of the disease, than in the case of carcinoma; yet amputation of a portion of a limb will not in every case prevent a reproduction of the disease, even although a joint intervene between the seat of the primary disease and the stump.

XI. *Contributions to Dental Pathology.* By S. James A. Salter, M.B., F.L.S., &c.—Mr. Salter, in the first place, makes some interesting remarks upon the shedding of the teeth and exfoliation of the alveolar processes consequent upon the eruptive fevers. He shows that such accidents are by no means unfrequent sequelæ of attacks of scarlatina, measles, and small-pox. He endeavours to account for the phenomena by the well-known fact, that in the eruptive fevers the poison of the disease spreads its chief force upon the tegumentary system. The bone-necrosis he looks upon as a secondary consequence of inflammation of the alveolar periosteum excited by the blighted teeth.

Some observations follow upon a peculiar affection, which the author describes under the name of "Warty Teeth," and also upon "Polypus of the Tooth-pulp."

XII. *On the alleged Sugar-forming Function of the Liver.* By F. W. Pavy, M.D., Lond.—As the author observes, the title of his paper appears somewhat discordant with the opinions expressed in his previous papers in the 'Guy's Reports.' In a former number of this Review* we called attention to Dr. Pavy's experiments, by which he endeavoured to show that the sugar formed in the liver is carried by the hepatic veins to the lungs and there destroyed. He never doubted at that time the truth of Bernard's doctrine, that sugar was really formed in the liver during life. The object of the present communication is to show, that the sugar which is found in the blood of the right side of a heart of a dead animal has not been generated in the liver, as hitherto supposed, but is nothing more than a *post-mortem* chemical transformation of a substance which is formed in the liver, and which happens to be with extreme facility, by a process allied to fermentation, convertible into sugar. The author's opinions are founded upon an extensive series of experiments, which have already been communicated to the Royal Society. On upwards of sixty different occasions on which he has examined the blood taken from the right side of the heart of a living animal, sugar has only been detected to the extent of the merest trace. Several other interesting observations corroborative of the author's views are also mentioned, for an account of which we must refer to the original paper. Dr. Pavy's facts will no doubt receive the attention which they deserve, and will call forth the investigations of other competent observers. If the facts are true, the conclusions carry along with them a high degree of probability.

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* April, 1856.

XIII. *The Influence of Diet on the Liver.* By F. W. Pavy, M.D., Lond.—The substance which is generated in the liver, and which, according to the author, exhibits such a remarkable facility for becoming converted into sugar, Dr. Pavy proposes to call *hepatine.* He describes what he has found to be the most economical and easy method of obtaining this substance from the liver in a tolerable state of purity. The quantity of hepatine which is formed in the liver would seem to vary very greatly with the nature of the diet upon which an animal is fed, being greatest when the diet has been vegetable, least, when the diet has been purely animal, and intermediate, when the diet has been mixed. Thus:

The average per-centage of hepatine yielded by the livers of eight dogs after an *animal* diet was	6·97
Ditto, in three cases after a *vegetable* diet	17·23
Ditto, in four cases after a *mixed* diet	14·5

The absolute size of the liver in dogs was also found to be materially increased after a vegetable diet.

XIV. *On Poisoning by Nicotina; with Remarks.* By Alfred S. Taylor, M.D., F.R.S.—The case which forms the subject of this communication is the second on record in which nicotina has been employed for the destruction of human life. The first occurred in Belgium in 1851. The Count and Countess Bocarmé were charged with the murder of the Countess's brother, a M. Fougnies, by administering to him nicotina, while dining with them in the Château of Bitremont. The deceased did not survive more than five minutes. The possession of the poison, as well as its detection in the body of the deceased, and the moral evidence, fixed the crime on the count, who was condemned and executed.

In Dr. Taylor's case, the poison was taken with suicidal intent, by a gentleman aged thirty-six, well acquainted with chemistry, who had for some months been in a state of great mental depression. Five months before his death, he had casually remarked in conversation that if at any time he took poison, he would select nicotina, because it was certain in its action, and would kill a person quickly. The quantity of the poison which he swallowed it was impossible to determine, and the time during which he survived after taking it was a matter of inference. It was assumed, however, from the circumstances, that he was insensible and powerless within a few seconds, and that he died in from three to five minutes. There were no convulsions. The poison appeared to have acted as a pure narcotic. After death, nicotina was detected in the stomach in considerable quantity. The chemical tests and manipulations employed are carefully detailed. It would appear that nicotina is very rapidly absorbed into the system, and causes a complete stagnation of the altered blood in the over-filled capillaries. The appearance of the various organs, as a result of capillary congestion, was such as Dr. Taylor had never before seen. They appeared as if they had been dyed of a deep purple-black colour, a condition which must have been the result of the action of the poison during only a few minutes. Dr. Taylor concludes that there is some foundation for the theoretical view of Bernard, that nicotina destroys life by arresting the circulation from the circumference to the centre. The paper terminates with some remarks on the properties of nicotina, and its effects upon the lower animals.

PART SECOND.

Bibliographical Record.

Art. I.—*A Dictionary of Practical Medicine, comprising General Pathology, the Nature and Treatment of Diseases, Morbid Structures, and the Disorders especially incidental to Climates, to the Sex, and to the Different Epochs of Life.* By James Copland, M.D., F.R.S., Fellow of the Royal College of Physicians; Honorary Member of the Royal Academy of Sciences of Sweden; of the American Philosophical Society; and of the Royal Academy of Medicine of Belgium; lately President of the Royal Medical and Chirurgical Society of London, &c. Parts XIX. and XX.—*London*, 1858.

Our readers, in fact the whole medical world, will, we are assured, with us congratulate Dr. Copland upon the completion of his great work, to which we are brought by the publication of the nineteenth and twentieth numbers. Whether we look to the magnitude of the undertaking, the profound research which it displays, or the vigour and breadth with which all the subjects are handled, it equally claims from us the homage due to the most admirable qualities that can adorn the scientific physician. It would be difficult indeed exactly to determine the influence which this vast work, long before its completion, has exercised upon the medical mind of this country. Doubtless much of what is now the common property of all educated practitioners might be traced to this fountain, and if Dr. Copland from time to time sees seed that he has sown, bringing fruit in various ways, it will be to him a gratifying proof of the hold which his labours have acquired over the medical public. It is our duty to thank him for having persevered to the end, and it is equally a duty and a pleasure to offer him the cordial good wishes that he may long be able to wear the laurels due to the citizen who has achieved what will assuredly be *ære perennius.*

Of the two numbers which bring the 'Dictionary of Practical Medicine' to a close, we can now only say that they prove the careful attention which the author continues to pay to the medical literature of the day; it may suffice to point to the complete manner in which he discusses the subjects of syphilisation, the most recent views regarding the parasites infesting the human body, and the researches into the histology and pathology of the supra-renal capsules.

A very complete Index is appended, which materially facilitates reference, and which the more deserves acknowledgment, because, from the contents of the work being alphabetically arranged, some authors might have thought themselves at liberty to save themselves the tedious labour of compiling an index as well as a "Classified Contents." The latter not only affords a very useful summary of the subjects elaborated in the body of the work, but serves also as a guide to the author's views on the classification of disease. Indeed, he has been true to himself, and we have little doubt that his cotemporaries and posterity will concede that his "attempt" "to arrest the attention, to engage it with an unflagging interest, to infix what is important or undisputed on the memory, and to carry on the mind, by an enlightened induction to a due recognition of pathological principles, and of therapeutical intentions and precepts," has been fully realised. Thirty years of his life have been devoted to the work by the author, and he has, we are told, laboured on it alone and unassisted. To him alone, therefore, belongs that full meed of praise which such devotion, such labour, such energy deserve.

Art. II.—*On Amputation by a Long and Short Rectangular Flap.* By Thomas F. Teale, F.L.S., F.R.C.S., Surgeon to the Leeds General Infirmary.—*London*, 1858. pp. 72.

From tables published at length by Mr. Teale, compiled from the reports for the last few years in the 'Medical Times' and 'Gazette,' it appears that out of 640 amputations of the thigh and leg for accident and disease, nearly one in every three proved fatal. In cases of amputation of the leg for accident, as nearly as possible one-half died. When we add to this statement the fact, that however much a compiler may be distinguished for carefulness and honesty of purpose, yet that the favourable cases find their way into his lists more readily than others, it must be allowed that a safer mode of performing amputations is a great *desideratum*. In the corresponding tables, published by Mr. Teale, where the amputations were performed according to his new method, the numbers are not sufficient to admit of comparison upon all points, but as far as they go they must arrest and command attention.

"The amputations of the thigh for disease present 3 deaths in 17 cases, or nearly in the proportion of 1 to 6. In the London hospitals the ordinary amputations of the thigh for disease show a mortality of 1 in 4½. In the Provincial hospitals the mortality is 1 in 4.

"The amputations of the leg for disease (by Mr. Teale's method) show a mortality of 1 in 27, which contrast most favourably with the ordinary modes of amputating. In the London hospitals these amputations are attended with a mortality of 1 in 3½, and in the Provincial hospitals of 1 in 4." (p. 21.)

The risk of a fatal termination is, however, by no means the only evil to which a patient who is unfortunate enough to lose a limb is subjected. It appears from the statements of those who have had most experience in the adaptation of artificial limbs, that the cicatrix has, as a rule, been adherent to the extremity of the bone, and that it has continued for years in an irritable condition. Thus, after amputation of the leg and thigh, the best mechanicians have been unwilling to allow any of the weight of the body to rest upon the extremity of the stump. Moreover, in cases where no weight has been allowed to rest upon the extremity of the divided bone, but has been distributed by means of a bucket over the surrounding parts, yet has there been "tenderness and pain occasioned in almost every instance where the integuments pertaining to the stump are forcibly drawn upward." The end of the stump is easily inflamed and abraded by friction." (p. 68.) In order to remedy these evils, Mr. Teale proposes his new plan of operating.

"The chief advantages of this mode of operating are:

"1st. The avoidance of tension.

"2ndly. The formation of a soft covering for the end of the bone, consisting of parts free from large nerves.

'3rdly. The non-disturbance of the plastic process, and consequent placing of the large veins of the limb, as well as the smaller veins of the bone, in a condition the least likely to take up purulent matter, and putrid blood or serosity.

'4thly. The favourable position of the incisions for allowing a free outlet for purulent and other discharges." (p. 10.)

The mode of performing the operation is by making two parallel longitudinal incisions, one on each side of the limb. These extend through the skin only, and are to be of the length of half the circumference of the limb. They are joined at right angles by two other transverse incisions which go down to the bone. The transverse incision for the upper or anterior flap is at the lower or distal extremity of the longitudinal incisions; the transverse incision for the lower or posterior flap is made so as to allow the flap to be one-fourth only of the length of the upper flap. The vessels and nerves are always to be left in the short flap. The flaps are dissected from below upward, and the bone is sawn through on a line corresponding with the upper extremities of the longitudinal incisions. The long anterior flap is then folded upon itself over the extremity of the bone. The reflected portion is connected with the other parts of the long flap, and also with the extremity of the short flap, by means of sutures, and the stump laid upon a pillow without dressing. The chief peculiarity of the stumps formed by this mode of operating "consists in their having a soft mass of tissues, devoid of large nerves, moveable over the sawn end of the bone, which enables them to bear pressure on their extremity." (p. 27.)

We heartily recommend Mr. Teale's mode of operating to the serious consideration of every hospital surgeon.

Art. III.—*A Manual of Photographic Manipulation, Treating of the Practice of the Art and its various Applications to Nature.* By Lake Price.—*London*, 1858. pp. 256.

This is one of the most compact and useful handbooks which have yet been published, with a view to giving students of the photographic art instruction on the subject. Mr. Price is well known as a clever manipulator, and has probably acquired his dexterity at the expense of much time and in spite of frequent disappointments. For these all beginners must be prepared, but with perseverance they may attain to success, and they can have no better guide than Mr. Price.

Art. IV.—*Demonstration of Diseases in the Chest, and their Physical Diagnosis.* By Horace Dobell, M.D., Licentiate of the Royal College of Physicians, &c.—*London*, 1858. pp. 115.

All who have earnestly sought to master the physical diagnosis of diseases of the thoracic viscera, have probably experienced the difficulties which have led Dr. Dobell to present us with the volume before us, which is intended to enable the student to interpret correctly the signs offered by auscultation and percussion. Although no explanations or drawings can serve as a substitute for personal study at the bedside, the labours of the student may be much abridged by a clear enunciation of the acoustic principles involved, and by an exposition of the manner in which structural lesions induce deviations from the phenomena belonging to the normal state. Dr. Dobell professes to meet these two requirements, and we think that both his intentions and his execution deserve considerable praise. The first three chapters of the work are devoted to the consideration of the laws of acoustics so far as they are applicable to the physical examination of the chest; and it is important that the student should explain the phenomena he meets with by reference to these laws, rather than by regarding the former as indications pathognomonic of certain diseases. Having acquired a knowledge of the properties and conditions of matter necessary to the production of acoustic phenomena, he will be in a position to understand the relation that may be established between certain lesions and those purely physical effects. To employ the author's words—

"Three conditions are essential to a knowledge of auscultation: 1. To understand the acoustic conditions necessary to the existence of different sounds. 2. To know these sounds when heard, and to connect them with their essential acoustic conditions so intimately that they shall stand to the mind as symbols of such conditions. 3. To associate the conditions thus symbolized with the processes of health and disease necessary to their existence in the human organism."

In order to promote the fulfilment of the third of these conditions, Dr. Dobell has adopted a plan that is novel in a work of this kind, of representing in a series of well executed coloured plates the actual lesions which are met with in the dead body, and which give rise to various deviations from the auscultatory phenomena recognisable in health. And although we quite agree with the author's remark, that a sound must be heard to be appreciated or remembered, we do not hesitate to say that the student who, after the examination of a patient, refers to this book, will have a much greater facility in understanding the *rationale* of the phenomena and of interpreting them correctly, than one who is satisfied with comparing what he hears to the descriptions of sounds given in handbooks, or to some typical sound pointed out to him by his teacher. The training of the understanding is as important in this department of medical diagnosis as in any other; and however valuable a powerful memory, it cannot be made in any way a substitute for the former.

From the title of Dr. Dobell's book, the reader might be led to expect a treatise upon all the diseases occurring in the chest. Only the more prominent forms of pulmonary disease are alluded to; diseases of the heart are not even touched upon; but what the author gives is well done and in the right direction.

ART. V.—*Fragmentary Remains, Literary and Scientific, of Sir Humphry Davy, Bart., late President of the Royal Society, &c.; with a Sketch of his Life, and Selections from his Correspondence.*—Edited by his Brother, JOHN DAVY, M.D., F.R.S.—*London*, 1858. pp. 330.

IT is well now and then to be reminded of the debt we owe to the great men of the past by such memorials as the one which Dr. Davy presents to us: to learn the marks by which the advancing waves of civilization may be traced in the history of human development, is an important duty which may not be neglected by those who care for the progress of mankind. Sir Humphry Davy was one of the master minds of the present century, whose name belongs not to the annals of chemistry alone, but deserves to be inscribed among the benefactors of humanity of all time. The present volume is a monument of the fraternal affection which bound Sir Humphry to his younger brother John, a physician whose independent researches have given him a high rank among the physiologists of the day. The editor brings before his readers a large number of letters, which possess almost throughout a double interest, attaching to the writer as well as to the receiver. Thus few will take up the book without being riveted by the romantic sensibilities displayed in Coleridge's numerous epistles; while the wide scope of intellect, the depth and warmth of feeling, the poetic glow, which manifest themselves in all that emanates from Sir Humphry himself, enhance our admiration of the man which his achievements in science have already secured for him. The following extract from a letter written by Sir Humphry to Coleridge, whose admiration of him seems to have savoured of idolatry, will at once show the relation in which they stood to one another, and the estimate Davy had formed of his friend. The epistle was written before Coleridge's departure for the Mediterranean in quest of health:

> "Years have passed since we first met; and your presence and recollections in regard to you, have afforded me continued sources of enjoyment. Some of the better feelings of my nature have been elevated by your converse, and thoughts which you have nursed have been to me an eternal source of consolation.
>
> "In whatever part of the world you are, you will often live with me, not as a fleeting idea, but as a recollection possessed of creative energy, as an imagination winged with fire, inspiring and rejoicing."

This is the very poetry of friendship; and what encouragement and cheering comfort is contained in the following words, written on the same occasion:

> "May you soon recover perfect health—the health of strength and happiness! May you soon return to us, confirmed in all the powers essential to the exertion of genius. You were born for your country, and your native land must be the scene of your activity. I shall expect the time when your spirit, bursting through the clouds of ill health, will appear to all men, not an uncertain and brilliant flame, but as a fair and permanent light, fixed, though constantly in motion,—as a sun which gives its fire, not only to its attendant planets, but which sends beams from all its parts to all worlds."

Those who are familiar with the former memoirs of Sir Humphry, will remember the various pieces of poetry by his pen, indicating no mean talent in their author; we cannot evince our appreciation of Sir Humphry's poetic vein better, or place before our readers a more convincing proof of the justice of our praise of the man, than by transferring to these, it may be too unpoetic pages, the following lines; we premise merely that they were written after watching the eaglets at Loch Maree taking their first flight under the guidance of the parent birds:

"The mighty birds still upward rose
In slow but constant and most steady flight,
The young ones following; and they would pause
As if to teach them how to bear the light,
And keep the solar glory full in sight.
So they went on till, from excess of pain,
I could no longer bear the scorching rays;
And when I looked again they were not seen,
Lost in the brightness of the solar blaze.
Their memory left a type and a desire;
So would I wish towards the light to rise,
Instructing younger spirits to aspire
Where I could never reach amidst the skies,
And joy below to see them lifted higher
Seeking the light of purest glory's prize:
So would I look on splendour's brightest day
With an undazzled eye, and steadily
Soar upward full in the immortal ray,
Through the blue depths of the unbounded sky,
Portraying wisdom's matchless purity:
Before me still a lingering ray appears,
But broken and prismatic, seen through tears,
The light of joy and immortality."

The Fragmentary Remains carry their recommendation with them; willingly would we have placed more numerous extracts before our readers, but we must confine ourselves to thanking the learned editor for having undertaken the labour, doubtless a labour of love, of weaving them into the connected tissue of the present work.

ART. VI.—*On Dropsy connected with Disease of the Kidneys (Morbus Brightii), and on some other Diseases of those Organs, associated with Albuminous and Purulent Urine.* Illustrated by numerous Drawings from the Microscope. By W. R. BASHAM, M.D., Fellow of the Royal College of Physicians, and Physician to the Westminster Hospital.—*London*, 1858. pp. 241.

THE object of this work is to call the attention of the practitioner to the value and importance of habitual examinations of the urine by the microscope. Every medical man who is in the habit of employing this instrument for clinical purposes will agree with Dr. Basham's observations on this point; the microscope is as essential to a correct diagnosis of renal disease as the stethoscope to that of thoracic disease; nor is it unimportant to study microscopically the characters of the urine in other diseases than those manifestly residing in the kidneys, because by such means we obtain further confirmation of our diagnosis, or are enabled to determine changes in the system manifested by the secretion of the most important depurating organs of the body. In forcibly dwelling upon the uses of the microscope, Dr. Basham does not overlook the necessity of chemical analysis; but this is not considered in detail, and is not within the author's scope, which is summed up by himself in these words: he wishes, in the work before us,

> "To ascertain if the products thrown off from the tubuli uriniferi during the progress of disease possess any specific characters from which correct deductions might be drawn as to the nature, degree, duration, or stage of the renal disorder."

We coincide with the author as to the importance of frequently repeated examination of urinary deposits; changes take place from day to day which cannot otherwise be appreciated, and the system which he adopts, of making drawings of the objects seen, is the surest one of preserving a trustworthy record, provided the drawings are correct. Those who are familiar with the microscopic appearances spoken of by Dr. Basham, will recognize the objects represented in his very numerous illustrations; we fear that the very globular appearance which the cells receive from the peculiarity of the shading will tend to mislead the junior practitioner, as he would fail to find the same features under the microscope, which exhibits to us the epithelial corpuscle as essentially a flat surface. Nor can we think that the great multiplication of illustrations of the same things with which the author furnishes his book, is likely to facilitate the student's comprehension of these matters; he is more likely to be bewildered than enlightened when he seeks to make out the essential differences between many of the drawings.

Dr. Basham's industry in the microscopic examination of the urine of his patients deserves great praise, and the number of important and interesting cases which he brings forward cannot fail to meet the attention of those who take up his book with the view to obtaining information on one of the most important classes of disease in the whole range of pathology.

The work is divided into eighteen chapters, in which, after a minute account of the microscopic objects to be found in albuminous urine, the various diseases associated with this symptom are successively discussed. The author's views as to the uniformly inflammatory origin of the forms of renal disease which are grouped together under the name of Bright's Disease would be liable to objections; but his teaching as to the manner in which these affections should be treated seems to us to be beyond cavil, and to show that the author is a sound and far-seeing practitioner.

Art. VII.—*The Veterinarian's Vade Mecum.* By John Gamgee, M.R.C.V.S., Lecturer on Veterinary Medicine and Surgery in the Edinburgh New Veterinary College, Member of the Pathological Society of London, &c., with numerous Illustrations.—*Edinburgh*, 1858. pp. 337.

This useful manual opens with an introduction, in which the forms of medicines and the various methods of their administration to domestic animals are discussed, intelligence of the latter point being materially facilitated by some very speaking illustrations. A Pharmacopœia Veterinaria follows, in which the remedies are arranged according to their therapeutic action; the different preparations in common use, the most suitable form of exhibition, and the doses, are given, and a series of prescriptions, partly taken from other authors, partly derived from Mr. Gamgee's personal experience, add to the practical character of this part of the work. The next section is devoted to the consideration of the various organic and inorganic poisons by which the life and the health of domestic animals are threatened; the substances are described, the symptoms which they produce are detailed, and the most approved treatment appended in each case.

The book concludes with 'Memoranda Therapeutica;' an alphabetical list of the various affections to which animals are liable, with the treatment appropriate to each. The whole appears to us to be very judiciously and conveniently arranged, and to present the professional and the amateur veterinarian with a large amount of useful and easily available knowledge.

Art. VIII.—*The Microscope in its Application to Practical Medicine.* By Lionel Beale, M.B., F.R.S., Physician to King's College Hospital; Professor of Physiology and General and Morbid Anatomy in King's College, London; Honorary Fellow of King's College. Second Edition, with 270 Woodcuts and a Coloured Plate.—*London*, 1858. pp. 390.

The opinion we expressed of the first edition of this excellent work has been ratified by the favourable reception which it has met with from the public, and we have no doubt that the same testimony will be as readily and as palpably accorded to the present edition, which contains a large amount of new matter. Dr. Beale's endeavours to meet the advances that have taken place in clinical microscopy during the last few years deserve especial recognition, for they enhance the value of the work by bringing it down to the most recent times, and enable us again to recommend it most strongly.

Art. IX.—*Syllabus of the Course of Lectures on Medical Logic, delivered in Marischal College and University, Aberdeen.* By Francis Ogston, M.D., Professor of Medical Logic and Medical Jurisprudence.—*Edinburgh*, 1858. pp. 44.

In the "Summary of New Publications" in the April number of last year, we adverted to a syllabus of lectures on medical logic, purporting to have been delivered in Aberdeen, to which, however, the lecturer's name was not appended. Another copy has lately come to hand, with the title as given above, and we have no doubt that the addition of Dr. Ogston's name will in itself be regarded by our readers as a guarantee that the subject is well handled. Of the syllabus, we would only say, after a careful perusal, that it indicates a masterly grasp, and that it awakens in us a most lively wish to receive from the learned author the filled up outline, in the shape of the lectures themselves. We want a work of the kind to tell us the present position of medicine among the sciences; we require a standpoint from which we may justly estimate the relation of the synthetical and the analytical methods, and their bearing upon our profession. Will Dr. Ogston afford us the opportunity of presenting our readers with a fuller account of the views which he appears to have elaborated with much care and acumen?

Art. X.—*An Introduction to Clinical Surgery.* By Furneaux Jordan, Demonstrator of Anatomy at the Queen's College, and Pathologist to the Queen's Hospital, Birmingham.—*London*, 1858. pp. 155.

This is simply intended as a handy-book for the student, to aid him in investigating and reporting surgical cases. The first twelve pages are occupied with a critical inquiry into the present state of surgical investigation. Here the author deplores the incomplete and uncertain state of the science, and suggests various remedies. He considers that too exclusive attention is paid to the local lesion, and not enough to the organic and functional condition of the general system. He inculcates the importance of statistics, more particularly in reference to cancer, and to such operations as resection of the knee-joint, and amputation of the thigh. Thus, as concerns resection of the knee-joint, he says, (p. 9.), "from its recency as well as several other causes, its statistics are unsatisfactory, the principal cause of those alluded to being the absence of sufficiently numerous, sufficiently comprehensive, and sufficiently honest reports." We object to the author's style in several places, and recommend him in future to adopt plainer modes of expression. Thus, at p. 3, we read of "chorographic mischief;" at page 8, of gliding "into higher regions of decision and catholicity;" and at p. 9 the following sentence occurs, "of inane speciosities, the surgical literature of cancer is already too uncomfortably redundant."

The second chapter contains observations on the investigation and registration of surgical cases, and these are amplified in the two subsequent chapters. In Chapter V. the author refers briefly to the instruments and agents used for obtaining surgical information, such as the microscope, stethoscope, speculum, bougies, probes, &c. The remaining one hundred pages of the book are occupied with the details of the symptoms and phenomena of surgical diseases and injuries, derived from modern works on the subject, and carefully arranged. We notice one or two typographical errors which ought to have been corrected, as at p. 53 we read ancylobletharon and symbletharon, for anchyloblepharon and symblepharon. The medical student must be guarded against every chance of misspelling.

Art. XI.—1. *Handbook of Chemistry, Theoretical, Practical, and Technical.* By F. A. Abel, Director of the Chemical Establishment of the War Department, and C. L. Bloxam, Professor of Practical Chemistry in King's College, London; Lecturer on Chemistry to the Royal Military Academy, Woolwich. Second Edition.—*London*, 1858. pp. 785.

2. *A Manual of Elementary Chemistry, Theoretical and Practical.* By George Fownes, F.R.S., late Professor of Practical Chemistry in University College, London. Seventh Edition.—*London*, 1858. pp. 726.

The authors of the 'Handbook' inform us, that without material alteration in the general scheme of the work, they have contributed the results of four years' additional experience in the different departments of the science. The introduction is occupied with the definitions of chemical terms, chemical combinations, decomposition, nomenclature, notation, and the phenomena relating to the physical condition of bodies. All these subjects are clearly discussed. In the next division of the work, extending over some eighty pages, chemical manipulation is amply-treated, the instructions supplied under this head being plain and explicit. The remainder of the volume is devoted to inorganic chemistry, and to analysis, both qualitative and quantitative. Non-metallic bodies come first in order. Under the head of oxygen, we find an excellent account of that remarkable body first discovered by Schönbein, called ozone, and which has been supposed to have some connexion with unhealthy states of the atmosphere. The authors describe its preparation as follows:—

> "Upon passing dry atmospheric air or oxygen over a pair of platinum wires between which a succession of electric sparks is allowed to pass, a small quantity of the oxygen will become converted into ozone, which may be recognised by its peculiar odour. Ozone is best obtained by placing a piece of recently-scraped phosphorus, about half an inch in length, into a clean bottle (of about two quarts' capacity), in the bottom of which is as much water as will half cover the phosphorus; the mouth should then be closed slightly (to prevent any mischief ensuing if inflammation of the phosphorus should take place), and the bottle set aside. Ozone is almost instantly produced, its formation being indicated by the ascent of a column of vapour from the piece of phosphorus, and the luminosity of the latter in the dark. Ozone may be detected in the bottle within a minute after the introduction of the phosphorus; if allowed to stand for six or eight hours, the air in the bottle will be abundantly charged with it. The phosphorus should then be removed, and the air freed from phosphorus acid by agitating some water in the bottle. If the phosphorus be allowed to remain for a longer period in contact with the ozone, the latter combines with it, being after a time completely abstracted from the atmosphere in the bottle." (p. 109.)

Air, they say, powerfully charged with ozone, can be inspired with difficulty; it acts powerfully on the mucous membranes, producing very disagreeable sensations; small animals immersed in it soon cease to exist. The most delicate test for its presence is prepared in the following manner:—

> "One part of pure iodide of potassium and ten parts of starch are boiled together for a few moments, with two hundred parts of water, and white filtering paper is saturated with the

liquid thus obtained. Such paper is immediately turned blue when introduced moist into ozonized air. If introduced dry, it will remain colourless, but becomes blue immediately upon being moistened.

"Paper prepared with a solution of sulphate of manganese is also a good test for ozone, becoming rapidly brown from formation of binoxide when introduced into ozonized air." (p. 110.)

The authors have been at pains to consult the highly interesting and important researches of Deville and Wöhler, upon the preparation and properties of boron, silicon, and aluminum, in the revision of the sections relating to those elements. In treating of the metals, they have appended much useful information respecting metallurgy. The instructions given under the head of analysis are well arranged; and in quantitative analysis some space has been devoted to the volumetric system of examination. In the remarks on coal we expected to have found some notice of Mr. Lewis Thompson's ingenious apparatus for ascertaining its heating power.

We have no hesitation in recommending this work as a useful practical compendium of chemistry.

The manual of Professor Fownes has been before the public for eleven years, and has had an extensive circulation as a text-book for students. The later editions have been brought under the supervision of Dr. Bence Jones and Dr. Hoffmann. In reference to the present edition, we learn from the advertisement that the progress of organic chemistry has rendered many changes requisite; accordingly, throughout all this department, improvements have been made, and many subjects re-arranged. The most important addition will be found in the theory of the polyacid alcohols.*

In our opinion the work will still be found a useful text-book.

We may observe, that both these books are illustrated with numerous appropriate well-executed engravings on wood.

ART. XII.—*Lehrbuch der Physiologie.* Von Dr. SCHIFF, Professor in Berr. Heft 1 & 2.—*Lahr*, 1858.

Compendium of Physiology. By Dr. SCHIFF.

DR. SCHIFF's 'Compendium of Physiology' forms part of the extensive series of treatises on medical sciences, edited by Dr. Schauenburg. According to this plan, which seems to become more and more the fashion amongst continental authors, Dr. Schiff's work does not appear as a whole, but in single numbers, two of which only have reached us hitherto. We cannot refrain on this occasion from expressing our disapproval of this system of publishing. An inducement is held out to purchase works which our sad experience tells us are frequently not completed, or if finished, the concluding portions appear after so long a lapse of time that the beginning and end of the work do not match. *Desinit in piscem mulier formosa superne.* These remarks will at once serve as an apology to our readers for our not always bestowing that attention upon the initiatory numbers of new productions sent to us which they might otherwise appear to deserve. Having premised this much, we have much pleasure in stating that the commencement of Dr. Schiff's Compendium promises well; the two first numbers are in a high degree instructive, and tend to increase the author's reputation as an original thinker and observer.

After a short introduction, in which he promises to abstain from hypothetical abstractions, and to base his researches on observation and experiment, following in the steps of Valentin and the Webers, the author discusses the various phenomena of motion. In order to give an idea of the manner in which he treats his subjects, we will mention the heads under which he considers the phenomena of "ciliary movement." He commences by naming the situations in which it is met with; he then describes the different forms of ciliary motion (the *modus uncinatus* of Purkinje and Valentin, and *modus undulatus* of Valentin); then the direction of the movement, viz., from the orifice to the interior; then the mechanical effect produced by it, viz., the movement of particles in a direction opposite to that of the movement of the cilia; then the use, viz., the propulsion of the excretion of the mucous membrane. The author dwells on the independence of ciliary motion from nervous action, and on the manner in which it is influenced by chemical agents. The chapter on muscular motion is more elaborate than the one just adverted to. Dr. Schiff maintains the existence of muscular irritability independent of the nervous system; and the chapter comprising the physiology of the nervous system, which is the author's favourite subject, also presents features of much interest. If the other sections of the work are treated in the same manner as those now before us, Dr. Schiff's Compendium will prove a useful addition to our stock of works on Physiology.

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* At p. 98, under the head of Electricity, a statement occurs which is erroneous, and which has been overlooked in the previous editions as well, and to which we draw attention, because it is particularly puzzling to the student: "When two solid conducting bodies are plunged into a liquid which acts upon them unequally, the electric equilibrium is also disturbed, the one acquiring the positive condition and the other the negative. Thus pieces of zinc and platinum put into dilute sulphuric acid, constitute an arrangement capable of generating electrical force: the zinc, being the metal attacked, becomes negative, and the platinum remaining unaltered, assumes the positive condition." Now, the metal most attacked is positive with respect to the other; in other words, the zinc is positive and the platinum negative.

ART. XIII.—*On some of the more Obscure forms of Nervous affections, their Pathology and Treatment; with an Introduction on the Physiology of Digestion and Assimilation, and the Generation and Distribution of Nerve Force: based upon Original Microscopical Observations.* By HENRY WILLIAM LOBB, L.S.A., M.R.C.S.E. Illustrated with Original Engravings, drawn on Wood by the Author.—*London*, 1858. pp. 312.

THE first hundred and twenty-two pages of Mr. Lobb's work are devoted to the description of his microscopical examinations of the blood, with reference to the waste and repair of the tissues, to an investigation of the physiology of digestion, of se- and excretion, of the functions of the nervous system. We find here the proof that the author has read much of the classical medical literature of the day; that he is fond of the microscope, and that he knows how to apply this valuable instrument both to physiological and clinical research. In this part of the work, however, as well as in the second division, which is devoted to the more immediate consideration of obscure affections of the nervous system, we meet with an amount of dogmatism which is particularly dangerous in the field of inquiry selected by the author. The affections themselves upon which he brings his microscopical examinations and his hypotheses to bear, are, in the first instance, various forms of dyspepsia, hypochondriasis, albuminuria, diabetes, spermatorrhœa, and the like, which are classed together as certain affections of the nervous system, the "symptoms of which are obscure, and which, if not alleviated, would develop themselves into organic degeneration, fatal diseases, or insanity." Stammering, hysteria, epilepsy, chorea, also receive their share of the author's attention. Of the whole, we would say that we regard this production as an indication that Mr. Lobb is capable of accomplishing something that may advance science, and establish his own reputation as a medical author. In the work before us he has grasped at too much. Let him confine his inquiries to one or a few of the many points touched upon in his work, and, with the talents which he evidently possesses, we believe that perseverance in the path of strict inductive science, and the avoidance of vague theorizing, will lead him to more definite and tangible results than he has yet attained, or than our readers would, on perusing his volume, be ready to accord to his researches.

ART. XIV.—*Transactions of the Pathological Society of London.* Vol. IX. Including the Report of the Proceedings of the Session 1857–8.—*London*, 1858. pp. 483.

THE present volume of 'Transactions of the Pathological Society,' while it presents us with a larger amount of matter than any of its predecessors, does not yield to any of them in the value of the information which it imparts. The arrangement continues the same, and the Society still pursues the excellent system of submitting specimens of special interest to the scrutiny of members, who are required to report thereon. We have so lately devoted much labour and considerable space to the Reports of the Pathological Society, that we are now unable to do more than to advert in terms of praise to the present volume, and to state that the profession manifest no lack of zeal in the prosecution of Pathological Science, if we may take the doings of this Society as a test.

ART. XV.—*Summary of New Publications.*

AMONG the works which we reserve for a fuller consideration, we would first mention the 'Horæ Subsecivæ' of Dr. Brown, the librarian of the College of Physicians of Edinburgh; it is a volume that we have already perused with much gratification, and which we recommend without hesitation. Dr. M'Cormac, of Belfast, reiterates his views regarding Consumption in a letter to the Imperial Academy of Medicine; views with which we have on a former occasion expressed our general concurrence. Hysterical Affections find an exponent in Mr. Tate, whose main treatment of these disorders consists in rubbing tartar emetic ointment over the spine. The Urinary Organs, and the Derangements of the Urine, are respectively treated by Dr. Morland, of the United States, and by Dr. Hassall, of London; to both we shall probably shortly revert. From North America, we have also received an essay by Dr. Dunglison, on the 'Deaf and Dumb,' and 'Illustrations of Typhus Fever in Great Britain,' by Dr. Upham. Both these treatises contain proof of independent research, and deserve attention. Dr. Graily Hewitt presents us with a paper on the 'Nature and Causes of Vesicular Emphysema in Early Childhood.' The description of a new chalybeate comes to us from Wales, where, it appears from Dr. A. Wynn Williams' account, that King Arthur's Well, near Carnarvon, is rivalling the German Spas.

In Surgery, we would first mention the continuation of Mr. Maclise's important work on 'Dislocations.' A much enlarged second edition has appeared of Mr. Henry Thompson's 'Pathology and Treatment of Stricture of the Urethra,' and a second edition has also been published of Mr. Skey's 'Principles and Practice of Operative Surgery.' The 'Radical Cure of Inguinal Hernia, by the Modern Operations of Wutzer' (not Wützer) and Wood, is strongly urged by Mr. Holthouse, in a well written pamphlet, in which the whole subject receives full attention, and which well merits perusal. The 'Lettsomian Lectures on Syphilis,' which were delivered before the Medical Society of London during the year that has just elapsed, by Mr. De Méric, and which at the time were published in the 'Lancet,' have been reprinted separately. We reserve their consideration until the appearance of Mr. De Méric's larger work on Venereal Diseases, which he tells us he is now completing. Three numbers of a German Odontological Journal, 'Der Zahnarzt,' may be mentioned; together with a continuation of Dr. Schauenburg's Series of Medical Manuals, in the shape of a work by himself on 'Eye Surgery.' Ophthalmic Surgery also receives a large contribution from Mr. Nunneley. It is also our duty to advert to the continuance of the 'Ophthalmic Hospital Reports,' of which the fourth number is before us, and which does not yield in interest to its predecessors. Doctors Sinclair and Johnston present us with a work entitled 'Practical Midwifery, comprising an Account of 13,748 Deliveries which occurred in the Dublin Lying-in Hospital, during a period of Seven Years, commencing November, 1847.'

Passing from the Medico-Chirurgical Departments to Physiology, we introduce to our readers the Outlines of that science by Dr. John Hughes Bennett, of Edinburgh, embracing the 'Functions of the Ultimate Tissues and Organs of the Body in Health and Disease.' Mr. Rainey has published a little illustrated work on the 'Formation of the Shells and Bone of Animals,' the greater part of which has appeared in this Review, and in the 'Microscopical Journal.' We may refer our readers to the October number for 1857, for the leading features of Mr. Rainey's observations and arguments. From

France we have received Dr. Lucien Corvisart's essay on the 'Digestion of Nitrogenized Food by the Pancreatic Fluid, based upon original Experiments.'

In Chemical Science, a new well-got up manual has reached us, the authorship of which is shared by Messrs. Northcote and Church, and which aims to be a complete and systematic 'Guide to Qualitative Analysis.' A volume by Dr. Watson, on the 'Food Grains of India,' which promises to be of great interest and value, is heralded by the appearance of an introductory Chapter on the 'Development of the Resources of India,' which we hope soon to see followed by the body of the work.

A Report on the Sanitary Condition of the Army, by a non-Commissioner, examines into the Report of the late Royal Commission on the same subject, and professes to point out various errors into which the Commissioners have fallen. We have to advert to a laborious 'Report on the Health and Mortality of the Navy, in the year 1856,' by Dr. Alexander Bryson. It is drawn up from the Returns lodged with the Director-General of the Medical Department of the Navy, and forms a Blue-book, printed by order of the House of Commons. Finally, we wish to draw special attention to the appearance of the sixth number of Dr. Mayne's valuable 'Expository Dictionary of Terms belonging to Medicine and General Science,' which brings us to the beginning of letter R.

PART THIRD.

Original Communications.

ART. I.

The Laws of Organic Form. By HERBERT SPENCER.

THE last number of the 'Medico-Chirurgical Review' adds another to the frequent illustrations of community of thought independently arising in different observers. Those who read both, must have perceived between Mr. Hinton's interesting paper on 'Physical Morphology,' and the last two pages of the critique on Owen's "Homologies of the Vertebrate Skeleton," a manifest kinship; not only in the rejection of the hypothesis of ideal types, but in the ascription of organic forms to the action of incident forces. This kinship would have been yet clearer had not the limits confined me to the most general statement. The theory of vertebral modification briefly indicated in the above mentioned critique, forms part of a much wider theory of animal and vegetable structure which I have for some years been developing—partly stated in already published writings, partly only remotely implied in them. In two essays on "Transcendental Physiology," and "Progress: its Law and Cause,"* I have aimed to show that the changes undergone in the evolution of a homogeneous germ into a heterogeneous organism, are interpretable as consequences of two universal dynamic laws:—1. That every homogeneous aggregation is in unstable equilibrium, because its different parts are differently exposed to incident forces; and 2. That every force produces more than one change. In these papers I have dealt with the phenomena under their most abstract form; and have not therefore referred to any of the more special laws of force that are involved. Nor indeed, though aware that for the detailed explanation of the facts the more special laws of force have to be consulted, and among them the law of the line of least resistance, have I been conscious that the morphological phenomena which Mr. Hinton cites were capable of the interpretation he gives them. But while the ideas he has enunciated are quite new to me, I have long been familiar with the application of the law of the line of least resistance to a different class of organic phenomena—those of nervous action. At page 544 of "The Principles of Psychology," I have introduced a note respecting the probable origin of nervous connexions, as resulting from the repetition of the actions to be co-ordinated; closing it with the remark, that "on a future occasion I hope to say something in justification of this hypothesis." While, for the purpose of reference, I have there intentionally embodied the idea, that between any two parts of an organism habitually excited in succession, there must habitually take place a restoration of equilibrium by the passage of some force (nervo-electric, or whatever else we may name it), I have intentionally avoided directly using the phrase "line of least resistance" to describe the route through which the equilibration must take place; not wishing to give so complete a key to the theory hereafter to be set forth. I must now, however, explain that the unwritten fifth division of the 'Principles of Psychology,' referred to in the preface as being withheld for the present, lest "certain of the suggestions contained in it might prejudice some against the doctrines developed in the others," has for its object to work out the idea which this note vaguely indicates. It will be its aim to show that the various grades of psychical changes, from those of reflex action up to those of the most casual association of ideas, are explicable (physically considered) as consequences of the law of the line of least resistance; that in any part of the organism impressed from without there is some force generated or liberated; that this force must discharge itself through the organism, in some direction; that if, in consequence of the connexion of external phenomena, there is some other part of the organism affected about the same time in such way as to involve an expenditure of force (as the contraction of a muscle), the discharge will take place between these through the line of least resistance; that the discharge will on subsequent occasions follow this line with greater facility, and that the more frequent the repetition of these associated actions the less will become the resistance to the discharge; that hence there will arise all degrees of cohesion among nervous states answering to the degree of frequency of these associated actions; and that thus there will result a general correspondence between the cohesions of nervous states and those of external phenomena. I regret to have to forestall myself by now indicating these views; but it seems the only way of hereafter avoiding untrue suppositions respecting the derivation of the doctrine.

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* Essays: Scientific, Political, and Speculative. By Herbert Spencer. London, 1858.

My chief purpose here, however, is to set down certain morphological ideas quite distinct from those of Mr. Hinton, but quite in harmony with them. As already said, the speculations respecting internal morphology appended to the criticism on Professor Owen's archetypal theory are corollaries of certain wider speculations on external morphology which I have been long pursuing. It will, however, be some years before I can write out in detail the chapters which I purpose devoting to them; and as Mr. Hinton is studying the facts from a neighbouring point of view, and will probably incite others to do the same, it seems desirable at once to publish in a brief form the general conclusions arrived at. I cannot better introduce them than by narrating how they were first suggested.

In the autumn of 1851, during a country ramble with a friend,* I happened to pick up the leaf of a buttercup, and drawing it by its foot-stalk through my fingers so as to thrust together its deeply-cleft divisions, observed that its palmate

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* Mr. G. H. Lewes.

and almost radial form was changed into a bilateral one; and that were the divisions to grow together in this new position, an ordinary bilateral leaf would result. Joining this observation with the familiar fact that leaves, in common with the larger members of plants, habitually turn themselves to the light, it occurred to me that a natural change in the circumstances of the leaf might readily cause such a modification of form as that which I had produced artificially. If, as they often do with plants, soil and climate were greatly to change the habit of the buttercup, making it branched and shrub-like; and if these palmate leaves were thus much overshadowed by each other; would not the inner segments of the leaves grow towards the periphery of the plant where the light was greatest, and so change the palmate form into a more decidedly bilateral form? Immediately I began to look round for evidence of the relation between the forms of leaves and general characters of the plants they belonged to; and soon found some signs of connexion. Certain anomalies, or seeming anomalies, however, prevented me from then pursuing the inquiry much further. But after-consideration cleared up these difficulties; and the idea has since widened into a general doctrine of morphology which I will now proceed to illustrate.

Let us set out with a fact familiar to every one—the modification produced in the general outline of an individual plant by its special circumstances. We know that a tree which, if standing alone, sends out horizontal branches and acquires a bulky form, will, if it grows in the midst of a wood, have a tall taper stem, branches confined to its upper part, and almost vertically directed; or if it grows at the edge of a wood, will have its outer branches well developed, and its inner branches scarcely at all developed; and we know that when one side of a tree or plant is much shaded, its stem will lean to the opposite side. That is, the direction and length of the stem, as well as the directions, lengths, and distribution of the branches, are to a great extent determined by surrounding conditions.

Pass now to the closely allied but apparently unnoticed class of facts which the forms of branches exhibit. If we examine a common fir-tree—and I choose a fir-tree because the regularity in its mode of growth makes the law more than usually manifest—we shall find that the uppermost branches which grow out of the leading shoot, have radially-arranged branchlets: each of them repeats on a smaller scale the type of the tree itself. But if we examine branches lower and lower down the tree, we find the vertically-growing branchlets bear a less and less ratio to the horizontally-growing ones; until, when we approach the bottom branches, we find the radial arrangement has wholly merged into the bilateral one. Shaded and confined by those above them, these eldest branches develop their offshoots in those directions where there is most space and light; becoming finally quite flattened and fan-shaped. And when we remember that each of these eldest branches, when first it diverged from the main stem, was radial, we see that not only between the upper and lower branches does this contrast in structure hold, but also that each branch is transformed from the radial to the bilateral by the progressive change in its environment. The like general truth is readily traceable in other trees; though it is less conspicuous in proportion as the form is more heterogeneous, and the contrast of conditions therefore less constant.

In the arrangement of leaves upon a branchlet we see the same law as in the arrangement of branchlets upon a branch. The leading shoot, and all the upper twigs of a fir-tree, have their pin-shaped leaves evenly distributed all round, or placed radially;* but as we descend, we find them beginning to assume a bilateral distribution, and on the lower horizontally-growing branches the leaves are confined to the two sides. The contrast between the Irish and English species of yew, supplies us with an equally marked illustration. The branches of the one, shooting up as they do almost vertically, are clothed with leaves all round; while those of the other, which spread horizontally, have their leaves bilaterally inserted.

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* It should be remarked, that here and throughout, the word *radial* is applied equally to the spiral and the whorled structures. These, as being alike on all sides, are similarly distinguished from arrangements that are alike on two sides only.

In trees with better developed leaves, the same principle is more or less manifest in proportion as the leaves are more or less enabled by their structure to maintain fixed positions. When the foot-stalks are very long and slender, and when, consequently, each leaf, according to its weight, the flexibility and twist of its foot-stalk, and the direction of the branch it grows from, falls into some indefinite attitude, the law is obscured. But when the foot-stalks are stiff, as for instance in the laurel, it will be found, as before, that from the topmost and upward-growing branches the leaves diverge on all sides; while the undermost branches growing out from the shade of those above, have their leaves so turned as to bring them into a horizontal row on each side of the branch. To which facts add this, that single-stemmed vertical plants, as the palms, the aloes, the sempervivum, &c., have radially-arranged leaves.

The distribution of the leaflets in a compound leaf comes next in order, and equally illustrates the principle. When the foot-stalk grows up vertically out of the ground, quite separate from other foot-stalks, the leaflets it bears are arranged in radial symmetry—witness those of the common wood-sorrel and the *Marsilea*. When the compound ground leaves with upright foot-stalks diverge in considerable number from a common centre, as in some kinds of lupin, the leaflets, while still arranged radially, show a slight bilateral leaning in conformity with the difference in their conditions on the sides next the axis of the plant, and next its periphery; and in the leaves that grow out of the flowering-stem, the tendency to a transition from radial to bilateral symmetry is clearly marked. In various species of clover which from creeping or procumbent stems send out nearly upright leaf-stalks, we see a radial distribution of the leaflets passing into a bilateral distribution—the leaflets next to the stem tending outwards; and this peculiarity will be found marked in proportion as, from the habit of the species, the relations of positions are constant. Similarly with the potentillas. Here, in each compound leaf, the leaflets next to the axis of the plant are less developed than the others, and are to a considerable extent turned outwards; add to which, that in those kinds of potentilla that have short leaf-stalks, this outward bending of the inner leaflets is most decided. And when, as in most compound-leaved plants, the average direction of the petiole approaches to the horizontal, the distribution of the leaflets is entirely bilateral. Among trees, like illustrations are to be found. The horse-chesnut leaf, though composed of leaflets that have a proximately radial arrangement, yet shows, by the much greater size of the outer leaflets, the influence of contrasted conditions in producing the two-sided form. In the *Bombax* these relations are equally visible; while the great majority of compound-leaved trees, having petioles that on the average are more horizontal than vertical in direction, have leaves of a completely bilateral structure.

Noting as we pass that leaves that are transitional between the compound and simple, either from the coalescence of the leaflets or non-development of lobes into leaflets, furnish further instances, often of an instructive kind, as in *Cecropia*, we come now to the forms of simple leaves. Here, as before, when the conditions are alike on all sides, the development is alike on all sides. Water-plants that have flat, floating leaves, supported upon long independent foot-stalks growing from the bottom, have their leaves symmetrically peltate. In the *Victoria Regia*, the foot-stalks of which, through radiating almost horizontally from a centre, are so long as to keep the leaves quite remote from each other, the leaf is almost symmetrically peltate, with a seam over the line of the foot-stalk, giving a trace of bilateralness. The leaves of the *Nymphœa*, much more closely clustered, and having less room transversely than longitudinally, exhibit a marked advance to the two-sided form; not only in the excess of the length over the breadth, but in the existence of a cleft over the foot-stalk, where in the *Victoria Regia* there is only a seam. Among land-plants similar forms are found under analogous conditions. The *Hydrocotyle*, which sends up direct from its roots a few almost upright leaf-stalks, has these surmounted by peltate leaves; which leaves, however, are unsymmetrically peltate, in correspondence with the slight contrast of circumstances which their grouping involves. In the *Cotyledon umbilicus* we have a striking illustration. The two or three root-leaves that grow up on vertical petioles before the flower-stalk makes its appearance, are symmetrically peltate; while those which subsequently grow out of the flower-stalk are first transitionally bilateral, and higher up completely bilateral. Another case is supplied by the *Nasturtium*, which combines the characters—a creeping stem, long leaf-stalks growing up at right angles to it, and unsymmetrically peltate leaves, of which the least dimension is, on the average, in the direction of the stem. The great majority of leaves, however, possess not a radial, but a bilateral symmetry; in conformity with the fact that, in the great majority of instances, the circumstances of the leaf are decidedly different in the direction of the plant's axis from what they are in the opposite direction, while transversely the circumstances are as decidedly alike. How completely the two-sided symmetry is thus determined is still more clearly shown by the fact, that in plants whose bilateral leaves habitually grow in such positions that their opposite sides are exposed to conditions that are more or less dissimilar, their opposite sides are more or less dissimilar: the form becomes unsymmetrically bilateral. The leaves of the elm, which grow out horizontally from each side of the branch, have their two halves unlike, in correspondence with the fact that the inner half, growing next to the branch, is more restricted in its space than the outer half. Many pinnate leaves, as those of the *Heracleum*, or common cow-parsnip, illustrate the same truth under another form. Here the successive pairs of pinnæ, growing from the leaf-stalk at decreasing distances, severally have their remote sides more confined than their near sides; and their near sides are hence the larger. But perhaps the most striking case is that furnished by the genus *Bigonia*. Here the leaves grow out in pairs in such way that while their inner edges greatly interfere with each other, the outer edges have abundance of room; and the result is a form that is extremely unsymmetrical, or indeed asymmetrical.* To which let me add the further instructive fact, that while in those species of this genus where the mutual interference is great, the leaf-stalk is laterally, or almost laterally inserted, in *Bigonia auriformis*, which has very long leaf-stalks and so gives room to its leaves on all sides, they have become unsymmetrically peltate.

Another extensive class of evidence is offered in the forms of flowers. These are radial in structure in those cases where the parts are similarly conditioned on all sides, and also in those where the parts are similarly conditioned on the average, though not in each separate flower; while they are bilateral in those cases where, throughout the plant, the conditions of the flower in one direction are habitually different from those in another. Note, as the first fact, that all flowers which grow out terminally with their faces upwards, have a radial symmetry; as witness the genera *Crocus*, *Iris*, *Primula*, *Gentiana*, *Linum*, *Tulipa*, *Saxifraga*, *Dianthus*, *Stellaria*, *Oxalis*, *Lychnis*, *Rosa*, *Potentilla*, *Glaucinum*, *Papaver*, *Anemone*, *Ranunculus*. In the second place, observe that flowers which, growing from whatever point, have peduncles that are so flexible as to permit the mouths to hang directly downwards, are also radial; as in *Fuchsia*, *Cyclamen*, *Hyacinth*. There is, I believe, no case in which a flower having an axis habitually vertical, presents a bilateral form. Radial symmetry, however, is not confined to flowers which open out their petals into a horizontal plane; and though at first sight it would seem that in conformity with the general principle these alone should be radial, yet further consideration shows that there are sundry conditions under which this character may exist in flowers otherwise placed. Bearing in mind that the radial form is the primitive form—that, morphologically speaking, it results from the contraction into a whorl, of parts that are originally arranged in the same spiral succession as the leaves—it is manifest that we must expect the radial form to continue wherever there are no forces tending to change it. What now must be the forces tending to change it? They must be forces which do not simply affect differently the different parts of an individual flower; but they must be forces which affect in a like contrasted manner the homologous parts of other individual flowers, not only on the same plant, but on surrounding plants of the same species. A permanent modification of the radial into the bilateral form can be expected only in those cases where by hereditary transmission the effect of the modifying causes accumulates. That it may accumulate it is needful, not that the seeds of one or a few flowers only should carry the impress of the modifying causes, but that the majority of the seeds should do so. And that the majority of the seeds should do so, implies that the majority of the flowers have had their homologous parts subjected to like differentiating forces. Hence it follows, that where the flowers of a plant grow in such a way as to preserve no constancy in the relations of their parts to surrounding influences, the radial form will continue. Now there are, as we shall see, various special causes which entail this variability of position. If a plant bears flowers on a number of branches which have all inclinations, from the vertical to the horizontal—as do the apple, plum, hawthorn—it is clear that the individual flowers are placed in countless different attitudes; and hence that a slight modification produced in any one by its particular circumstances, being neutralized by some opposite modification produced in another, the structure remains radial. When, again, the flowers grow in upright panicles, it is clear that though very few of them open horizontally, yet that if their positions were averaged, the plane of the mouth would be horizontal, and there is therefore no bilateralness. Or when, as in the genus *Galium*, the panicles, growing out laterally, do not give an average horizontally to the faces of their flowers, we see that the great flexibility, alike of the plant itself and of the panicles, involves an inconstancy of position which prevents the radial symmetry from being changed. This inconstancy of relative position is produced in various other ways—by extreme flexibility of the peduncle, as in the Blue-bell; by the tendency of the peduncle to curl to a greater or less extent in different directions, as in *Pyrola;* by a spiral twist of the peduncle differing in degree in different individuals, as in *Convolvulus;* by a spiral twist in the unfolding petals, similarly various in degree, as in the *Malvaceæ;* or by extreme flexibility of the petals, as in *Lythrum*. Add to which, that in other cases the like general result arises from a progressive change in the attitude of each flower, as in *Myosotis*, where the unfolding of the stem entails a transition from an upward position of the mouth to a lateral position; or as in the *Cruciferæ*, where the like effect follows from a changed direction of the peduncle. But when, having examined all the various cases in which laterally-growing flowers remain radial, and having found that inconstancy in the relative position of parts characterizes them, we turn to cases in which the relative position of parts is more or less the same in all the flowers, we find a two-sided form. In illustration of this I may name the genera—*Orchis*, *Utricularia*, *Salvia*, *Salix*, *Delphinium*, *Mentha*, *Teucrium*, *Ajuga*, *Ballota*, *Galeopsis*, *Lamium*, *Stachys*, *Glechoma*, *Marrubium*, *Calamintha*, *Clinopodium*, *Melittis*, *Prunella*, *Scutellaria*, *Bartsia*, *Euphrasia*, *Rhinanthus*, *Melampyrum*, *Pedicularis*, *Linaria*, *Digitalis*, *Orobanche*, *Fumaria*, &c.; to which may be added, all the Grasses, and all the Papilionaceæ. In the most marked of these cases it will be observed that the flowers are sessile upon the side of an upright stem, and so are kept in quite fixed attitudes; and in the other cases the peduncles are very short, or else of such character as to secure a general uniformity in the positions. Where bilateral flowers grow terminally on an almost upright stalk, as in *Pinguicula*, and in the violet tribe, we see that the mode of growth is such that the flower always unfolds its parts sideways in the same relative positions. Where, as in *Verbena* and *Azalea*, we have a terminal cluster of flowers which exhibit a certain modification of the radial into the bilateral form, we perceive the change to be in correspondence with the contrasted conditions to which the different sides of the flowers are subject. Some terminal clusters, indeed, furnish us with very significant evidence. In our wild geraniums, for example, we find the singly-placed flowers symmetrically radial; but the cultivated species, in which the single flowers are replaced by clusters, display manifest bilateralness. And there are also cultivated varieties of primrose, in which the single flower being similarly developed into a group, there has similarly arisen in the flowers a decided two-sidedness in conformity with the two-sidedness of the new conditions. Allied species, growing wild, in some cases supply analogous illustrations. Thus, in *Scabiosa succisa*, which bears its numerous small flowers in a hemispherical knob, the component flowers, similarly circumstanced, are all equal and all radial; but in *Scabiosa arvensis*, in which the numerous small flowers form a flattened disk, only the much-confined central ones are radial, while at the edge we find them very much larger, and conspicuously bilateral. From these clustered flowers we pass naturally to the class *Syngenesia*, which furnishes us abundant and striking evidence. When, as in some genera, the calyx is largely developed, and overtops all the florets, these are equal and radial. But mostly it happens either that all the florets have become bilateral by the development of a strap-shaped corolla, the outer ones being the largest, or else the closely-wedged central florets are equal and radial, while the peripheral ones are different —commonly two-sided and strap-shaped. One of the most interesting examples is that of the *Centaurea;* the inner florets of which are small and vertical in direction, while the outer florets are large and lateral in direction. And here may be remarked, in passing, a clear indication of the effect which great flexibility of the petals has in preventing a flower from having its original radiate form modified; for while in *Centaurea cyanus* the large outward-growing florets having short stiff petals are decidedly bilateral, in *Centaurea scabiosa*, where they have long flexible petals, the radial form is scarcely at all modified. Of evidence, however, that is to be found in the structure of flowers, perhaps the most remarkable and conclusive is supplied by the *Umbelliferæ*. In some cases, as when the component flowers have all plenty of room, or when the surface of the umbel is more or less globular, the modifications are not conspicuous; but where, as in *Vibernum*, *Chærophyllum*, *Anthriscus*, *Torilis*, *Caucalis*, *Daucus*, *Tordylium*, *Heracleum*, &c., we have the flowers closely clustered into a flat surface, we perceive a number of remarkable modifications. In an umbel of *Heracleum*, for instance, note—first, that the external umbellules are larger than the internal ones; second, that in each umbellule the central flowers are less developed than the peripheral ones; third, that this greater development of the peripheral flowers is most marked in the outer umbellules; fourth, that it is most marked on the outer sides of the outer umbellules; fifth, that while the interior flowers of each umbellule are radial, the exterior ones are bilateral; sixth, that this bilateralness is most marked in the peripheral flowers of the peripheral umbellules; seventh, that the flowers on the outer side of these peripheral umbellules are those in which the bilateralness reaches a maximum; and eighth, that where the outer umbellules touch each other, the flowers, being unsymmetrically placed, are unsymmetrically bilateral.

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* These words are often used as equivalents; but it would be well to confine the one to imperfect symmetry, and the other to total absence of symmetry.

Throughout the animal kingdom the same general principles hold good; and are, indeed, still more readily traceable. Here, as before, those parts of the organism are similar which are similarly acted upon; and there results a form that is spherical, or radial, or bilateral, that is unsymmetrical or asymmetrical, according as the conditions determine.

In a class of gelatinous bodies, the *Thalassicollæ*, which float passively in the sea, and present in turn all their sides to the same influences, we have a globular form: being in every direction equally affected, the surface is in every direction of the same outline. The common *Gregarina*, immersed in nutrient fluids which it absorbs through all parts of its limiting membrane, is spheroidal; while in those species of *Gregarina* which are hooked to the surface of the intestine, and so have their parts somewhat differently exposed to the surrounding matters, we find an ovate or pyriform outline, showing the spherical symmetry merging into the radial.

Wherever creatures are so circumstanced that on all sides of some one axis the conditions are, on the average of cases, equal, we find a symmetrical distribution of parts round that axis—a radial symmetry. The *Cœlenterata* furnish various illustrations of this. The solitary polypes—hydroid or helianthoid—mostly stationary, and when they move, moving with any side foremost, do not, by locomotion, subject their bodies to any habitual contrast of conditions. Mostly fixed, with their mouths upwards or downwards, or if laterally, then at all varieties of inclination, the individuals of a species taken together are subject to no gravitative action affecting some part of the disk more than others: nor indeed would gravitation be likely to influence bodies so nearly of the same weight as their medium. And in respect of their relations to prey, there is nothing in the circumstances of these animals involving that any one side of the disk will be differently acted on from any other. Similarly with the compound *Hydroida* and *Helianthoida.* Averaging the conditions of the individuals united in a group, there is no force tending to produce any other than the radial form. Again, among the Monostome and Rhizostome *Medusæ*, we find under another set of conditions the same general truth. Advancing through the water by the contraction of a disk, there is nothing in the locomotive action of these animals which reacts more upon some sides than others; and as, from accidental causes, all points on the edge of the disk come upward in their turns, there is no side permanently affected in a different way from the rest, and no side becomes unlike the rest. But among other groups of the *Acalephæ*—as the *Diphydæ*, which move through the water in a more or less constant attitude—there is decided bilateralness. And here I may fitly notice the objection which some may feel to this and other such illustrations, that they amount very much to physical truisms. It may be said, that if the parts of a medusa are disposed in radial symmetry around the axis of its motion through the water, there will of course be no means of maintaining a constant attitude, and the equality of conditions may be ascribed to the radiateness as much as the radiateness to the equality of conditions; while conversely, where the parts are not radially arranged round the axis of motion they will necessarily gravitate towards some one position, implying a balance on the two sides of a vertical plane—a bilateralness; and that the maintenance of the two-sided conditions may be as much ascribed to the bilateralness, as the bilateralness to the two-sided conditions. Here seems an awkward criticism. The form and the conditions are to this extent necessary correlates; and in so far as it asserts this, the objection harmonizes with the argument. But to the implied question—Why make the form the result of the conditions, rather than the conditions the result of the form? the reply is, that as in all the various illustrations that have been cited from the vegetable kingdom the conditions are manifestly the antecedent, and the form the consequent, it may be fairly presumed that the like relation holds throughout the animal kingdom.

Returning now to the facts, we have to note that the various divisions of the *Echinodermata* furnish other evidence. The ancient *Cystidea* and *Crinoidea*, with rooted upright stems, had a radial distribution of parts. In the *Asteriadæ* and *Ophiuridæ*, which move over rocks and weeds with now one and now another of their rays foremost, the average of the actions is alike on all sides, and the radial form continues. In the *Echinidæ*, however, while some genera—as the common Sea-urchins—show no manifest divergence from this type, others—as *Echinocyamus*, *Spatangus*, *Amphidotus*—have a marked bilateralness; and as far as I can learn, this difference corresponds with the fact, that whereas in the one group the movement is indiscriminately in any direction, in the other it is habitually in one direction.

Among Annulose animals the relation between the kind of symmetry and the nature of the conditions is still more marked. In the first place, we see that the lowest forms, which habitually move through a homogeneous medium pressing equally on all sides—as those that wriggle through the water by undulations, that are not in one plane, or through the earth by means of contractions—have their substance symmetrically disposed on all sides of the axis of motion—the tranverse section is circular: whatever may be the distribution of the internal organs, the external outline is radial. And in the second place, we see that where the motion is habitually *over* a surface, or through a medium specifically lighter than the body—that is, where the conditions are alike on the two sides of the line of movement, but unlike above and below it—then we have a conspicuous bilateral symmetry: as witness the *Articulata, Insecta, Arachnida.*

Throughout the vertebrate sub-kingdom, the relation between shape and conditions is so uniform and simple as to need no special examples. We all know that every vertebrate animal habitually moves with one end foremost, and that it keeps its flanks habitually balanced on the two sides of a vertical plane; and we all know that in correspondence with these conditions the form is bilaterally symmetrical: the only exceptions being the cases in which, as in the sole, the lateral surfaces are not habitually balanced on the two sides of a vertical plane, and in which, consequently, the bilateralness is unsymmetrical.

Among the *Mollusca* the evidence is varied and instructive. Only the leading facts, however, can here be noticed. "In all Mollusks the axis of the body is at first straight, and its parts are arranged symmetrically, with regard to a longitudinal vertical plane, just as in a vertebrate or an articulate embryo."* This bilateral symmetry is more or less retained in the Pteropods, the Cephalopods, the naked Gasteropods, which severally preserve an habitual attitude during their motions either through the water or over the surfaces they frequent. Is it not the fact, too, that among the Lamellibranchs, those which are habitually locomotive, as the fresh-water mussel, and those which are habitually fixed in such way that the opening of the valves is in all varieties of direction, as the marine mussel, preserve their bilateral symmetry; while those which are rooted in such way that one of the originally similar halves is constantly downwards, as the oyster, lose their bilateral symmetry? But the fact of most significance is, that among the cephalous Mollusca, when by over-development of some parts the symmetry of the body at large has been destroyed, "this symmetrical over-development never affects the head or foot of the mollusk:" only those parts which by enclosure in a shell are protected from environing actions, lose their bilateralness; while those external parts which, through the movements of the creature, remain subject to bilateral conditions, remain bilateral. Indeed, whoever refers to the anatomy of the common whelk, and after observing how not only the outline of the head and foot are two-sided, but also the arrangement of the nerves in the external part of the body, then notes how the two-sidedness of form and organization merges into an asymmetrical structure where the body becomes covered by the shell, will see how striking an illustration is thus afforded of the alleged general law.

And this case fitly introduces the remaining class of evidences derivable from the animal kingdom—that exhibited in the contrasts between internal and external forms. It is a familiar fact, that among animals in general, while the organs of animal life are disposed symmetrically, the organs of vegetative life are disposed asymmetrically. That is to say, while those parts of the body which are conversant with environing actions that are similar in certain directions, display similarity in corresponding directions, those parts of the body which are not in immediate relation to environing actions display no correspondence with them in distribution.

Having to pass rapidly over so wide a field, I have of course been unable to do more than set down the most salient facts; and have necessarily been obliged both to pass over a great mass of evidence by which the argument might be enforced, and to say nothing of objections which explanation would readily meet. I venture to think, however, that the proofs assigned go a considerable way towards establishing the position, that the forms of all organisms are dependent on their relations to incident forces: including under this title both those forces to which they are passively subject, and those which they experience as the reactions of their own actions. We find—

1. That where the conditions are alike in all directions—either constantly, as in the *Gregarina* ,or on the average of successive instants, as in the *Volvox* and

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* Professor Huxley on the Morphology of the Cephalous Mollusca.

the *Thalassicollæ*, or on the average of many individuals, as in the *Protococci*—there we have symmetry of three dimensions, or spherical symmetry.

2. That where the conditions are alike on all sides of one axis, but different in line of this axis—either constantly, as in the mushroom, and in vertically-growing plants and trees, in peltate leaves and terminal flowers; or on the average of successive instants, as in many echinoderms; or on the average of many individuals, as in irregularly-placed flowers and in zoophytes—there we have symmetry of two dimensions, radial or circular symmetry.

3. That where the conditions are alike only on the opposite sides of one axis, while they are unlike in all other directions—as in the majority of leaves, in many flowers, in the higher classes of the *Annulosa*, and throughout the *Vertebrata*—there we have symmetry of one dimension, bilateral or linear symmetry.

4. That where the conditions, widely unlike in all other directions, are only partially alike on the opposite sides of one axis—as in a few flowers, in many leaves, in the Sole and the Hermit crab—there we have unsymmetrical bilateralness.

5. And that where the conditions are indeterminate as with the *Amœba*, or have no similarity in any directions, as with the viscera of the higher animals, there the structure is asymmetrical.

Of course the general principle expressed in these several propositions cannot, taken alone, explain all the facts of morphology: otherwise it would tell us why two different seeds, though similarly circumstanced, unfold into different plants, which it does not. But it is quite possible that, if joined with the law of hereditary transmission, understood in its most transcendental sense, it may be the general principle underlying all morphological phenomena. For the interpretation of any form we have to take into account—first, the hereditary type; second, the action of the general external forces of gravity, light, heat, &c.; third, the action of more special external forces, as those of local currents when the organism is stationary, or those of resistance when it is locomotive; fourth, the action on the whole organism and its separate parts, of those forces dependent on increase of size; and fifth, the actions produced by the parts on each other by change of relative development. Of these factors, the first is manifestly by far the most important; and indeed appears to dwarf all the rest into insignificance. But, as just hinted, it is very possible that this first is but the cumulative result of all the rest. It may be that each hereditary type has arisen by the repeated superposing of modifications upon modifications—each new set of modifications being produced by a change in the relations to incident forces. And the universal harmony which, as we have seen, exists between the forms and the distribution of the forces, is strongly suggestive of this supposition. It is indeed true that throughout the animal kingdom this harmony may be regarded as a necessary consequence of teleological adaptations; though even here there are numerous phenomena not thus explicable. But such an hypothesis affords no explanation whatever of the morphology of plants. Designed fitness of form to function does not in the least account for the different outlines of leaves; nor does it show us why one flower should be radial and another bilateral. Here, however, where the doctrine of final causes fails utterly, we find innumerable illustrations of the dependence of form on conditions—alike in individual plants and in races of plants. The cases, awhile since noted, in which from increase of nutrition a single terminal radial flower is transformed into a group of flowers, which take on a bilateral form in consequence with their new positions, and transmit this bilateralness to their descendants, gives us the key to vegetable morphology in general. And those who are duly impressed with the unity which pervades the organic creation, will see it to be highly probable that animal morphology conforms to the same laws.

I will only add, that the foregoing generalizations alike confirm, and are confirmed by, the argument respecting the origin of vertebral forms. Indeed, in the absence of the untenable hypothesis of an ideal type, it seems to me that many of the facts admit of no conceivable explanation save one based on the general principles briefly indicated in the preceding pages.

Art. II.

On the Supposed Antagonism of Consumption and Ague. By Thomas B. Peacock, M.D., F.R.C.P., Assistant Physician to St. Thomas's Hospital, and Physician to the Hospital for Diseases of the Chest, Victoria Park. Read before the Hunterian Medical Society, Nov. 3, 1858.

In the year 1802, Dr. Harrison,* of Horncastle, in Lincolnshire, in an address delivered to a local medical society, remarked upon the infrequency of pulmonary affections, and especially of consumption, in the fens, as compared with other districts, not marshy, within the limits of his practice.

In 1811, Dr. Wells contributed a paper to the Society for the Improvement of Medical and Chirurgical Knowledge,† in which he contended that consumption and intermittent fever were opposed to each other, and quoted, in support of his views, the observations of medical writers in different countries, to the effect that in localities where aguish affections were common, consumption was rare, and that with the decline in the prevalence of the former disease the latter became more frequent.

In 1841, M. Boudin,‡ who was medical officer to the French Contingent in the Morea, and while serving at Marseilles saw much of the diseases of the troops in Algeria, strongly contended for the antagonism of phthisis and ague. Shortly after, the question was discussed at the Académie de Médecine,§ and much correspondence ensued in the medical journals. M. Boudin's views were, however, by no means generally received; and some observers maintained that, so far from the two diseases being opposed to each other, Laennec was probably more correct in regarding the pernicious or malignant intermittent and remittent fevers as conducing to the developement of phthisis. More recently, M. Boudin‖ has maintained his opinion, but the question must be regarded as still *sub judice*, and the following contribution towards its illustration may therefore not be without its value.

The arguments advanced by M. Boudin are:

First, that wherever endemic fevers are prevalent, consumptive diseases are rare, and that the frequency of the one class of cases is inversely proportionate to that of the other.

Secondly, that a decrease in the prevalence of malarious affections in different districts has been followed by an increase in the number of consumptive cases; and

Thirdly, that phthisis is more curable in aguish districts than in other situations.

First. In reference to the less prevalence of phthisis in aguish localities, M. Boudin has published statements of the relative frequency of the two affections in different parts of Europe, as estimated from the number of cases treated in hospitals and the mortality among the general population, and he has also availed himself of the similar information afforded for other parts of the world by the English army reports. In reference to the latter, it has been shown by M. Genest¶ that they do not bear out the inference they have been advanced to support; and it is evident that all calculations of this kind are open to two fallacies—first, that the small proportion of cases of phthisis may be relative and not absolute, depending on the great prevalence of other forms of disease; and secondly, that a chronic affection, like phthisis, may be of less frequent occurrence in consequence of many persons who would otherwise have fallen victims to it being carried off by the more acute and rapidly fatal malady.

Secondly. In support of the assertion that in districts formerly subject to malarious diseases, the decline in their prevalence is followed by increase in the number of phthisical patients, both Dr. Wells and M. Boudin instance the alterations which took place in the sanitary condition of London in the last century. During the seventeenth and to the middle of the eighteenth century, remittent and intermittent fevers were both prevalent and fatal in the metropolis, while at the end of the last century they had almost entirely disappeared, the cases which occurred in practice being generally imported from the adjacent marshy districts, or the fens of Lincolnshire and Cambridgeshire.* While this change was proceeding, we are informed by Dr. Wells that the prevalence of phthisis increased; at the beginning of the century the cases of consumption entered in the bills of mortality constituting only one-eighth of the whole number of deaths, while at the end they amounted to fully one-fourth. Reference to the bills shows, however, that these statements are in no degree to be regarded as exact. In 1700, the cases of consumption are included with asthma and bronchitis under the head of tissick, and we cannot therefore estimate the proportion which they bore to the other forms of pulmonary affection; but in 1750 the consumptive cases alone amount to 4543, and all the other pulmonary diseases, including tissick and hooping-cough, to 558. These numbers are, however, very different from those which we now know to represent the relative prevalence of these different affections. Thus in the Registrar-General's Report for 1855, the cases of consumption entered among the deaths in the metropolis amounted to 7656, and those from all other pulmonary affections, including hooping-cough, croup, and influenza, to 14,403, or nearly double the number. It may, therefore, safely be inferred that in the bills of mortality at the former period many deaths were entered as from consumption which were really due to other forms of pulmonary disease. Dr. Fothergill,† indeed, states that the number of cases of consumption returned in the bills was excessive; but it is evident that before his time the frequency of the disease had given it among foreigners the reputation of being the peculiar endemic of the island.

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* Med. and Phys. Journal, vol. viii. 1802, p. 221.

† Transactions, vol. iii. 1812, p. 471.

‡ Traité des Fièvres intermittentes, p. 103. Paris, 1842; and Etudes de Géographie Médicale. Paris, 1845 and 1846.

§ The discussions will be found reported in different numbers of the Gazette Médicale de Paris, année 1843, deuxième série, tom. xi.

‖ Traité de Géographie et de Statisque Médicales, tom. ii, p. 684. Paris, 1857.

¶ Gaz. Méd. de Paris, deuxième série, tome xi. pp. 578 and 650.

Were, however, the bills of mortality at that period entirely to be depended upon, they would not warrant the inferences which have been drawn from them; for in the year 1750, when the different forms of aguish affection were still both prevalent and fatal, and when the drainage of London and its environs was very defective, the deaths from consumption constituted a much larger proportion of the total mortality than at present, when the sanitary condition of the metropolis is so greatly improved. Thus, in that year, the deaths from consumption were one-fifth of the whole number registered, whereas in 1855 they constituted only one-eighth of the mortality from all specified causes, or more exactly, 13 per cent. in males, and 11·7 per cent. in females. For the investigation by statistical data, of the effects of improved drainage in the metropolitan districts upon the relative prevalence of ague and phthisis, we are not in possession of accurate observations extending over a sufficiently prolonged period; and though medical men practising in this and other countries have reported, that in their districts the decline in the prevalence of aguish affections has been followed by an increased fatality from phthisis, observations of this kind do not possess the exactitude of numerical statements.

Thirdly. It has long been, and to some extent is still, a popular notion, that phthisis is prevented or rendered more curable by residence in malarious districts; and Dr. Harrison informs us that consumptive patients were in his time sent to the fens in the hope that the disease would be arrested. He also instances the case of a delicate female who having removed from a marshy district, presented symptoms of consumption, and which subsided on her return into the fens. Dr. Wells recommends that consumptive patients should be sent to reside in marshy districts; and M. Boudin has published a memoir‡ by M. de Crozant, in which the particulars are given of four cases of consumption which were cured in an aguish locality. Two of these had been under the care of M. de Crozant, and the others occurred in the practice of M. Lizon, of the town of Donzy, in the department of the Nièvre. This town, which contains between two and three thousand inhabitants, is situated on the banks of the Noain, a tributary of the Loire, and is surrounded by marshes, and ague is prevalent in the district, while consumption is very rare; so rare, indeed, that M. Lizon, during a period of twenty years, had only seen seven cases, of which the two before referred to were cured. On the other hand, a neighbouring practitioner, M. Gambon, reported that in adjacent districts which were not marshy, as at Cosne, and where ague did not prevail, consumption was a common complaint. Similar observations have been recorded by M. Bérenguier of the district of Rabastens, in the department of Tarn,[*] and by other writers. Facts of this kind, however, do not prove the opposition of consumption and ague, or that the malarious poison is capable of preventing the occurrence of phthisis. For, supposing it to be true that consumptive affections are less prevalent and more curable in marshy districts, it may be, as was supposed by Dr. Harrison, the "soft and moist" character of the air which exercises the beneficial influence.

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* Sir Gilbert Blane, Med. Chir. Trans., vol. iv. 1813; and Select Dissertations, p. 115. 1822.

† Works by Lettsom, 1784, 4to, p. 89. On Weather and Diseases, 1751.

‡ Etudes de Géographie Médicale, p. 18. 1846.

1. The question of the antagonism of consumption and ague would appear to admit of ready solution by comparing the mortality from phthisis in different districts of a similar character, except that, in some, aguish affections should be prevalent, and, in others, that they should be absent or only rarely seen. Dr. Greenhow, in his able and elaborate report on the sanitary state of the people of England,[†] has inserted a table giving the proportion of deaths in males and females, calculated on the respective numbers living, from ague, phthisis, and other diseases of the respiratory organs, deduced from the returns to the Registrar-General for the nine years 1846 to 1854 inclusive, in the Wisbeach, Spalding, Whittlesey, North Witchford and Stroud districts, in which ague prevails; and in Richmond in Yorkshire, Leominster, Leighton Buzzard, Wycombe and Liskeard, in which no deaths from ague were reported. From this table it appears that the prevalence of phthisis in the non-malarious districts is less than in the similar aguish districts; thus, he remarks that Liskeard contrasts favourably with Wisbeach, Wycomb with North Witchford, Leominster with Whittlesey, and Richmond with Spalding, and Dr. Greenhow concludes, that while "it would be a too hasty inference to affirm from data of so limited a character, that the opinion that malarious influence is unfavourable to the development of phthisis, is altogether unfounded, the present facts, at least, afford it no support."

The districts compared by Dr. Greenhow correspond closely as to the amount and density of the population, and the proportion of persons residing in towns and in country situations; but no account is taken of the relative number of persons at different ages, nor of the influence of climate, though these are most important considerations in estimating the relative prevalence of consumption in different localities.

To effect a satisfactory comparison, the districts compared must not only contrast as regards the prevalence of ague, but they must possess similar climates, and must correspond in the density of the population, the proportion of persons at the ages most prone to consumption, and in the social position and occupations of the inhabitants. The population must be but little affected by immigration, and the mortality must not be modified by the existence of large public hospitals or institutions in which persons are received from other localities.

It will be at once seen that it is not easy to select districts which combine all these requirements. Similarity of climate will best be obtained by adopting adjacent districts, but districts which are adjacent are seldom decidedly opposed as regards the prevalence of ague. Thus the Dartford and Bromley registration districts might at first sight appear very suitable for comparison, but it will be found that the Dartford[‡] district, though generally malarious, embraces portions which are healthy, while that of Bromley,[§] though generally healthy, is in some parts malarious, and thus the death-rate from ague is high in both districts.[‖]

Of the metropolitan registration division, the Greenwich and Lewisham districts have a high rate of mortality from ague and remittent fever,[¶] while there are other districts in which these affections very rarely occur,[*] yet these different localities do not admit of satisfactory comparison for the purpose of determining the influence of malaria on the prevalence of phthisis. Indeed, the change in the metropolitan population from immigration, the excess of persons at certain ages, the differences in their social position, and the various hospitals, workhouses, barracks, &c., vitiate all calculations of the relative frequency of phthisis and ague in different districts.

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[*] Annales d'Hygiène Publique, tome 38, p. 251. 1847.

[†] Page 109.

[‡] Including the sub-districts of Bexley, Dartford and Farningham.

[§] Including the sub-districts of Bromley and Chislehurst.

[‖] The rate of mortality from ague, estimated on the supposition of the population consisting of 100,000 persons, was respectively 3·8 and 6·8 annually for the 5 years 1851 to 1855.

[¶] Or 2·4 from ague only, and 7·04 from remittent fever in Greenwich, and 1·7 from ague, and 10·4 from remittent fever in Lewisham.

It will thus be seen that the selection of the localities to be compared requires careful consideration. For comparison, I have collected from the manuscript tables contained in the Registrar-General's Office, the returns of the deaths from certain causes in some of the districts bordering upon the Wash, and have compared them with similar facts relative to other districts immediately adjacent. For the opportunity of consulting the returns, I am indebted to Dr. Farr and Mr. Hammack, of the Registrar-General's Office. The period over which the comparison extends is the years 1851-52-53-54 and 55, and the districts selected are those of Wisbeach, Whittlesey, North Witchford (including March and Chatteris in the Isle of Ely), Caxton and Chesterton in Cambridgeshire; Oundle and Thrapston in Northamptonshire; Huntingdon, St. Ives and St. Neots in Huntingdonshire; and Biggleswade in Bedfordshire.[†] These districts are most of them but slightly elevated above the level of the sea, and must correspond closely in climate. Geologically they are either fenny, or are situated upon the lower greensand formation and upon the upper, middle and lower oolites. They lie within a short distance of the Wash, and between and on either side of the rivers Nene and Ouse. The population is chiefly engaged in agriculture, the only manufactures of any importance carried on in any of the districts being the straw-plait, which prevails in Biggleswade and to a limited extent in Caxton; lace-making, which is somewhat extensively followed in St. Neots and Thrapston, and slightly in Oundle and Caxton; and shoemaking, which is carried on in Thrapston. The density of the population in the different districts is very similar, the towns are all of small size,[‡] the proportion of persons at the ages most prone to consumption does not differ materially, and the population is a moderately stationary one.[§] The districts also afford the required contrast in the prevalence of aguish affections. In Caxton no death from these causes was registered during the five years selected for comparison; in Chesterton, St. Ives, and Biggleswade, the mortality from this cause was small; and in St. Neots, Whittlesey, Oundle, Thrapston, Wisbeach, North Witchford and Huntingdon, a large proportion of deaths occurred.

In the annexed table will be found the population of these several districts, as ascertained in the census of 1851, the extent of the districts, the proportion of persons to the square mile in each, and the relative proportion of persons between the ages of fifteen and forty-five inclusive; together with the annual proportion of deaths from ague and remittent fever combined, from ague only, and from phthisis, other diseases of the respiratory organs, and all causes, estimated on the supposition that the population of each district amounts to 100,000 persons of both sexes, and of each sex separately. The districts are arranged according to the relative mortality from aguish affections, and this is inferred from the number of recorded deaths from both ague and remittent fever, as the former disease very rarely proves fatal, but rather terminates in the form of remittent fever. As however the correctness of the returns of deaths under the latter head may be open to question, a column is added, giving the proportion of deaths from ague only,

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[*] In all the Southern and Eastern districts of the metropolis, in which the largest proportion of the working population reside, aguish affections are reported as causing death, yet in none of the districts, except the two named above, do such diseases originate at all frequently. I have for long been in the practice of inquiring among the persons who apply at St. Thomas's Hospital, labouring under ague, where the disease has been contracted, and with very few exceptions it turns out that the patients have either had ague in some well-known marshy district, or have come from such locality very shortly before the commencement of the symptoms.

[†] The adjacent Bedford district cannot be included in the calculation, for the town population is much larger than that of the other districts, and a large proportion of the inhabitants are employed in the unhealthy occupation of lace-making, and thus the mortality from phthisis is disproportionately high, being in the two sexes 285 for 100,000 persons living, or 207·7 in males, and 359·8 in females.

[‡] The Census Report for 1851 does not give the relative proportion of the population residing in urban and rural situations, and this is only approximatively obtained by comparing the population of the towns with that of the general district. But in the whole of the county of Huntingdon the respective proportions are for the towns 25, and for the country 75. In Cambridge 31 and 69. In Northampton 28 and 72. In Bedford 30 and 70.

[§] The proportion of persons resident out of the population, estimated at 10,000, who were born in the county of Huntingdon at the time of the last census, was 6994; in Northampton, 7586; in Cambridge, 7155; and in Bedford, 7577. In the London districts the proportion is 6167. In Cornwall, where the least change takes place, the proportion is 9322. In the extra metropolitan parts of Middlesex, where the greatest change occurs, the proportion was 4604.

TABLE *of the relative mortality from certain causes, showing the population, area, density of population, and the annual proportion of deaths from aguish affections, (including ague and remitting fever), Ague alone, Phthisis, other diseases of the Respiratory Organs (including Croup, Influenza, Hooping-cough, and all diseases of the Registrar-General's Class 6), and All Causes, in certain Registration Districts. The calculations are based on the supposition that the population of each district, and of the males and females separately, amounted to 100,000. The data are derived from the MS. tables of the causes of death, in the Registrar-General's Office, for the years 1851 to 1855 inclusive. The districts are arranged according to the degree of prevalence of malarious affections.*

Districts.	Sub-Districts.	Population in 1851.			Area in Acres.	Persons to Square Mile.	Proportion of population per 1000 between ages 15 and 45 each.		Proportion of deaths from aguish affections.		Ague only both Male and Female.	From Phthisis.		From other diseases of Respiratory Organs.		From all Causes.	
		Total.	Males.	Fem.			Mal.	Fem.	Mal.	Fem.		Mal.	Fem.	Mal.	Fem.	Mal.	Fem.
Caxton, ...		11,065	5,740	5,325	49,885	142	450	447	0	0		181·	326·7	195·1	138·9	1775	1859
							M. & F. 449				0	251·2		168·		1816	
Chesterton, ...	Willingham, Fulbourne, Great Shelford.	25,170	12,728	12,442	76,728	210	486	470	1·5	1·6		256·	364·	420·3	215·3	2053	1994
							M. & F. 478		1·5		1·5	309·8		316·2		2024	
St. Ives, ...	Warbays, Somersham, Swavesey, St. Ives.	20,594	10,323	10,271	62,930	210	464	470	1·9	1.9		240·2	334·9	253·8	221·9	2032	1900
							M. & F. 467		1·9		·97	287·4		237·9		1966	
Biggleswade, ...	Potton, Biggleswade.	23,436	11,619	11,817	58,041	258	472	484	3·4	5·08		196·2	294·4	407·	321·	1950	2256
							M. & F. 478		4·2		·85	245·5		364·		2089	
St. Neots, ...	St. Neots, Kimbolton.	18,825	9,309	9,516	65,256	185	466	475	4·2	10·5		191·2	266·9	322·2	323·6	2013	1834
							M. & F. 470		7.4		2·1	228·8		322·9		1923	
Whittlesey, ...		7,687	3,702	3,985	25,181	196	465	472		17·5		248·5	220·8	318·7	301·1	2393	2148
							M. & F. 469		7·8		0	248·1		309·6		2266	
Oundle, ...	Oundle, Weldon, Fotheringay.	15,655	7,871	7,784	69,822	143	479	448	12·7	5·1		180·4	220·9	210·9	156·6	1951	1790
							M. & F. 463		8·9		8·9	200·5		183·9		1871	
Thrapston, ...	Thrapston, Raunds.	12,841	6,426	6,415	51,188	161	466	481	9·4	9·3		180·5	277·4	230·3	202·6	2097	2042
							M. & F. 475		9·3		6·07	228·9		216·4		2060	
Wisbeach.	Levington, Wisbeach, Walpole, St. Peters, Terrington, St. Clement, Walsoken, Upwell.	36,215	17,911	18,304	181,585	186	488	499	5·5	14·2		216·6	240·3	378.5	347·4	2358	2157
							M. & F. 494		9·8		4·4	228·9		362·8		2256	
NorthWitchford	Chatteris, March.	16,243	8,101	8,142	59,823	174	460	483	14·8	7·3		180·2	225·4	308·5	240·7	2039	1898
							M. & F. 472		11·		3·6	217·9		274·6		1968	
Huntingdon, ...	Ramsey, Sawtrey, Spaldwich, Huntingdon.	20,900	10,392	10,508	77,180	173	483	465	26·9	47·5		202·	252·	337·2	327·	2017	1989
							M. & F. 474		37·3		27·7	225·8		332·		1977	

and it will be observed that their number corresponds generally with the results deduced rom the two diseases combined.

The comparison here instituted is open to objections, but I believe it to be sufficiently accurate for the purpose. From an inspection of the Table it will be seen that, taking the prevalence of malarious affections as indicated by the deaths in both males and females, the places in which aguish affections were least fatal are those in which the largest number of deaths from phthisis were registered, and vice versâ. Thus in Caxton, where no death from malarious causes was registered, and Chesterton and St. Ives, where the deaths only amounted to 1·5 and 1·9 annually per 100,000 persons living, the proportion of deaths from phthisis was 251·2, 309·8, and 287·4; while at Huntingdon and North Witchford, with the deaths from malarious causes amounting to 37·3 and 11, the proportion of cases of phthisis was 225·8 and 217·9. But on looking more closely at the table it will be found that this rule does not apply throughout. Thus Biggleswade and Whittlesey, where malarious affections prevail to the extent of 4·2 and 7·8, lose 245·5 and 243·1 from phthisis. While Oundle, with a large prevalence of ague, 8·9, has the lowest proportion of phthisis, or 200·5.

If the deaths from phthisis in the several districts in males and females separately, be compared with the prevalenee of malarious affections, similar discrepancies will be found to exist. Thus among males the fatality of phthisis is seen to be almost identical in Caxton (181) without any death from malaria; in Oundle (180·4), where the deaths were 8·3; in Thrapston (180·5), where they were 9·3; and in North Witchford (180·2), where they were 11. So also there is but little difference between Biggleswade (196·2), where the malarious deaths were 4·2; St. Neots (191·2), where they were 7·4; and Huntingdon (202), where they were 37·3; and between Chesterton, St. Ives, and Whittlesey, where the malarious deaths are 1·5, 1·9, and 7·8, and the phthisical deaths were 256, 240·2, and 248·5.

The deaths from phthisis in females exceeded those in males in all the districts except Whittlesey; but the relative mortality varies equally in different districts, and bears no regular relation to the prevalence of aguish affections. The rate is highest in Chesterton, Caxton, Biggleswade, St. Ives, Thrapston, St. Neots and Huntingdon, and lowest in Whittlesey, Oundle, North Witchford, and Wisbeach. While therefore, as a general rule, in the districts compared, a large prevalence of aguish affections coincided with a low rate of mortality from consumption, and a small prevalence of ague with a high rate of mortality from consumption, this rule is liable to such marked exceptions that we are not warranted in inferring that the susceptibility to phthisis is in any great degree influenced by the causes which give rise to ague.

The difference in the prevalence of phthisis in the different districts, as shown in the table, are doubtless referable to a variety of causes, among which may be mentioned the proportion of the population at the periods of life at which phthisis most generally occurs, the nature of the occupations and social condition of the inhabitants, and the sanitary conditions of the towns and villages, and especially of the cottages, in which they reside. To investigate the extent to which these several causes are influential would require more leisure and greater knowledge of the part of the country than I possess. Had the period over which the calculation extends been a longer one, it is quite possible the differences would have been less marked; but this exception rather applies to calculations of the prevalence of diseases of the epidemic class, and so chiefly influences the statement of the relative proportion of deaths from other diseases of the respiratory organs and from all causes.

II. The information elicited by statistical investigation being thus only negative, it remains to inquire whether clinical observation be capable of yielding more positive results.

Dr. Wells states that he was not aware whether a person affected with pulmonary consumption is more capable of resisting ague or not; but that Dr. Caldwell, of Philadelphia, had observed such to be the case. My own experience is opposed to that of the American author. During the last two years I have met with five cases of the co-existence of consumption and ague at St. Thomas's Hospital, and a sixth at the Victoria Park Hospital. The particulars of these cases I shall proceed briefly to narrate.

Case 1.—Ague occurring in a person labouring under phthisis, and the combined diseases proving fatal:—

Susan Roberts, aged 34, a married woman, residing at Woolwich, was admitted into St. Thomas's Hospital, under my care, on the 15th of September, 1856. She stated that she had been suffering for five months from symptoms of irregular ague, and had gradually got worse notwithstanding that she had had medical advice. The attacks came on generally every forenoon: she had not decided rigors, but felt chilly for about three hours, and then became hot and perspired profusely. She had a malarious aspect; the pulse was quick and feeble; the tongue slightly furred; and the bowels confined. She also complained of cough, and was very thin. She was directed to take two grains of quinine three times daily.

Up to the 20th she had an attack each day, and the last seizure was the most severe; the quinine was increased to three grains, and wine was ordered. On the 4th of October, four grains of quinine were given, and the quantity of wine was increased. On the 8th, the following notes were taken—she has continued to have attacks every other day; they come on at 8 A.M., but are slighter than before, and the last two have not been followed by perspiration. Her face is pallid and thin, her eyes glassy, and her general aspect malarious; the pulse is quick and feeble; she takes her food well; and the bowels are acted upon once or twice in the twenty-four hours. She states that she has always been delicate, and, two years ago, she had inflammation of the chest, with pain in the right side, difficulty of breathing and cough, but no expectoration; she, however, once spat what looked like blood; but her medical attendant told her it did not come from the chest. She took much cod-liver oil at that time. She was then living on the sea-coast of Lincolnshire, but never had ague till she came to live at Woolwich. Her father and mother are living, but she has lost one brother and three sisters of whom a brother and sister died of consumption. Her husband has had three attacks of ague since he resided at Woolwich, and had been each time under treatment by myself. At the present time she has a cough, and much expectoration. There is some deficiency of resonance on percussion at each apex, and falling in, especially on the right side. Bronchial respiration is also there heard, and with a full inspiration there is some subcrepitation. At the left apex the signs are less marked. There is some deficiency of the resonance on percussion, low down on the right side, both before and behind, subcrepitation is there audible. To continue the quinine, and take the cod-liver oil; ʒ ij. bis quotidie; 3 glasses of wine. On the 10th, the dose of oil was increased to 3 drachms. She died suddenly, without any obvious cause, and apparently syncopic, when up in the ward, on the 15th.

Post-mortem examination.—The pericardium was healthy; the opposed surfaces of the right pleura were so firmly attached by old adhesions as to render it difficult to remove the lung entire; the left lung was adherent at the upper lobe; both lungs were studded with tubercle, but the right especially. The right upper lobe was excavated into an irregular cavity, and the middle lobe and posterior portion of the lower lobe were solid; the anterior portion was crepitant, and free from tubercle; the upper lobe of the left lung was solid from tuberculous deposit, with intercurrent inflammatory exudation; the lower lobe also contained interspersed miliary tubercles, but was still, to a considerable extent, crepitant; the abdominal organs were healthy, except the liver, which was large and waxy-looking; and the kidneys, which were also large, and had their capsules adherent, but were not apparently otherwise diseased.

In this case it may fairly be inferred that the patient was phthisical before she took ague, but the disease had advanced rapidly during the time that she suffered from that disease, and its fatal termination had certainly been accelerated by the complication.

Case 2.—Ague occurring in a person labouring under incipient consumption; the former disease cured, the latter still progressing:

Mary Turner, aged 31, a married woman, residing in Gravel-lane, in the Borough, applied at St. Thomas's Hospital as an out-patient, on the 30th of September, 1858. She stated that she had been ailing for two years, suffering from cough, and that she had spat a little blood occasionally, and had lost much flesh. She had been more seriously ill for two months. She complained of cough and expectoration, and for the last two weeks had had aguish symptoms, consisting of rigors, which commenced at eleven in the forenoon every other day, followed after about an hour and a half by heat, and then by profuse sweating. She also had perspirations, which came on at nights. She had not, for several years, been out at service, and had never previously had ague. She was pale and malarious-looking; the pulse 100, and feeble; the bowels regular. On examining the chest, the resonance, on percussion, was found to be impaired at each apex, more especially on the left side, and bronchial respiration and loud cough resonance were there heard, and there was some rhonchus, with a forced inspiration. She was directed to take the cinchona and acid mixture, with 5 grains of the disulphate of quina, and half a drachm of the compound tincture of camphor, three times daily, and the hyoscyamus, and Dover's powder pills at night.

On the 17th of October she stated that she had had the shivering every other day, though less severely than before. She had gained strength, and her cough had been less troublesome.

On the 21st, she said she had been too ill to attend on the 14th. During the week in which she continued to take the medicine she had only one attack; but during the last week, being without medicine, she had two attacks, but they were less severe than before.

Case 3.—Ague occurring in the early stage of phthisis; the former disease cured, the latter still progressing:

James Tayler, aged 25, a painter, residing at Rochester, was admitted an out-patient of the Victoria Park Hospital on the 6th of August, 1858. He stated that he had been declining in health and subject to cough and expectoration, and occasionally of blood, for the last three years. About two years after the commencement of his illness, and one year before he applied at the hospital, he had an attack of ague, which disease he contracted while working at Cliff in the marshes between the Thames and Medway. The aguish attacks were decided, and recurred every other day for four or five times, and slightly for a longer period, but they were ultimately stopped by medicine. He has not had any symptoms of ague since, bnt his health had rapidly given way. When he applied at the hopsital he complained of a severe cough, which was attended by copious expectoration, especially in the morning; he was pale and thin. The tongue coated at each side, and the pulse quick. The whole of the left side of the chest expanded very imperfectly, and there was a distinct *bruit de pot-felé* heard on percussion beneath the left clavicle, with loud cavernous sounds and gurgling in that situation. He was directed to take the cinchona and acid mixture, with cod liver oil three times daily, and a soap and opium pill every night. He was last seen on the 16th of Dec., and the phthisical symptoms were then advancing.

Case 4.—Signs of commencing consumption and aguish symptoms coincident; result not known:

Hannah Jones, aged 52, a married woman, residing in Blackfriars Road, applied as an out-patient at St. Thomas's Hospital on the 6th of November, 1857. She stated that she had been ailing for nine months, suffering from cough and expectoration, and that four months before she had spat blood. She had a very malarious aspect and complained of suffering from chills, commencing about 10 o'clock every forenoon, and followed by heat, but without sweating. On examining the chest the resonance on percussion was found to be deficient at the apex of the left lung, but there were no other marked signs of tuberculous deposit. She was directed to take quinine, and was relieved from the aguish symptoms, and ceased attending on the 27th of November. Her subsequent history is not known.

In addition to the cases which I have now related, and two to be reported hereafter, I have met with three other instances of the coexistence of consumptive symptoms and ague. One of these was that of a lady residing in the Isle of Sheppy, who with slight evidences of consumption had decided ague, which was removed by treatment. Another was the case of a young girl from Rochester, an out-patient at St. Thomas's Hospital, who had advanced consumption and decided ague, and died shortly after she was brought to me. The third was an out-patient at the Victoria Park Hospital, residing in Stepney, a middle-aged man, who presented the usual signs and symptoms of phthisis in the second stage, and had irregular aguish symptoms, which were relieved by treatment. Of the subsequent history of three of these cases in which the pulmonary affection was in an early stage, I have no knowledge; but leaving them out of consideration, the others which I have reported are sufficient to show that phthisical patients may take ague, and that when that complication occurs the symptoms of consumption are greatly aggravated and its fatal result hastened.

It is, however, quite possible, that though consumption may not grant immunity from attacks of ague, yet the occurrence of ague may prevent the subsequent development of consumption. My attention has only recently been directed to this inquiry, and it is not a large number of phthisical patients from aguish districts which come under my notice, but I have at present two patients, who inform me that they have had attacks of ague before their phthisical symptoms occurred, and that they have continued to reside up to the present time in the districts in which they contracted the disease. The following cases, indeed, may be regarded as "crucial instances," in deciding the question of the antagonism of phthisis and ague, inasmuch as they not only afford examples of the coincidence of the two affections, but show that phthisis may develope itself in persons who have had previous attacks of ague, and who have continued constantly exposed to the malarious influence.

Case 5.—Ague occurring in the phthisical patient, who had previously had several attacks of ague. The former disease cured; the latter still in progress:

M. A. Heard, aged 30, a married woman, residing at Aveley, near Purfleet, in Essex, applied as an out-patient at St. Thomas's Hospital on the 23rd of September, 1858. She stated that she had been ill for five months, suffering from cough and expectoration, and had occasionally spat small quantities of blood; her voice had been for some time hoarse, and she had lost much flesh, and was pale and thin. She also suffered from attacks of ague, which came on every other day at 11 a.m., with shivering, after which she became hot and sweated profusely. She said that she had had an attack of ague when ten years old, and while residing in the neighbourhood of Rochford, which continued at intervals for fifteen months, and she had since had two or three other attacks, and she had resided all the time in different parts of Essex bordering on the marshes. On examining her chest the resonance on percussion was found to be impaired, and there was loud cough resonance at each apex. She had been taking quinine and cod-liver oil, and the same remedies were continued; on the 21st of October the aguish attacks had entirely ceased, and she was otherwise improved. Since this time she has been admitted into the Victoria Park Hospital for Diseases of the Chest. She has had some recurrence of the aguish symptoms in the form of chills, coming on at a regular period. There is marked dulness on percussion with bronchial respiration, and undue cough resonance at the right apex, and the resonance on percussion is impaired, and the respiration harsh, and the resonance with the cough increased at the left apex.

Case 6.—Phthisis and ague in a person who had previously had ague. The latter disease relieved, the former progressing:

William Cox, aged 46, a labourer who had resided at Woolwich for the last twelve years, and had had ague six years before, applied as an out-patient at St. Thomas's Hospital in August last, and was admitted into Jacob's Ward, under my care, on the 2nd of September, 1858. He stated that he had spat blood seven or eight years before, but had only been seriously ill for a year, and had been suffering from symptoms of irregular ague for nine weeks. When admitted, he complained of difficulty of breathing, cough, and expectoration; he was emaciated; the pulse was weak, and he had a very malarious aspect. Every other morning he became very chilly, and had violent retching, followed by heat and sweating, but he had no distinct rigors; on examining his chest the resonance on percussion was found to be generally deficient at the upper parts, where also the cough resonance was very loud, and sonorous and mucous rhonchus were heard in all parts of the chest, and were especially marked in those situations. He was directed to take three grains of quinine and ʒj of compound tincture of camphor in ℥iss of the cinchona and acid mixture three times daily, and wine and a nourishing diet were allowed. On the 11th a blister was applied between the scapulæ, and an ipecacuan and squill pill was ordered to be given at night. On the 15th he was much better, the aguish symptoms had entirely disappeared, and he looked much less malarious, but he still had a severe cough and copious mucopurulent expectoration. There was some deficiency of the resonance on percussion at each apex, with falling in of the parietes there, and bronchial respiration and loud cough resonance, and these signs were the most marked on the right side. There was also some subcrepitant rhonchus heard low down in each dorsal region, but the respiration was generally free. The cod-liver oil was directed, in doses of two drachms, with the tonic and cough mixture, to be taken twice daily.

On the 22nd he was not improving; he continued to lose flesh, and complained that he had taken fresh cold, and wished to be made an out-patient; this was done, but he has not since returned to the hospital.

The facts which I have now detailed, conclusively show that neither does the existence of consumption prevent the occurrence of ague, nor the occurrence of ague preclude the subsequent development of consumption; but they do not prove that the supposed antagonism may not, in some degree, exist. For, on the one hand, phthisical patients, subjected to the influence of malaria, may take ague in less proportion than healthy persons similarly exposed; and on the other hand, persons who have had ague may be less liable to consumption than those who have not had the disease. These objections I have no means of meeting, but when it is considered that, within a period of two years, five cases of the co-existence of the two diseases have fallen under my own notice, at one public institution, where the majority of phthisical patients do not come from malarious districts, the coincidence cannot be regarded as exceptional or rare.* I cannot, therefore, but conclude, that it is not probable any material antagonism exists between phthisis and intermittent fever. The facts do not, however, warrant the denial of the supposition altogether, and there are probably few popular ideas which have not some foundation in truth.

The correct inference would appear to be that the influence, if any, exerted on the prevalence of consumption, by a malarious atmosphere or by ague, is far less important than that of various other causes which affect the development of that disease. The practical conclusion also to be arrived at is, that as phthisical patients may take ague, and as such complication materially aggravates the original disease, we should, in selecting residences for consumptive patients, avoid those situations which are marshy, or in which aguish affections are known to prevail.

Art. III.

An Experimental Inquiry on Endocarditis, by the Synthetical Method. By Benjamin W. Richardson, M.D., L.R.C.P., Physician to the Royal Infirmary for Diseases of the Chest, and Lecturer on Physiology at the Grosvenor Place School of Medicine.

As the eye of the reader falls on the first lines of this paper, I beg him to receive the assurance that the paper is not written to represent any special theory in Pathology; but to record certain experimental results, and to indicate a new way to the discovery of facts as yet hidden in the phenomena of disease. Farther, I would pray that any reasonings which may be offered on the experimental evidence may be considered as apart from the evidence. For the evidence belongs to the present—*it is;* the argument belongs to the potential—*it may be.*

The method of studying disease which this paper is intended to illustrate, belongs exclusively to modern science, in so far as medicine is concerned. It has as yet been confined to the labours of a few enquirers, and has received, I believe, no systematic exposition as a general plan, from any one in medicine, except myself. The method consists in inducing, by experimental means, the phenomena of special diseases. The method may be called the "synthetical method" of studying disease.

It is remarkable as an historical fact, that no science, save medicine, has been content to rest on analysis as its only foundation; yet is there no fact more obvious. If the question be asked, Why is not the knowledge of disease positive in its character like other scientific knowledge?—there is a ready answer; that, as compared with other sciences, the science of medicine proper, has always stood on one leg, while the other sciences have walked on two; I mean on analysis and synthesis.

As a philosophical process, I would accord to analysis the greatest importance; it gives finality to fact; but in the first stages of an enquiry into a series of unknown phenomena, I look on synthesis as the co-equal with analysis, in the progress of discovery. Synthesis suggests and produces facts, which at once suggest and produce new modes of analysis, and which, later in the day, support analytical results. Synthesis is essentially inventive as a process, and if it never leads to pure demonstration, it sometimes supplies evidence so closely bordering on the demonstrative, that analysis has little left to perform in the way of completion. All disease is natural synthesis.

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* During the two years in which these five cases occurred, I treated at St. Thomas's Hospital, among the in and out patients, 236 cases of ague, and at least 262 cases of phthisis. The precise number of cases of the latter disease I cannot give, as some cases of phthisis are entered in the out-patients' book as "affections of the chest." I have also met with several other cases in which there was reason to suspect tendency to phthisis in aguish patients.

I have shown, in another place, to what a grand extent the synthetical process might be applied in the study of various classes of diseases.* I shall not open this abstract question now, for it were out of place such proceeding; but I beg the reader while observing the second and immediate intention of this paper, to receive the paper altogether as a representation of a general scheme of research.

The special subject of this paper is, the disease Endocarditis and its artificial production. Let us observe first, the simple facts of an induced endocarditis.

It is the fact then, that endocarditis can be experimentally produced in an inferior animal: the fact I shall endeavour to demonstrate further on.

The mode of producing this condition consists in the introduction into the circulation of an acid.

The only acid capable of producing this effect, so far as present researches teach, is the lactic acid.

The acid is best introduced by the peritoneum into the bodies of the animals subjected to experiment. This mode of introduction has the following advantages:—

1. The operation is easy to perform and almost painless.
2. The acid solution, if the animal is deprived of fluids for some hours before and after the operation, is absorbed readily from the peritoneum.
3. The fluid being taken into circulation, with the acid largely diluted, the effects produced are comparatively slow, and are so defined, that the symptoms set up can be watched from day to day.
4. The operation is attended with no immediate danger as an operation, and it can be repeated many times on the same subject.

In my experience, extending now to sixteen experiments, the production of endocarditis has failed in no one instance.

The effect of the acid in producing endocardial inflammation is well marked in carnivorous animals, as dogs and cats, and I have recently tried it on a rabbit with the same result.

As I have already published† the details of the first experiments for the production of endocarditis, I need not repeat those details here: it will be better to give the summary of the results of all the experiments.

When an ounce of solution of lactic acid, containing ten per cent. of the acid itself, is introduced into the peritoneal cavity of an animal, I refer more especially to the dog, no symptom is peculiarly manifested for the first six or eight hours, except one, viz., rapid action of the heart. True, the animal, if he has recently been fed, may vomit immediately after the operation, but this may be avoided by suppressing food and drink for a few hours beforehand.

After six hours, symptoms of a general kind occur; the acid having ordinarily disappeared from the peritoneum long before that period; the animal is now feverish, the skin is hot, and there is thirst; he has occasional shiverings, slight in degree, and he seems languid and cross. If the muscles are grasped, there is often evident and angry expression of pain.

The prominent signs of heart affection usually appear about twelve hours after the injection; the breathing is hurried; there is also a short dry cough, and decided uneasiness in the chest; the action of the heart continues to be rapid, and is now irregular and bounding. If the animal is at this point of the experiment freely purged, or if he passes a large quantity of urine, the symptoms all subside, and renewal of the injection is necessary, in order to sustain the effect. The effects continuing, either from the first, or after the repetition of the operation, the sounds of the heart become modified, and the modifications run as follow:

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* On the Investigation of Epidemics by Experiment. Edinburgh Veterinary Review, vol. i. No. II., 1858.

† Essay on the Coagulation of the Blood. Appendix. London, 1848.

1st. The abnormal condition is confined in the primary stage to the *right* side; gradually it extends to the left, but the right catches it first; a result which, from the conformation of the chest, is easily traced out in the dog.

The kind of modification of sounds is invariably the same; it consists in a gradual disappearance of the first sound, and in the production of a peculiar sharp or accented second sound. I have observed this state extending over twenty-four hours, and, in one instance, over two days, during the whole of which period, there could only be distinguished one sound, and that the second. In time, however, this peculiarity passes away, and in place of a first normal sound, a gentle and musical purr obtains, which ultimately becomes a loud and peculiarly well-marked systolic bruit. This sound established, it is comparatively permanent. If the animal does not die from the acuteness of the attack, the murmur remains for weeks. In one instance, it was well marked in a dog for five weeks, the animal in the meantime having recovered his health, as it seemed, completely.

In one instance, these endocardial symptoms were accompanied with well-marked pain in the joints and limbs; this pain crippled the animal, and it was curious to observe that the pain was rarely for a day at a time in the same limb. Sometimes the animal limped with one hinder limb; in a few hours he would be using that limb freely, but would not trust a fore limb to the ground, and so on, but this case was exceptional. The joints in this instance were found normal after death, but the death was not produced until many weeks after the acute symptoms had passed away.

Such are the symptoms produced by the use of the acid poison.. I would now beg special attention to the pathology which attends these symptoms in their several stages. For the sake of connecting pathology with symptom, I would divide the symptom, keeping purely to those which are endocardial, into four stages.

(*a.*) The stage when the action of the heart is simply excited, the sounds being normal.

(*b.*) The stage when the first sound is lost, and the second accented.

(*c.*) The stage when the first sound is replaced by the faint purr.

(*d.*) The stage when there is marked and sustained systolic bruit.

If the animal be killed, or if it die during the first of these stages (*a*), the valves, whether auriculo-ventricular or semilunar, are free and not thickened, but the endocardial surface throughout is of a brilliant vermilion colour. Even the pale margins or flaps of the valves have a pinkish hue; there is also over the membrane throughout, a villous or velvety appearance, and beads are abundant. The pathological signs of this stage often come on in an extraordinarily brief period after the injection; as soon, in fact, as the injected fluid finds its way through the heart, the injection of the endocardial surface is developed. In the first well-marked illustration of this stage, the pathological indications were brought out in a period of six hours. But I have since performed three experiments, in one of which the fluid injected into the peritoneum was absorbed within an hour, and the injection of the endocardial surface was well pourtrayed. In the other experiments, one on a rabbit, the other on a cat, the same events occurred in two hours.

In the second stage (*b*), when the first sound is lost, the mischief, on whichever side it may be, is confined to the auriculo-ventricular valves. The whole structure of the valve is thickened and 'œdematous.' I have seen the segments of the tricuspid valve fixed in this swollen condition, resembling each an injected uvula, and lying so close to each other, that when the heart was contracting, they must have cushioned against each other, thus fulfilling their office of preventing regurgitation, passively, i. e., without tension or movement. In this œdematous stage, if the valve be pricked with a needle, a clear white lymphy fluid exudes, and by frequent pricking, the valve structure emptied of its effusion, collapses and assumes a flaccid condition.

In the third stage (*c*), the valves remained thickened, but the red colour and œdematous state were both reduced. Beneath the endocardial surface of the valve, there is a paleness as from coagulated effused lymph. If the needle is applied now, there is no exudation; the valve has some limited play, unless it is bound down by adhesion, and its structure is firm. Beads which generally fringe the margin of the valves all round, from being œdematous prominences in the earlier stages, are pearly looking and are moderately firm.

In the fourth stage (*d*), when the systolic murmur is purely marked, the valves have shrunken, and have regained imperfect play, but are still thickened and unyielding. Beads which may be present are extraordinarily firm, their presence adding not a little to the loudness and musical character of the systolic murmur.

I have confined my description so far to the changes which are produced in the tricuspid or mitral valves. The disease, in a word, is mainly confined to one or both of these structures. A very slight thickening, not sufficient at any time to interfere seriously with their duties, is all I have ever observed in the semilunar valves of either side.

The simple facts connected with the production of artificial endocarditis having been thus demonstrated, we may turn with advantage to the consideration of certain questions suggested by the facts themselves—questions which all may reason upon from the facts, and by the discussion of which the ultimate practical meaning of the experimental results may be elicited.

The experiments have a bearing of great interest in relation to the physiology of the sounds of the heart.—In every instance where the first sound was lost, and inspection of the heart was made during that stage, four instances, the auriculo-ventricular valves were so modified that their tension was clearly impossible.

In every instance where the systolic murmur was established, the same valves were implicated, but in a different way, i.e., they were found restored partly to their action, but indurated in structure.

In every case throughout the whole series the second sound was always distinct; in no case, at any stage, were the semilunar valves so implicated as to lead to the inference that their action was impaired.

Better than any physiological physical inquiry, these results prove to my mind that the systolic sound is due to tension of the auriculor ventricular valves, and the second sound to tension of the semilunars. What can be more conclusive? Two animals are well—they have two sounds to their hearts, and two sets of valves in alternate play; the animals are made ill by experiment, and the first of these sounds is entirely lost; you listen to the heart for hours, and there is the one solitary tick; it is listening to a clock; you destroy, and cut down to the heart of one of these animals in this stage, and you find the ventricular valves inactive; you let the second animal have longer respite, and the first and lost sound returns, but modified as a murmur. You kill and cut down to the heart of the second animal, and you find the ventricular valve in a position to act, it is true, but devoid of flexibility, incapable of tension, and as it were fitted up for the production of murmur by the blood stream.

The production of symptoms so specific by the simple act of introducing an acid into the circulation is another point of interest.—In commenting on this fact of production, I have carefully avoided on all occasions from connecting it too closely with any theory of disease occurring spontaneously in the human subject. That the fact is full of meaning in relation to the disease, endocarditis, in the human subject, is undeniable; and I feel safe (while faintly tracing out such relationship), in offering the following propositions:

1. That lactic acid present in the blood, will produce endocarditis.
2. That lactic acid could not exist in the blood without producing endocarditic mischief.

A question has been asked me many times, viz., whether other acids than lactic introduced into the circulation in the same way would not produce the same effects? From the composition and from the influence exerted on blood by certain other acids, viz., acetic, formic, and citric, I inferred that a similar influence on the heart to that produced by the lactic was at least probable; and I shall notice presently something positive on this point. But before so doing, I would impress strongly the argument that, although half a dozen analogous substances having the properties of acids, should produce the analogous symptoms and pathology when introduced into the body (by virtue of the same law as makes them exert analogous chemical results out of the body), such occurrence would not, as has been superficially assumed, reduce the value of the experimental evidence already collected in its bearings on disease.

Let the reasoner, for the moment, forget all old analytical speculations regarding the origin of diseases in which endocarditis is a symptom. Let him then take up as a new and simple proposition, any day proveable by experiment, that endocarditis may be artificially produced by introduction into the body of one of a limited series of analogous compounds. Let him after this return to his analytical mode of research, and if in that manner he should find present in the human body, in a case of endocarditis which did not produce disease, one of the very agents by the use of which he had been enabled to elicit the same disease by synthesis, then I contend that such observer has arrived as near to a specific cause as he can arrive by any experiment whatever.

The connexion then of an acid poison in the body, with endocarditis as a result, in cases of rheumatic endocarditis, seems to stand as follows:

Synthesis entirely and as completely as synthesis is capable, supports the connexion. The synthesis is positive and demonstrative.

The analysis is corroborative, but has not yet advanced beyond theory. In acute rheumatism analysis has only found an acid poison of the producing series in the excreta. It is absurd certainly to assume that ounces of an acid of the producing series, thrown off from the skin of a sick man, should not be derived from his blood; and I shall be able to show that one reason of such acid poison not having been found in the blood of the rheumatic man is, that it has never been sought after in the right place. But I will not wait to support theory; it is most sound to state the pure fact that analytical demonstration of a producing poison in the blood is not yet supplied.

But what of the other acids; on this point I have one valuable experiment. As acetic is nearest in its approach to lactic acid, I have performed with every possible care the experiment of introducing this acid into the circulation. The acid so introduced, produces many of the general symptoms caused by the lactic, and some disturbance of the heart. But the specific cardiac symptoms are wanting.

Returning to the simple teachings of the experiments, there are certain local peculiarities of great importance to which allusion must be made.

It is remarkable, that when the inner surface of the heart is affected by the acid poison, the mischief is mainly confined to the auriculo-ventricular valves. The semilunar valves escape as a general rule. In like manner, the lining coats of the blood-vessels do not seem to suffer any more than in rheumatic endocarditis. From this, one can but infer, that some certain histological peculiarity, as yet not understood, acts in unison with the peculiar poison to produce the lesion of the ventricular valves; and that some certain histological peculiarity, not as yet understood, tends to the protection of the aortic valves and lining coat of the vessels. This observation, small enough indeed in its individual meaning, assumes a much wider form, when it is considered with the local origin of diseases generally. It bases in fact this proposition, long previously theoretically conceived and admitted, but not so definitely proved. *That a special poison, present in the blood and bathing all tissues, may have its influence, as a disease-producing agent, localized in one structure or organ.*

In the course of my description of the symptoms and morbid anatomy in the cases of endocarditis induced by experiment, I dwelt specially on the circumstance, that in these instances the disease was primarily manifested on the right side of the heart. At first sight, this occurrence is an anomaly, when compared with ordinary endocarditis, and hastily glanced at, would serve to break the idea of relationship between induced and spontaneous endocardial inflammation. But a little reflection dispels these hasty inferences, and offers not merely a proof of relationship, but an explanation absolutely of the known fact, that in spontaneous endocarditis, the left side of the heart is the common seat of the disorder.

The cause of the difference in the two classes of cases, I mean the induced and the spontaneous, seems then to be simply this. In the cases of induced endocarditis, the poison introduced into the body by an absorbing surface finds its way into the circulation by the venous blood. It follows, that as the poison traverses the circulatory canals, it comes in contact with the inner surface of the right side of the heart first: in the pulmonic circuit it undergoes some loss, and so entering the left cavity is less active in its effects. In other words, the poison in these instances, in so far as the heart is concerned, is derived from the systemic circuit, and is lost in part in the pulmonic circuit.

On the other hand, in rheumatic endocarditis the evidence all points to the supposition that the poison is a product of respiration. Hence, as the poison traverses the circulatory canals, it comes in contact, first, with the inner surface of the left side of the heart: while, in the systemic circuit, it undergoes loss or combination, so that the blood returning by the veins is not poisoned, and the right side of the heart escapes.

Reversing the previous proposition, the poison in these cases, in so far as the heart is concerned, has a pulmonic origin, and a systemic destruction.

The further inference from this argument also is, that the action of the producing poison, both in the artificial and the spontaneous endocarditis, is directly on the part affected; i.e., by contact with the endocardial surface. Let us examine this question from another point of view.

Seeing that a certain series of changes are produced in the endocardial membrane when the necessary condition, viz., a producing poison, is present: our reason admits of but two modes by which the changes induced could originate. Either the poison has been carried into the affected part through the nutritive vessel or vessels of the part, and thus has produced its effect, *a tergo*, by interference with nutrition; or it has been applied to the free surface of the part, and has produced its effect by direct contact, like a blister applied to the skin. Many poisons have the privilege of producing their effects by both methods; but in reference to lactic acid and endocarditis, and in reference to an assumed rheumatic poison and endocarditis, there is no alternative but to accept that the action of the poison is by direct contact with the free surface.

For the position of the question is this: that in the artificial endocarditis the right side of the heart is first affected; in the rheumatic endocarditis the left side is primarily, and by a general rule which has but rare exceptions, singly affected. Now, it is clear that, if the effect of the poison in these cases, one or other, were *a tergo*, i.e., by introduction to the endocardial surface through the nutritive vessel, the two sides of the heart would share equally in the catastrophe, inasmuch as they are both fed from a common source and the same blood. But if the action of the poison is by direct contact of the poison with the free surface of the membrane, the occurrence of endocarditis in the induced cases on the right side, and in the spontaneous cases on the left side, is easily and satisfactorily accounted for.

The observations here made lead me to recur for a moment to a remark already offered relative to the methods which have been pursued by those chemists who have searched for an acid condition of blood in rheumatic cases. As far as I can discover, every inquiry in this direction has been made on venous blood. But it is obvious, if the foregoing arguments are correct, that the venous blood is never charged with the free poison. The poison being disposed of in the systemic circuit by elimination and combination can only be present in *arterial* blood, whither the chemist should turn for evidence.

The experiments which I have had the good fortune to institute, for the production of endocardial inflammation, have enabled me in a measure to trace out some of the changes which result in the foundation of organic cardiac disease. The reader will, I trust, pardon me while I briefly offer a note on this subject.

In artificial endocarditis the first morbid sign is simple vascularity of surface; the second, effusion of lymph into the subserous tissue; the third, coagulation of the effused fluid; and the fourth, either absorption of the effused matter, or the nutrition of this matter and its transformation into new and abnormal structure. Thus the ordinary sequences of the inflammatory process are the same in endocarditis as in other inflammatory types.

On the pathology of ordinary endocarditis much discussion has at various times arisen respecting the origin of the bead formations so often met with, after fatal cases, fringing the margins of the valves. Some have argued that these beads are the result of exudation, others that they are simple deposits from the blood. Some years ago I was inclined to the deposition theory. I take the present opportunity, from positive knowledge, of retracting this view. Tracing the formation of the bead step by step in these later inquiries, I have demonstrative proof that the bead originates in an exudation beneath the endocardial surface.

But, although the bead has this unquestionable origin, it is, I believe, often the basis of a secondary deposit from the blood. I have explained that in the œdematous stage of endocardial inflammation, the swollen surface gives out on puncture or slight pressure a plastic fluid. In endocardial inflammation the exuded matter sometimes finds its way to the free surface; and once exuded in this manner, it may, and probably does, form a favouring point for the direct deposition of fibrine from the blood. The advocates of the exudation theory and those of the deposition theory may therefore, in the main, be both right; but the former have the priority in the argument.

The last point on which I would rest, relates to the connexions which may be presumed to exist between a free acid in the blood and the fibrine of the blood. As the blood is always alkaline in health, as the solution of fibrine is clearly due to the presence of alkali, and as the fibrinous deposition is common in acute rheumatism, the theory has been most ingeniously and ably propounded by Dr. Fuller, that the deposition in rheumatic fever is due to neutralization of the alkaline solvent of the blood. Consonant with this view, I had expected to find that in my experiments of introducing lactic acid into the blood, I should ensure free fibrinous deposition. Remarkably enough, in every instance, the reverse obtained; the blood in these cases was only feebly coagulated and rarely showed sign of free separation of fibrine. The explanation of this apparent contradiction could only be sought for by experiment with the acid, on blood itself. So going to the experiment, I found at once, that lactic acid, although it will but feebly dissolve condensed fibrine, has the same effect as a fixed alkali when added to blood newly drawn. The same power of holding blood fluid is possessed by the acetic and citric acids.

We cannot, therefore, in observing the tendency to fibrinous deposition, retain the impression that the tendency to fibrinous deposition in rheumatism is due to neutralization of the true blood solvent, seeing that the assumed neutralizing agent is itself a fixed solvent of the blood; but we must take, rather, the two occurrences of an acid poison and an increase of fibrine as connected with a common cause and as coincidental phenomena.

In a few concluding sentences, I will put forward what seems nearest to the truth in reference to spontaneous rheumatic endocarditis and its cause.

1. The evidence, both analytical and synthetical, goes to prove that the disease is due to the presence of an acid poison having the properties of lactic acid but the evidence stops short, as yet, of absolute demonstration in the analytical part.

2. The synthetical evidence brings to the verge of demonstration the supposition that the action of the poison which produced the disease is direct on the free surface of the endocardial membrane; that the poison acts, in a word, after the manner of a local irritant.

3. The evidence, analytical as well as synthetical, points to the conclusion, that the

producing poison of spontaneous endocarditis is formed in the pulmonic circuit, and is destroyed in the systemic.

4. The results of endocardial inflammation are the same in order as in inflammation of other structures; and that the bead formation is due to exudation.

5. A poison circulating in the blood and bathing all the tissues, may elect one tissue for its specific manifestation of effects as a poison.

6. While the presence of a specific acid poison in the blood will account for the specific disorder endocarditis, the other and attendant events in the rheumatic attack are to be sought for in a more general originating cause. Further back, toward such origin, I dare not attempt to penetrate on this occasion. In such direction, new and laborious experimental research is required, bearing on the one fundamental question, the oxygenation of animal bodies in health and disease.

PART FOURTH.

Chronicle of Medical Science.

HALF-YEARLY REPORT ON PHYSIOLOGY.

By Hermann Weber, M.D.

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I. General Physiology.

1. Von Bezold: *On the Distribution of Water, Organic Substance, and Salts in the Animal Kingdom.* (Verhandl. d. Med. Ges. zu Würzburg, vol. viii., p. 251; and Canstatt's Jahresbericht der Physiol. Wissenschaften. in Jahr. 1857, p. 139.)
2. Scherer: *On the Proportion of Water and Mineral Substances in the whole Organism.* (Verhandl. d. Würzburg, Gesellschaft., vol. viii., p. 266.)
3. Bauer: *On the Per-centage of Water in the Organism.* (Dissert. Inaugur., Würzburg, 1856; and Canstatt, l. c., p. 141.)
4. Schlossberger: *Contributions to the Chemical Knowledge of Fœtal Life.* Second Article. (Liebig's Annalen, vol. ciii., p. 193; and Canstatt, l. c., p. 141.)

Scherer's and Von Bezold's analyses, made in the laboratory of the former, lead to the following inferences:—1. Every animal possesses a certain proportion of water, of organic and of inorganic substances, which proportion depends on the age of the animal and on the class to which it belongs. Thus, the average for adult mammalia is—water, from 68 to 71 per cent., organic substance from 24 to 28 per cent., inorganic substance, from 3 to 5 per cent. Birds contain less water, slightly more organic, and considerably more inorganic, substance than mammalia. In the class of reptiles, the per-centage of water is still smaller, that of organic substance slightly, but that of inorganic substance greatly, augmented; while the batrachians, compared with the mammalia, exhibit a larger amount of water, a slightly smaller proportion of inorganic, but a much smaller proportion of organic, constituents. 2. The analogy of the anatomical constitution of the body of different species of animals implies analogy in the chemical composition with regard to the three series of substances (water, organic and inorganic substances.) 3. The development of every animal is connected with certain changes in the proportion of the three series of substances. These changes appear to be analogous in every one of the three first classes of vertebrate animals, and consist—*a*, in the decrease of water from the fœtal state to that of complete development (from 87 per cent. to 71 per cent.), which decrease is most rapid during the first eight days after birth; *b*, in the increase of the solid organic constituents (from 11 per cent. to 25 per cent.), which increase is greatest in the first eight days after birth; *c*, in steady increase of the inorganic solid constituents, which increase is slowest during the first period of life.

Similar researches have been made by Bauer, likewise under Scherer's superintendence. Bauer gives the results of the analysis of four mice. The amount of water varied between 68·012 and 71·654 per cent., that of solid organic substance between 24·35 and 28·49 per cent., that of solid inorganic substance between 3·36 and 3·99 per cent.

Schlossberger communicates a series of similar examinations, exhibiting the proportion of water, solid constituents, salt, and fat contained in the fœtus, and in various of its organs. The fœtuses analysed belonged to the bovine species. The proportion of water of these fœtuses, between three and eight weeks old, varied from 91·77 to 92·76 per cent., that of fat from 0·53 to 0·60 per cent., that of ashes from 1·07 to 1·27 per cent., that of organic tissue-forming substance from 6·27 to 6·43 per cent. Of the single organs, the blood contains least water, 81·90 to 82·28 per cent.; then follow the liver, the spleen, the thymus, organs which appear very active in the fœtus, and contain a large amount of blood; the greatest quantity of water is found in the brain and lungs, viz., 89·24 to 92·59 per cent.

II. Food and Digestion.

1. Denis: *New Researches, Chemical, Physiological, and Medical, on the Albuminous Substances* (Paris, 1856; and Canstatt, l. c., p. 149.)
2. Fresenius: *Chemical Examination of the most Important Kinds of Fruit.* (Liebig's Annalen, vol. ci., p. 219; and Canstatt, l. c., p. 137.)
3. Rinse Cnoop Koopmans: *Contribution to the Knowledge of the Digestion of Vegetable Albuminous Substances.* (Moleschott's Untersuch., vol. ii., p. 158; and Donder's Onderzockingen, p. 71.)
4. Weber, G.: *Nonnulla de Digestibilitate Carnis.* (Gryphiæ, 1857; and Canstatt, l. c., p. 63.)
5. Busch: *Contribution to the Physiology of the Digestive Organs.* (Virchow's Archiv, vol. xiv., p. 140, 1858.)
6. Valentin: *Contribution to the Knowledge of the Hybernation of Marmots.* (Moleschott's Untersuchungen, vol. iii., p. 195.)
7. Heidenhain: *On the Absorption of Fat,* see Sub. iv.

Denis admits only five representatives of the albuminous substances in the vegetable and animal organism—viz., in plants, *glutin* (gluten,) in animals, *albumin* (of eggs), and *serin* (albumin of serum), *casein*, *fibrin*, and *globulin*. All these substances, the author assumes, are insoluble in their pure state; wherever they are found dissolved, they owe this condition to a combination with acid, alkaline, or neutral salts. The coagulation does probably not cause any alteration in the chemical composition. Denis never met in plants with substances analogous to the albumin, fibrin, or casein of animals; the common gluten and the vegetable fibrin, are, according to him, only modified glutin, the vegetable albumin only a combination of glutin with salts, and the vegetable casein, the amandin (emulsin), and legumin, only products of the metamorphosis of this substance.

Fresenius communicates the results of a series of examinations of various kinds of fruit. We can extract only the figures belonging to some of the most common species of fruit.

I. *According to the per-centage of Sugar.*

	per cent.		per cent.
Peaches	1·57	Gooseberries	7·15
Apricots	1·80	Pears	7·45
Plums	2.12	Apples	8·37
Greengages	3·12	Mulberries	9·19
Raspberries	4·00	Cherries (sweet)	10·79
Strawberries	5·73	Grapes	14·93
Currants	6·10		

II. *According to the per-centage of Free Acid (expressed as the Hydrate of Malic Acid.)*

	per cent.
Pears	0·07
Cherries (sweet)	0·62
Peaches	0·67
Grapes	0·74
Apples	0·75
Greengages	0·91
Apricots	1·09
Plums	1·30
Strawberries	1·31
Gooseberries	1·45
Raspberries	1·48
Mulberries	1·86
Currants	2·04

III. *Showing the proportion between the Acid, the Sugar, and the Protein and Gum, &c.*

	Acid.	Sugar.	Protein, gum, &c.
Plums	1	1·63	3·14
Apricots	1	1·65	6·35
Peaches	1	2·34	11·94
Raspberries	1	2·70	0·96
Currants (red)	1	3·00	0·07
Greengages	1	3·43	11·83
Strawberries	1	4·37	0·08
Gooseberries	1	4·93	0·76
Mulberries	1	4·94	1·10
Apples	1	11·16	5·60
Cherries (sweet)	1	17·29	2·76
Grapes	1	20·18	2·03
Pears	1	94·60	44·40

IV. *Showing the proportion between Water, Soluble Solid Substances, and Insoluble Solid Substanees.*

	Water.	Soluble Substances.	Insoluble Substances.
Raspberries	100	9·12	6·88
Strawberries	100	9·39	5·15
Plums	100	9·74	0·87
Currants (red)	100	11·00	6·62
Gooseberries	100	12·18	3·57
Apricots	100	13·31	2·07
Pears	100	14·25	5·54
Peaches	100	14·64	2·10
Mulberries	100	16·57	1·47
Apples	100	16·89	3·16
Greengages	100	18·52	1·22
Cherries (sweet)	100	18·61	1·53
Grapes	100	22·81	5 81

The proportion of albumen is in all fruits so small, that we refer for the figures relating to it to the original, as also for the table showing the per-centage of ashes. In comparing the proportion of acids, sugar, protein and gum, water, &c., it must always be borne in mind that Fresenius probably examined German fruit, and that the proportion of the various constituents varies greatly in different years, even in the same country.

Rinse Cnoop Koopmans' observations and experiments on the digestion of vegetable albuminous substances lead the author to the inference that gluten is not completely dissolved by diluted acid, but that the addition of pepsin to the acid produces a perfect solution, which is effected likewise by acidulated artificial gastric juice. With regard to the formation of peptones from vegetable albuminous substances, the author is of opinion that gluten is, by means of the gastric digestion, transformed in a gluten-peptone. Comparative experiments made with gluten and boiled albumen show that the acid best suited for the digestion of the former is much more diluted ($\frac{1}{2000}$th to $\frac{1}{400}$th) than that for the digestion of the latter ($\frac{1}{275}$th to $\frac{1}{60}$th).*

G. Weber performed, under Budge's guidance, experiments on the digestibility of various kinds of meat and fish, partly on dogs, partly by means of an apparatus for artificial digestion. The authors observed the breaking-down of the muscular fibres in the transverse direction as in general preceding that in the longitudinal direction. Raw meat was more slowly digested than cooked meat. The difference between meat boiled tender and baked was small; that of young animals appeared in general more digestible. The less rich fishes, as pike, plaice, sole, &c., are more quickly digested than the richer kinds, as salmon.

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* For a further account of Dr. Koopmans' researches, see British and Foreign Medico-Chirurgical Review, vol. xx.

Busch furnishes valuable information on the physiology of the digestion and nutrition in general, derived from observations and experiments made on a female patient, aged thirty-one, in whom, through external violence, the intestinal tract was divided into two separate portions—the superior consisting of the stomach, duodenum, and a small part of the jejunum—the inferior of the larger part of the jejunum and the whole of the ileum and colon. The author had therefore the rare opportunity of observing the phenomena of a fistula of a jejunum in an otherwise healthy human subject. The accident had taken place six weeks before the admission of the patient into the University Hospital at Bonn; the wound in the abdominal walls was healed, with the exception of the fistulous opening. The woman had a voracious appetite, and devoured large quantities of food; the greatest part of this, mixed with the gastric, pancreatic, and intestinal secretions and bile, passed away from the fistulous opening in the jejunum; no particle of it entered the contracted and too distant aperture of the inferior portion of the intestinal tube. Extreme emaciation, loss of strength, tendency to sleep, and hoarseness, were amongst the principal constitutional phenomena.

The plan pursued by Busch, in order to restore the strength of the patient, consisted in the introduction of strong broths with eggs beaten up in them, occasionally also of pieces of boiled eggs and meat, into the inferior portion of the digestive tube. The strength of the patient increased rapidly under this treatment, and she gained also considerably in weight. This fact is of great interest, as it corroborates the view, that nutrition can take place—for some time, at all events—without the admixture of the gastric, pancreatic, duodenal, and hepatic secretions to the food. One might feel inclined to draw the further inference, that a complete fistula situated in the upper part of the small intestines renders the nutrition of the body through the stomach impossible; but this inference is made improbable by the circumstance that the ingestion of food through the mouth alone was sufficient to keep up and increase the strength of the patient after the recovery had reached a certain stage under the plan above described.

Although our space is limited, we cannot refrain from giving some of the principal observations made by the author, referring for the details to the essay itself. 1. There are two distinct sensations of hunger; the one is caused by the nervous system in general perceiving the want of fresh supply for the exhausted tissues; the other is produced by the nerves of the digestive organs alone. The former of these may continue, even when the digestive organs are filled with food. 2. The peristaltic motion of the intestines is not continuous, but shows periods of rest and of increased action; there is, however, no regularity in the alternation of rest and action. The force of the peristaltic motion could not be ascertained with precision, but it was sufficient to overcome a column of water two feet high. In the lower portion of the digestive tube, distinct antiperistaltic motion was frequently manifested. 3. The quantity of the intestinal juice (*succus entericus*) is very small and constantly alkaline. It will be remembered that this observation is in opposition to that of Frerichs, but in corroboration of Bidder and Schmidt's results. The per-centage of solids varied between 3·87 and 7·4 per cent., which is considerably greater than that stated by the authors just mentioned. 4. There can be no doubt that the intestinal juice exercises a digestive influence on proteinaceous substances. This process, however, is accompanied by that of putrefaction. 5. The intestinal juice transforms starch into grape-sugar. 6. It does not transform cane-sugar into grape-sugar. 7. The cane-sugar, absorbed as such, does not reappear in the urine. 8. Without the influence of bile or pancreatic juice, there is either no absorption of fat or at all events it takes place only to a very limited amount. 9. The reaction of the mixture of digestive secretions passing from the upper portion of the tube during the state of fasting was almost always neutral, only in rare instances slightly acid or alkaline. 10. This mixture of fluids did never exhibit the reaction of saliva. We may therefore conclude that the saliva is absorbed before the mixture reaches the jejunum. 11. The average per-centage contained in these fluids was 2·48. 12. The first portions of the food introduced into the stomach appeared in the jejunum from between fifteen to thirty minutes after the commencement of eating. 13. Solutions of cane-sugar disappear to the greater part already in the beginning of the digestive canal; what reaches the jejunum is transformed into grape-sugar. 14. Raw albumen of eggs is likewise partly absorbed already in the stomach and adjacent part of the digestive tube; the portion which enters the jejunum is unchanged. 15. Gum passes unchanged into the small intestines. 16. Gelatine becomes dissolved, and does not coagulate again; the greater part of it is absorbed. 17. Part of the casein contained in the milk reaches the jejunum in an uncoagulated state. 18. The mixture of the digestive fluids contained in the duodenum emulsifies fatty substances completely, if its reaction is alkaline, but less completely if it is acid. 19. This mixture of fluids exercises likewise a digestive influence on proteinaceous substances. 20. The lowest quantity of digestive fluids entering the jejunum within twenty-four hours amounts to more than one-seventeenth of the weight of the body.

Valentin's researches show that marmots, during the state of hybernation, continue forming fæcal masses, which are from time to time excreted, in general together with the urine. These fæcal substances do not consist of the remains of food, but of excretive products of the body (mucus, epithelium, biliary constituents.) The stomach of hybernating marmots always contains some almost transparent acid fluid, in which are floating white flakes, composed of epithelium. The fluid contains 1·40 to 2·21 per cent. solids, and is considered by Valentin as the result of desquamation and dissolution of the superficial layers of the mucous membrane of the stomach.

III. Blood, Circulation, Respiration.

1. **Schlossberger**: *Contribution to the Chemical Knowledge of Fœtal Life.* (Annalen d. Chemie, l. c.; and Canstatt, l. c., p. 169).
2. **Welcker, H.**: *On the Quantity of Blood and its Colouring Power, &c.* (Zeitsch. f. rat. Med., Ser. III., vol. iv., p. 145. 1858.)
3. **Vierordt**: *The Phenomena and Laws of the Velocity of the Circulation of the Blood, according to Experiments.* (Frankfurt, 1856; and Henle and Meissner's Bericht über Anat. und Phys. im Jahre 1857, p. 481.)
4. **Milne-Edwards**: *Lectures on Comparative Physiology and Anatomy.* (Vol. i., Paris, 1857; and Henle and Meissner, l. c., p. 242.)
5. **Nelson Pi**: *On the Movements of the Heart, the Sounds Produced, the Pulse, and the Movement of the Blood.* (American Med. Monthly, vol. x., p. 81, Aug., 1858).
6. **Calliburces**: *On the Influence of Warmth on the Activity of the Heart.* (Gaz. Hebd., No. VI., p. 468: and Canstatt, l. c., p. 69.)
7. **Valentin**: *Contribution to the Knowledge of Hybernation.* (Moleschott's Untersuchungen, vol. ii., p. 285. 1857.)
8. **Schnepf**: *On the Vital Capacity of the Lungs.* (Paris, 1858; Virchow's Archiv, vol. xiv., p. 213. 1828.)

Schlossberger found the blood of several fœtuses of cows either neutral or only slightly alkaline, coagulating by heat, without the previous addition of acetic acid. The blood of a fœtus, thirty weeks old, contained per 1000:

Coagulable substance	169·6
Fat	0·5
Ashes	9·6
Water	819·0

The ashes were composed of 6·1 soluble and 3·5 insoluble constituents; they yielded 1·3 per millemetre of oxide of iron.

The per centage of fibrin was very small; an incomplete coagulation took place after between two and four days; the blood contained in the heart and large vessels was quite fluid.

Welcker has continued his researches on the total amount of blood in various animals, according to his own method.* The author considers the number of observations hitherto made still too small to allow of the drawing of certain inferences, yet it appears to him very probable that the cold-blooded animals have much less blood than the warm-blooded; that there is an increase in the proportion of blood from the fishes to the naked, and from these to the scaly, amphibia; that the birds contain the largest amount, while the mammalia keep an intermediate place between the scaly amphibia and the birds. The average proportion of blood to the weight of body was found to be, for fishes, 1 : 49; for naked amphibia, 1 : 20·5; for scaly amphibia, 1 : 17·2; for birds, 1 : 12·1; for mammalia and man, 1 : 14·7. With regard to the differences observed between various species of the same family of animals, Welcker is inclined to believe that the size is of influence; but, contrary to Valentin, he has observed that the smaller animals contain a larger proportion. Within the same species the younger animals seem to possess a larger relative amount of blood, and the males a larger than the females.

Concerning the colouring power of the blood, Welcker's experiments tend to prove that in mammalia, birds, amphibia, and fishes, it nearly exhibits the proportion of 5, 4, 3, and 2; but the author himself is of opinion that more numerous experiments are necessary to establish this observation as a law.

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* Conf. the Physiological Reports in Nos. 38 and 43 of this Journal.

Vierordt calculates the total amount of blood by means of multiplying the figure expressing the mean duration of the circulation (T) with that for the amount of blood (V) passing from the heart in every unit of time, the total amount of blood being, according to Vierordt—VT: For man the author found V to be 207 cubic centimetres; T 23, 1 seconds; therefore the total amount of blood, 4782 cubic centimetres—5·06 kilogrammes—$\frac{1}{12.6}$ of the weight of body (63·6 kilogrammes). From a large number of experiments on the duration of circulation and on the average amount of blood, Vierordt arrived at the following inferences:

1. The mean duration of circulation of a species of mammalia is equal to the average time in which the heart completes 26-28 pulsations. 2. The mean proportion of the blood expelled by one ventricular contraction to the total amount of blood is nearly constant in the various families of mammalia. Every systole throws about $\frac{1}{26.5}$ to $\frac{1}{28}$ of the total amount of blood into the aorta. 3. The average duration of circulation is in proportion to the average duration of a complete ventricular movement (systole and diastole), or in an inverse proportion to the mean frequency of pulsation of the species of animal.

	Duration of Circulation.		Duration of a Pulsation of the Heart.	
	Seconds.	Relative.	Seconds.	Relative.
Rabbit	7·46	1	0·286	1
Young Goat	14·14	1·89	0·545	1·80
Dog	16·7	2·22	0·625	2·19
Man	23·1	3·09	0·833	2·91
Horse	31·5	4·22	1·091	3·82

4. The mean durations of circulation of two species of animals are nearly in an inverse proportion to the quantities of blood with which equal portions of the animal are supplied in an equal space of time. In one minute, one kilogramme of body receives the following quantity of blood:—Rabbits, 592 grammes; young goat, 311 grms.; dog, 272 grms.; man, 207 grms.; horse, 152 grms. 5. The mean duration of circulation is shorter in smaller than in larger animals. 6. The mean arterial pressure of blood in two species of mammalia bears a nearly inverse proportion to the mean quantity of blood flowing within an equal space of time through a portion of body of the same weight:

	Pressure of Blood in Millim. Hydrg.	Quantity of Blood.
Horse	280	152
Dog	150	272
Goat	135	311
Rabbit	80	592

According to this theory, the pressure of blood for man would be 200 millim. hydrg. 7. The mean arterial pressures bear nearly the same proportion as the mean durations of circulation. 8. The velocity of blood within the capillaries is in general proportional to the quantity of blood passing within the same time through portions of body of the same weight. The capillary circulation is therefore more rapid in the smaller than in the larger species of mammalia.

Milne-Edwards gives the diameter of the blood-globules of many species of animals. The author draws, from the comparison of the blood-globules of animals belonging to various classes of vertebrata, the inference that there exists a certain relation between the size of the blood-globules and the physiological activity, especially the respiratory want of the animal—viz., that the respiratory want is in an inverse proportion to the size of the globules, or that the greater the respiratory want, the smaller the diameter of the globules. This relation is evidently much in harmony with the view that the blood-globules are the bearers of oxygen, as the same weight of smaller globules offers a larger surface than the same weight of larger globules.

Nelson considers the diastole of the heart as the effect of an active movement. He assumes that the heart-muscles, unlike voluntary muscles, are possessed of a double power, that of expansion as well as of contraction (p. 82.) The distension of the ventricles, the author says in another place, "is an active and inherent force." The mechanical forces acting on the movement of the blood in the veins are:—1. That furnished by the heart and arteries; 2. That by the pleural vacuum of the thorax; and 3. The expansive power of the auricle. A fourth, "possibly physiological," power the author is inclined to assume, from the fact that soon after death the body becomes mottled, even in the elevated parts of the body, while these parts are left completely white at a later period. Nelson himself does not endeavour to explain the nature of this assumed fourth force. The emptiness of the arteries and their flattening after death is "due to two forces,—a vacuum and an interstitial circulation or movement; and because the pleural vacuum can with difficulty reach through the hepatic system to the mesenteric arteries, they are never found so completely empty as the arteries belonging to the extra digestive system."

Calliburcès has made experiments on the influence of warmth on the action of the heart. He found that the number of pulsations of the heart of a frog is considerably increased by immersing his hind legs into warm water; the same result was obtained after section of the crural nerves, and after poisoning by curare, showing, to the author's mind, that the nerves are not connected with this increased frequency of pulsation. The immersion of a cut out frog's heart into water of 40° Cent. (140° F.) raised the number of contractions from 18 to 94 per minute; in a second case from 38 to 80. Water of 25° C. (77° F.) caused an increase from 30 to 62; water of 50° (122° F.) from 36 to 72. Water of 10° (50° F.) stopped the action of the same heart, while soon afterwards the immersion into water of 50° (122° F.) produced 82 pulsations.

Valentin found that the waking marmot excreted in the average 75 times more carbonic acid, and inhaled 41 times more oxygen than the same animal in the most complete state of hybernation. The stages between waking and most profound hybernation yielded intermediate figures. A waking hedgehog yielded about 20·5 more carbonic acid, and consumed 18·4 times more oxygen than one in the state of hybernation. The author's observations further show, that the profounder the stage of hybernation the more is the consumption of oxygen in excess to the excretion of carbonic acid. The proportion of carbonic acid excreted to the oxygen consumed was, in the waking state, 1 : 0·90; in the half-sleeping condition, 1 : 1·01; in the quiet sleep, 1 : 1·39; in the profound sleep, 1 : 1·65.

Schnepf's observations on the vital capacity of the lungs are made by Bernard's spirometer. Regarding the influence of age, he found that the capacity is highest in the age of twenty. The greatest increase takes place between the years of fourteen and seventeen. The average increase for every year below the age of ten is about 140 cubic centimetres; the average capacity at the age of ten years being 1400 cubic centimetres; from the tenth to the twentieth year the annual increase is more considerable, amounting in the average to 260 cubic centimetres. For the period of decrease after the age of twenty no certain figure, expressing the annual diminution, can be fixed as yet. The capacity of women is smaller than that of men. With regard to height of body, persons of the same height may exhibit a difference of 1200 or 1300 cubic centimetres. Although the vital capacity in general increases in proportion to the height, yet there are many exceptions, and the age is, according to the author, of greater importance. There does not exist a certain relation between the circumference of the chest and the vital capacity.

IV. ABSORPTION, NUTRITION, AND METAMORPHOSIS OF MATTER; SECRETION AND EXCRETION.

1. HEIDENHAIN: *The Ways of Absorption of Fat.* (Moleschott's Untersuchungen, vol. iv., p. 251, 1858.)
2. BERNARD: *O nthe Mechanism of the Formation of Sugar in the Liver.* (Compt. Rend., vol. xliv., p. 578, 1857; and Gaz. Méd., No. 13, 1857.)
3. PELOUZE: *On the Glycogenic Matter.* (Compt. Rend., vol. xliv., 1857; and Henle and Meissner, l. c., p. 247.)
4. SANSON: *On the Formation of Sugar in the Animal Economy.* (Compt. Rend., vol. xliv., pp. 1159 and 1323, and vol. xlv., p. 343, 1857; and Henle and Meissner, l. c., p. 247.)
5. BÉRARD: *On the Physiological Formation of Sugar in the Animal Economy.* (Bull. de l'Acad., vol. xxii., p. 774, 1857; and Schmidt's Jahrb., vol. xcvii., p. l., 1858.)
6. CHAUVEAU: *The Substance which in the Blood of Starving Animals reduces the Oxide of Copper, is Fermentable Sugar.* (L'Union, 1857, No. 89, p. 366; and Schmidt's Jahrb., l. c., p. 1.)
7. FIGUIER: *New Facts and Considerations against the Existence of the Sugar-forming Function of the Liver.* (Gaz. Hebd., vol. iv., pp. 577 and 608, 1857; and Schmidt's Jahrb., l. c. p. 1.)
8. SCHIFF: *On the Glycogenic Substance in the Liver of Frogs.* (Archiv f. Phys. Heilk., 1857, p. 263; and Schmidt's Jahrb., l. c., p. 1.)
9. LEHMANN: *On the Formation of Sugar in the Liver.* (Schmidt's Jahrb., vol. xcvii., p. 1, 1858.)
10. MOOS: *On the Glycogenic Function of the Liver, especially in its Relation to the Nervous System.* (Archiv d. Vereins f. wiss. Heilk., vol. iv., p. 38, 1858.)

11. **Nasse, H.**: *On some Differences in the Condition of the Liver of Starving and of Fed Animals.* (Archiv d. Vereins f. wiss. Heilk., l. c., p. 77.)
12. **Pavy**: 1. *On the Alleged Sugar-forming Function of the Liver.* 2. *The Influence of Diet on the Liver.* (Guy's Hosp. Rep., third series, vol. iv., p. 291, 1858.)
13. **Valentin**: *Contribution to the Knowledge of Hybernation.* (Moleschott's Untersuchungen, vol. ii., p. 1; vol. iii., p. 195, 1857; and vol. iv., p. 58, 1858.)

Städeler: *On the Oxidation of Albumin by Hypermanganate of Potash.* (Erdmann's Journ., vol. lxii., p. 251; and Canstatt, l. c., p. 159.)

15. **Weismann**: *On the Formation of Hippuric Acid in Man.* (Zeitsch. f. rat. Med., third series, vol. ii., p. 331, 1856.)
16. **Hecker**: *Remarks on the so-called Lithic-acid Infarct of Children.* (Virch. Archiv, vol. xi., p. 217; and Henle and Meissner, l. c., p. 339.)
17. **Krabbe**: *On Phosphoric Acid in the Urine.* (Kopenhagen, 1857; and Canstatt, l. c., p. 181.)

Heidenhain continued the researches commenced by Brücke, Funke, Köllicker, and others, on the construction of the mucous membrane, and especially the epithelium of the small intestines. Brücke's statement, that the broad surface of the epithelial cells, pointing to the cavity of the tube, is open, is supported. After having injected oil into the stomach of frogs, the author was enabled to recognise on the intestinal mucous membrane, prepared by means of a weak solution of chromic acid, that the epithelial cells and their continuations are provided with continuous hollow channels. From this observation, and similar ones made on fishes, and also, though with more difficulty, on mammalia, Heidenhain concludes:—"That the epithelial cells combined with the cells of the sub-epithelial tissue, which are in an open connexion with the former, offer a system of hollow channels provided with complete walls, which channels serve as pre-existing passages for the fat from the intestinal tube to the chyliferous vessels." The author is inclined to consider his observation as a proof of the correctness of the view of some histologists, that the areolar-tissue corpuscles are the commencement of the lymphatic vessels (Virchow, Leydig, Friedreich).

The subject of the formation of sugar in the liver has occupied, during the last ten years, some of the most able physiologists and physiological chemists. Opposite statements have been made by various observers, and have been repeated within the last two years. The same facts are quoted in support of the opposite views. We purpose giving a short digest of the principal essays on the subject, and commence with the description of the substance discovered by Bernard and Hensen,* as giving rise to the formation of sugar. When pure, this glycogenic substance is a whitish, tasteless, inodorous, neutral, apparently not crystalline powder; it is soluble in water, presenting, when the solution is moderately concentrated, a milky appearance; it is insoluble in alcohol; with iodine it yields a reaction which resembles that of dextrine—viz., it gives rise sometimes to a bluish, sometimes to a violet, a reddish-brown, or a dark blood-red colour; it does not reduce the oxide of copper from an alkaline solution, but it is easily transformed into sugar by ferments, saliva, pancreatic juice, blood, &c. It is, however, important to observe that, although the contact with the saliva at the temperature of about 100°, leads to an almost instantaneous production of sugar, when the solution of the substance is neutral, a small addition of either acid or alkali can interfere with such a result. By Hensen, this body has been called sugar-forming substance, by Bernard glycogenic matter, Pavy proposes the term hepatine, Nasse calls it amylum (Stärkemehl), Schiff is likewise inclined to consider it as animal amylum, and Pelouze's researches show, at all events, the greatest analogy between this substance of the liver and vegetable amylum, quite independently of the great susceptibility of transformation into sugar common to both. By means of fuming nitric acid, Pelouze succeeded in transforming the substance in question into *xyloidin*, and found for it the formula $C_{12}H_{12}O_{12}$, that for vegetable amylum being $C_{12}H_{10}O_{10}$. Schiff describes also the microscopical appearance of the liver-amylum, and his statement is corroborated by Nasse. They consider the minute granules found within the liver-cells, besides the nucleus and fat-globules, as representing the sugar-forming substance; they find that the number of granules is largest in the livers yielding most sugar, smallest in those from sickly animals yielding a small amount of sugar; they find the granules altogether wanting in livers of animals perished by starvation, as also in pieces of liver which had been exposed to the influence of saliva.

With regard to the presence or absence of the glycogenic substance in the liver, Bernard found that febrile diseases and other disturbances in the nutrition caused diminution or total disappearance of the substance. Nasse performed a large number of experiments (most of them on rabbits, some on dogs), respecting the influence of food or abstinence, as well on the liver in general as also on the quantity of liver-sugar and sugar-forming substance contained in it. The livers of all the fed animals, their food having consisted in carrots, or in potatoes and hay, yielded much sugar (2·1 per cent.), that of most of the rabbits deprived of food during forty-two to sixty-six hours before death, yielded a very small quantity, but always some; that of those perished from starvation yielded none whatever. The glycogenic substance, too, was wanting in animals subjected to starvation; while amongst the fed animals, the amount was larger in those which had received vegetables rich in sugar mixed with proteinaceous substance, than in those fed on amylaceous food and potatoes. Pavy's experiments on the same subject lead to further results. This author's articles will be reviewed in another section of this Journal; we can, however, not refrain from shortly mentioning the results obtained by him regarding the influence of the quality of food on the amount of glycogenic substance (Pavy's *hepatine*). The average per-centage of hepatine in the liver of eight dogs kept on animal food was 6·97, in the liver of three dogs kept on vegetable food it was 17·23, in those of four dogs kept on tripe and sugar 14·5. We may reasonably infer, that from sugar and allied substances the extra amount of hepatine was derived in these instances.

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* Conf. this Journal, No. 39. 1857.

Moos, in his researches on the influence of section of the pneumogastric nerves on the amount of sugar yielded by the liver, found that on an average (from 6 cases) 500 grammes of rabbit, after the section of the pneumogastric nerves, yielded 0·1 gramme of liver-sugar; while 500 grammes of healthy rabbit yielded 0·7 grammes of sugar—i. e., seven times more. The result of the experiments performed on dogs was a similar one. The author, however, is not inclined to attribute to the section of the pneumogastric nerves a *direct* influence on the sugar-forming substance; but he explains the diminution or disappearance of sugar by the abstinence from food, and the increased metamorphosis of matter after the section of the nerves; a view which is in accordance with the results obtained by Nasse in his experiments on the section of the pneumogastric nerves,* and on the influence of starvation on the liver.

In his experiments on the influence of the spinal marrow on the sugar-forming function of the liver, Moos observed that galvanic irritation of the spinal marrow causes diabetes in a shorter time even than the well-known puncture of the medulla oblongata; while section of the spinal marrow causes the speedy disappearance of the sugar, even if the ingestion of food continued. As the vagi do not exercise a direct influence, and the spinal marrow has no other communication with the liver than through the sympathetic nerves, the author considers these as the regulators of the formation of sugar in the liver.

With regard to the influence of hybernation and similar conditions, Valentin found that even after five months' torpidity, the liver of marmots still contained sugar-forming substance; but that the liver of animals perished from exhaustion, during or after hybernation, contained no trace of it. Schiff found, that the liver of frogs contains no sugar in February, that already in January artificial diabetes cannot be effected in them.

Figuier maintains the assertions mentioned in our former reports; his memoirs continue to exhibit, according to Lehmann, the same proofs of chemical inaccuracy as they did before.

Sanson endeavours to demonstrate, that all the sugar met with in the system, both of herbivora and carnivora, is derived from the food; that meat, too, contains a large amount of dextrine, that the glycogene or sugar-forming substance is only dextrine in an altered condition. Sanson finds, as well in the venous as in the arterial blood, a substance analogous to dextrine; he considers that the liver only abstracts it from the blood, and stores it up within its tissue. That the blood may contain sugar, the analyses of Bernard himself and of Lehmann sufficiently prove. Nasse, in his last memoir, again communicates observations on the subject; he found sugar in the blood of dogs after amylaceous diet, but did not find it after animal food. The quantity of sugar found by these observers is too small to sustain Sanson's view.

The results of Chauveau's experiments have been mentioned in a contribution to this Journal by Harley;† we may, therefore, refer to that memoir.

Bérard found in the thoracic duct of an ox, fed with animal food through a fistula made in the first stomach, a large quantity of sugar. On this and other similar experiments he bases the view, that sugar is constantly formed in all parts of the animal organism, which sugar, by means of the absorbents, is carried to the centre of the circulation; that, however, the digestion adds to this permanent formation of sugar, another intermittent but more active one. The author thinks that not in tubes and hollow channels, but within the substance of the tissues the formation of the sugar takes place. We need scarcely mention, that Bernard would explain the existence of sugar in the thoracic duct as derived from the lymphatics of the liver.

With regard to the influence of food or of abstinence on the size and physical condition of the liver, Nasse found, as the mean weight of the liver of fourteen rabbits fed on potatoes, hay, and carrots, 43·53 p. m., the maximum being 55·47 p. m., the minimum 32·6 p. m.; that of sixteen rabbits killed after, in general, forty-two to forty-three hours' starvation, 35·12 p. m., the maximum being 43·28 p. m., the minimum 24·8 p. m.; that of six rabbits perished of themselves, after a shorter abstinence, 34·8 p. m., the maximum being 37·8 p. m., the minimum

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* Archiv für wissensch. Heilk., vol. ii. 1856; and this Journal, No. 35. 1856.

† On the Origin and Destruction of Sugar in the Animal Economy, No. 39. 1857

30·0 p. m. If we compare these figures with those found by Valentin, for the liver of the marmot in the beginning and at the end of hybernation, we find for the former condition 33·3 p. m., for the latter 22·5 p. m.

Of Pavy's researches on the influence of various kinds of food, we will only mention, that the mean weight of the liver of eleven dogs, under the influence of animal food, amounted to $\frac{1}{30}$th part of the entire weight of the animal; that of five dogs kept on vegetable diet to $\frac{1}{13}$th; that of four dogs kept on animal food mixed with sugar to $\frac{1}{16 \cdot 3}$th of the entire weight.

The liver of starved animals is, according to Nasse's observations, of a brownish red or dirty violet colour, and is always dark-coloured; that of fed animals of a greyish red, and lighter colour, as well on the surface as in the interior; in the former state it is firmer, more tenacious; in the latter softer.

The gall-bladder of the starved animals, in Nasse's experiments, was always quite filled, the bile was thick and of green colour; the gall-bladder of the fed animals was almost empty, the bile had the appearance of coloured mucus. Valentin's researches on the hybernation of marmots give for the gall-bladder in the commencement of hybernation a condition analogous to that found in fed animals; at the completion of hybernation, to that found in starved animals.

The just-mentioned essays of Valentin are replete in interest for the study of the phenomena of nutrition and metamorphosis of matter in general. The average loss of 1 kilogramme marmot after about 163 days of hybernation is stated as 351·45 grm. The following table shows how this loss is distributed between the different organs.

A. *Commencement of Hybernation.*

Organs.	Per cent. of weight of body.
Muscles of trunk and limbs	26·19
Skeleton	17·34
Fat	17·05
Skin	16·39
Liver	3·33
Stomach	1·91
Colon	1·69
Small intestines	1·49
Gland of hybernating mammals	1·33
Brain	1·08
Larynx and lungs	1·03
Tongue	0·62
Heart	0·51
Diaphragm	0·60
Kidneys	0·61
Eyes	0·34
Spinal marrow	0·26
Salivary glands of mouth	0·17
Œsophagus	0·13
Penis	0·098
Spleen	0·096
Urinary bladder	0·091
Suprarenal capsules	0·048
Harder's glands	0·048

B. *Termination of Hybernation.*

Organs.	Per cent. of weight of body.
Muscles of trunk and limbs	29·73
Skeleton	24·85
Skin	17·13
Colon	3·31
Small intestines	2·26
Liver	2·25
Brain	2·03
Stomach	1·63
Tongue	0·99
Larynx and lungs	0·95
Kidneys	0·93
Heart	0·72
Gland of hybernating mammals	0·68
Eyes	0·63
Diaphragm	0·54
Spinal marrow	0·48
Œsophagus	0·26
Urinary bladder	0·25
Salivary glands of mouth	0·23
Spleen	0·14
Harder's glands	0·08
Penis	0·07

We find at once that the brain, the spinal marrow, the eyes, the œsophagus, the colon, and other organs, have a much larger per-centage at the termination than at the beginning of hybernation—i.e., that their proportionate loss is smaller than that of other organs; while the fat (not introduced by the author into the series B), which in the beginning of hybernation amounted to 17 per cent. of the entire weight, had decreased to less than $\frac{1}{8}$th per cent. The gland peculiar to the hybernating mammals exhibits, after the fat, the greatest proportionate loss, sinking from 1·33 per cent. in A, to 0·68 per cent. in B; the liver, too, loses much in proportion, maintaining in B only 2·25 per cent., while it occupies in A 3·33 per cent.

If we compare the results of hybernation with those of starvation, as described by Chossat,* we find that the starving pigeon daily consumes in the average 40 times more muscular substance than the marmot in the state of torpor, and only 11 times more fat, 33 times more of the tissue of the alimentary canal, 18·3 times more liver, 15 times more lung, 5 times more skin. The starving pigeon consumes, therefore, compared with the torpid marmot, a considerably larger proportion of noble organs, as muscle, than of the more easily recoverable fat. The starving animal, having lost in weight the same proportion as the hybernating animal after the state of torpor, will be left much weaker than the latter. The mean daily loss from starvation in rabbits and guinea-pigs bears to that from hybernation the proportion of 18·3 : 1.

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* Recherches Expérimentales sur l'Inanition. Paris, 1843.

The phenomena of excretion of two marmots in the state of torpor are given in the following figures:

Marmot.	Mean weight of body in grammes.	Average amount of daily excretion during the whole duration of the hybernation.			
		Fæces.	Urine.	Sum of perceptible excretion.	Perspiration.
No. 1	802·6	0·019	0·807	0·826	1·100
No. 3	1383·4	0·024	1·283	1·307	2·198

The larger figures belonging to No. 3 are caused by the circumstance of the animal having been repeatedly awake, and having, on the whole, slept less profoundly. Another table given by the author demonstrates, that the relative loss of weight bears an inverse proportion to the profoundness of the state of hybernation.

Städeler has repeated Bechamp's* experiments on the formation of urea from albumen; he did not, however succeed in obtaining any urea; but in making the experiment, as much as possible according to Bechamp's description, he found that he had formed benzoic acid. The view that benzoic acid may be formed within the organism from proteinaceous substances is, therefore, rendered more probable by experimental proof.

Weismann's researches on the formation of hippuric acid in man corroborate likewise the just-mentioned view. This author finds, that hippuric acid is excreted with the urine, even after during several days no other but animal food had been taken. As, however, hippuric acid appears to be always formed by the union of benzoic acid and glycin,† it seems correct to infer that benzoic acid is a constant product of the metamorphosis of tissue. Weismann found that his urine, when he lived on mixed diet, contained on an average 0·14 per cent. of hippuric acid, or 2·17 grammes daily. When he took, during three days, only meat and eggs, the amount of hippuric acid varied from 0·72 grammes to 0·79 grammes. The quality of food exercises, therefore, a distinct influence on the quantity of the acid excreted. The urine of several patients affected with typhoid fever, who had for between two and four weeks taken nothing but milk and broth, contained likewise about 0·05 per cent. of hippuric acid.

Hecker examined the urine of children within the first days after birth. The specific gravity was always very low, the quantity of solid ingredients small. The urine of still-born children was always slightly acid; in one case it was composed, according to Hoppe's analysis, of 3·26 p.m. organic, 2·71 inorganic solid constituents, and 994·03 water. The urine of a healthy child, eight days of age, was pale straw-colour, acid, of 1·00233 specific gravity, and contained (Hoppe):

Urea	0·41	p. m.
Lithic acid	0·11	„
Other organic constituents	6·14	„
Salts { Soluble	1·60	„
Salts { Insoluble	0·01	„
Solids	8·27	„
Water	991·73	„

Until to the end of the third day the amount of urine is inconsiderable; during the following days it is about three ounces; the composition remains up to the twenty-fifth day similar to that of the still-born child. The proportion of lithic acid was always small; Hecker is, therefore, opposed to the view that the formation of a lithic-acid deposit in the kidneys of children is a physiological phenomenon.

Krabbe found, as the average of phosphoric acid for twenty-four hours, 3·429 grammes, of which quantity 0·600 grammes were combined with earthy bases. With regard to the cloudiness and the sediment formed in some urines by the process of boiling, the author states that the sediment contains, besides lime, phosphoric acid, carbonic acid, and organic matter; sometimes also magnesia. Krabbe found such urine always alkaline, neutral, or only slightly acid. Scherer adds to the abstract from Krabbe's essay, that he has long ago remarked that the precipitation of the earthy phosphates by boiling is caused by the presence of the so-called neutral phosphates of lime and magnesia; that, however, also the sesquicarbonates of lime and magnesia may give rise to this phenomenon during boiling.

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* Conf. this Journal, No. 41. 1858.

† Conf. Kühne and Hallwachs on the Formation of Hippuric Acid after the Ingestion of Benzoic Acid, in this Journal, No. 43, 1858.

V. Nervous System.

1. **Kussmaul** and **Tenner**: *On the Origin and Nature of Epileptiform Convulsions from Loss of Blood, and on Epilepsy in General.* (Moleschott's Untersuchungen, iii., p. 1. 1857.
2. **Stich**: *Contribution to the Knowledge of the Chorda Tympani.* (Annalen d. Charité, Jahrgang viii., p. 59; and Canstatt, l. c., p. 114.)
3. **Spiegelberg**: *On the Nervous Centres of the Movements of the Uterus.* (Zeitsch. f. rat. Med. third series, vol. ii., p. 1, 1857; and Schmidt's Jahrb., vol. xcvii., p. 277.)
4. **Spiegelberg**: *On the Movement of the Intestines.* (Zeitsch. f. rat. Med., l. c., p. 44; and Schmidt's Jahrb., l. c., p. 279.)
5. **Kupffer** and **Ludwig**: *On the Relation of the Pneumogastric and Splanchnic Nerves to the Motion of the Intestines.* (Zeitsch. f. rat. Med., third series, vol. ii., p. 357, 1858.)
6. **Budge**: *On the Sensitiveness of the Abdominal Ganglia.* (Deutsche Klinik, No. 20, 1858.)
7. **Czermak**: *Contribution to the Knowledge of the Influence of Nerves on the Secretion of Saliva.* (Sitzungsbericht d. Wien. Academ., vol. xxv., Heft 3, p. 3; and Canstatt, l. c., p. 119.)
8. **Harless**: *On the Importance of the Neurilemma.* (Zeitsch. f. rat. Med., third series, vol. iv., p. 168, 1858.)
9. **Moos**: *On the Influence of the Nervous System on the Glycogenic Function of the Liver.* (Conf. sub. iv.)

Kussmaul and Tenner produced, by compression and ligature of the arteries of the neck in healthy rabbits, the same epileptiform convulsions which are seen in animals bleeding rapidly to death. In very debilitated or etherized animals neither loss of blood nor compression of the arteries caused the convulsions. The closure of both carotid and subclavian arteries was necessary in order to effect the convulsions; if circulation was allowed to continue in any one of the four vessels, convulsions never ensued, but only weakness and paralysis. The general convulsions usually commenced about eight to eighteen seconds after the interruption of the arterial circulation. For the description of the convulsions we must refer to the essay itself. Epileptiform they are, according to the authors, because the animals before the commencement of the general convulsions fall down, deprived of voluntary motion, with the appearance of loss of consciousness, never screaming before or during the attack; further, because the pupils are, during the attacks, dilated and motionless, and the eyeballs likewise fixed; that the attacks begin with contraction of the cervical muscles, that the respiration is suspended, although the heart's action is uninterrupted; and that the atonic convulsions of the limbs end in tonic convulsions, are additional causes for the authors' view regarding the nature of the attacks. As soon as the interruption of the circulation ceases, the convulsions leave off. The sudden return of the blood to the head never causes convulsions. By means of holes made in the cranium, the authors observed that the stoppage of the circulation was followed by capillary anæmia and venous oligæmia of the brain and its membranes; that the venous oligæmia became diminished simultaneously with the commencement of the convulsions, but that the paleness of the brain continued. As the essential cause of the convulsions, the disturbed nutrition of the brain is considered. The diminished mechanical pressure is regarded as of only secondary importance. The attacks took place even when a great part of the cerebrum, cerebellum, and medulla oblongata was laid open. Anæmia of the spinal marrow caused only paralysis of the limbs, the muscles of the trunk and respiration, never real convulsions; when the privation of blood was sudden and complete, slight trembling motions of the limbs preceded the paralysis. A series of experiments, made by taking off parts of the cerebrum, proved that the epileptiform convulsions did not depend on the cerebrum, but on the suddenly interrupted nutrition of the motor centres situated behind the optic thalami. From these results, and the authors' experience on man, they infer that epileptic convulsions in man occur only when, besides the cerebrum, some of the parts, or all parts of the brain situated behind the optic thalami, become suddenly deprived of the sufficient amount of blood; that, however, the fall preceding the convulsions, the loss of consciousness and sensibility, depend on the affection of the cerebrum. Congestion of the brain, either active or passive, appears to them a very improbable cause of epileptiform convulsions. The sphagiasmus and trachelismus of M. Hall, the authors say, is not to be considered as a source of epileptic paroxysms, but the laryngismus is such a source. It seems further probable, from some experiments, that some forms of epilepsy are connected with a spasm of the contractile elements of the cerebral arteries. The medulla oblongata, therefore, as the origin of the contraction of the glottis and of the vaso-motor nerves, may be regarded as a frequent cause of epileptic paroxysms.

Stich observed that in cases of paralysis of the facial nerve the corresponding margin of the tongue exhibits a diminution of taste, which phenomenon does, however, not extend to the root of the tongue. The diminution of taste does not take place if the cause of paralysis lies on the base of the cranium; but if the cause lies at the origin of the chorda tympani, or has a more peripheric situation, the disturbance of taste occurs. Schiff remarked this phenomenon in a man whose facial nerve had been cut through close to its emersion from the stylomastoid foramen. It appears, therefore, that this disturbance is attributable not to the facial nerves, but to the fibres derived from the fifth pair.

Spiegelberg concludes, from numerous experiments on rabbits, cats, and guinea-pigs, regarding the movements of the uterus, 1. That the stoppage of the circulation is the cause of the peristaltic movements of the uterus. The uterus resembles in this respect the intestinal tract. Compression of the aorta immediately below the diaphragm causes always movements of the tubes of the uterus and intestinal tract, which continue as long as the compression lasts, and cease as soon as the interruption of the circulation is suspended. 2. The pneumogastric nerves exercise no immediate influence on the uterus; but if they stop the action of the heart, this circumstance acts like the compression of the aorta. 3. Irritation of the medulla oblongata excites contractions of the uterus, which phenomenon is not influenced by section of the pneumogastric nerves. 4. The cerebellum is the principal centre for the movements in question. Irritation of any part of it excited the action of the uterus, that of the posterior half of the corpora quadrigemina was likewise often successful, that of the cerebrum never. 5. The movements may be excited from every portion of the spinal marrow, but especially from the lumbar and sacral portion. The movement of the pregnant uterus may be stopped by means of irritation of the spinal marrow; this, however, the author attributes to over-excitation. 6. The sympathetic and sacral nerves form the means of communication between the nervous centres and the uterus. 7. The impulse to the parturient action of the uterus is not given by the nervous system, but probably by local changes of the circulation in the uterus, occurring at the termination of pregnancy.

The same author confirms the dependence of the intestinal movements on the spinal marrow and brain. From all places which excite the movement of the uterus, that of the intestines may be likewise excited. Interruption of the circulation effects likewise very active movement of the intestinal canal; compression of the aorta does it to a greater degree than compression of the vena cava or vena portæ. This would be opposite to Brown-Séquard's view, that the carbonic acid in the blood is the cause of the peristaltic movements. It would also appear incorrect to ascribe the peristaltic motion of the intestines exposed to the air immediately after the death of the animal merely to the excitation by the air.

Pflüger's assertion, that irritation of the splanchnic nerves stops the peristaltic movement of the intestinal canal, has been repeatedly contradicted. Kupffer and Ludwig found that irritation of the nerves in question may as well excite the action of the intestine as also stop it, according to the condition of the latter. We may add to this, that there are now various observations extant, that the same nerve may cause movement and may arrest movement. Schiff's view, that the arresting of movement is not a peculiar action of the nerve, but the product of over-irritation—i.e., exhaustion, appears to deserve the attention of experimenters. We need only mention that Erkhard observed acceleration of the heart's contractions by *moderate* irritation of the pneumogastric nerve.

Budge found, in his experiments on various animals, contrary to the view expressed by Romberg,* that the ganglion semilunare and the ganglion mesaraicum are in a considerable degree sensitive; at all events, as sensitive as the pneumogastric nerve, the splanchnic nerve, the ramus communicans, and the cervical ganglion of the great sympathetic; but much less so than the fifth pair or the posterior roots of the spinal nerves.

Czermak confirms Ludwig's observation, that irritation of the sympathetic branch of the submaxillary gland, or of the cervical portion of the sympathetic nerve, may excite the secretion of saliva just as much as irritation of the glandular branch of the lingualis. The author remarks, however, in addition, that electric irritation of the cervical part of the sympathetic nerve may diminish and even suspend the secretion of saliva. Sometimes the irritation of the glandular branch of the lingualis does not influence the secretion of saliva when the sympathetic nerve had been previously irritated. If both nerves are stimulated simultaneously the secretion rapidly reaches its maximum, but after fifteen or twenty minutes it becomes diminished and frequently ceases altogether. We have therefore another proof of the arresting action of the nervous system, if we do not prefer to explain the phenomenon just described as the product of over-irritation.

The experiments of Harless, made on the nerves of frogs, prove that the sheaths of the nerves are of great importance in regulating the irritability of the nerves. The author considers himself entitled to infer that the irritability and the conducting power of the nerve are in close connexion with the proportion of water contained in the nerve—viz., that they become much increased by any loss of water sustained by the nerve. He further maintains, that the degree of irritability and conducting power varies with the degree of pressure exercised by the sheath; that this pressure becomes increased by the process of exudation of the sheath; that, therefore, the iritability and conducting power of the nerve depend to a great degree on the percentage of water contained in the sheath.

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* Lehrbuch der Nervenkrankheiten, third edition, vol i. p. 151.

HALF-YEARLY REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By Robert Hunter Semple, M.D.

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I. *On the Beneficial Effects of the Employment of Belladonna in Incontinence of Fæcal Matters, existing alone, or accompanied with Enuresis in Children.* By Dr. A. Richard. (Bulletin Général de Thérapeutique, Aug. 15, 1855.)

Dr. Richard, being in attendance on a boy suffering under incontinence of fæcal matters, which had resisted all the methods of treatment usually recommended, was induced to try the effects of belladonna, which succeeds so often in cases of incontinence of urine. He prescribed the syrup of belladonna internally, and placed a suppository coated with belladonna pomade in the rectum. Two days after the commencement of this treatment, the child had no more involuntary stools, and was cured of his infirmity. The same treatment was afterwards employed upon four patients suffering from the same inconvenience, and in three the results were very satisfactory. M. Bercioux, who conducted the treatment of the four patients, considers that belladonna may probably act more rapidly on the functions of the rectum than on those of the bladder, and that instead of being classed among the stupefying agents, it ought henceforth to be considered as an exciting medicine like strychnia and ergot of rye, inasmuch as it cures an affection which M. Bercioux considers to be due to muscular atony. The editors of the 'Bulletin,' however, do not assent to this view of the action of belladonna, but they regard the incontinence as being due to a destruction of the equilibrium existing between the two antagonistic muscular powers of the rectum or the bladder, and that the belladonna, by its special properties, restores the equilibrium, and thus brings back the functions to their normal condition.

II. *On Glycerole of Tar and Oil of Cade (Juniperus Oxycedrus) as Local Applications in Eczematous and Impetiginous Eruptions.* (Bulletin Général de Thérapeutique, Aug. 15, 1858.)

Modern observation has proved that resinous and empyreumatic products are among the most powerful local remedies in the treatment of cutaneous eruptions. Purified tar, mixed with lard, is in constant use at the Hôpital St. Louis in Paris, but since the introduction of glycerine into pharmacy, M. Gibert employs it in place of lard. In order to facilitate its use, the mixture is thickened by the addition of starch. This preparation possesses the advantage over ordinary greasy applications, of being removed by water. This *glycerole* is prepared by mixing together thirty grammes of glycerine with two grammes of purified tar, and adding while hot fifteen grammes of powdered starch. By this means a pommade is obtained which is not very thick, and is easily spread. This topical application relieves itching, dries up excoriations, absorbs exhalation, and dissipates redness—in fact, it acts as an astringent and resolvent without producing irritation. Thus eczema rubrum, impetigo, prurigo of the scrotum and anus, acne rosacea, and sub-inflammatory mentagra, are modified in a very advantageous manner under its use. Another product frequently employed by M. Gibert is the oil of cade (*juniperus oxycedrus*), but from the circumstance that this oil possesses empyreumatic properties in a much higher degree than tar, it is usually mixed with oil of sweet almonds or cod-liver oil. A combination with the latter oil is known under the name of the *huile cadée* at the Hôpital St. Louis. It is particularly in pruriginous, papular, and obstinate eczematous eruptions about the anus and the generative organs, that M. Gibert has found this oil efficacious. In certain cases, he combines with the use of this ointment the internal employment of the liquor arsenicalis.

III. *On the Opiate Treatment of Rheumatic Fever.* By R. W. O'Donovan, Belturbet. (The Dublin Quarterly Journal of Medical Science, May, 1858.)

The opiate treatment of rheumatic fever, though not new in medical practice, is shown by Mr. O'Donovan to have been very successful in seven cases which he records. The opium was the chief remedy employed in all the cases, a little castor-oil being sometimes given to obviate constipation, and in some instances soothing liniments were applied to the joints, and quina was administered internally. Mr. O'Donovan commenced with a full dose of opium—namely, two grains every second or third hour, till the pain was relieved and the patient slept, or at least until he felt easy and composed; and headache was never observed. The opium treatment shortens the duration of the disease, as was proved by one of the cases in which the disease had lasted for thirteen weeks without mitigation, but in which, under the use of opium, relief was obtained in two days. In another case, the opium was commenced on the sixth day of the illness, and convalescence was established in four days afterwards. Another case was convalescent in four days, and a fourth was relieved in seven days. Mr. O'Donovan also found that the excessive sweating, characteristic of the disease, yielded to opium.

IV. *On the Pharmacology and Toxicology of the Ranunculaceæ.* By Professor Clarus, of Leipzig. (Zeitschrift der Gesellschaft der Aerzte zu Wien, Aug. 16, 1858.)

Most of the ranunculaceæ indigenous to Germany are distinguished by an acrid burning taste, and on this account, and from their injurious effects upon cattle when fresh, they are considered to be poisonous. The ranunculus sceleratus, R. flammula, and R. acris, and the Pulsatilla pratensis and P. vulgaris, may be regarded as being endowed with the most acrid properties. The fact that the true ranunculi lose their acrid taste by drying, while the pulsatillas retain their active principles, leads to the belief that while the poisonous properties of the former are due to the presence of a volatile principle, dissipated by drying, those of pulsatilla depend upon the presence of the same volatile principle, together with another acrid and narcotic substance. Dr. Clarus has ascertained that the narcotic principle of the pulsatillas is *anemonin*, and the acrid principle is a resin with an acid reaction, while the acrid principle common to the ranunculus sceleratus and pulsatilla pratensis is a volatile oil.

From chemical experiments on the ranunculaceæ and the administration of the products to the lower animals, Dr. Clarus has arrived at the following conclusions:—1. The ranunculus sceleratus, and probably the rest of the ranunculi, belong to the class of narcotico-acrids, inasmuch as they induce inflammation of the stomach and bowels, and irritation of the kidneys, and they diminish the number of the pulse and of the respirations. 2. The acrid principle of the ranunculus sceleratus is a volatile oil, soluble in ether, of a very pungent taste and smell, which probably becomes decomposed, by keeping in close vessels, into inert anemonic acid and the narcotic principle, anemonin. 3. The narcotic principle of ranunculus sceleratus is anemonin; it exists in this plant in much smaller proportion than in pulsatilla pratensis, and hence the narcotic effects, such as diminution of the pulse and of the respirations, and palsy of the extremities, are less marked than in pulsatilla, while the acrid effects, such as inflammation of the stomach and bowels and hyperæmia of the kidneys, preponderate. 4. The resin from the ranunculus sceleratus is almost inert, and produces only a slight increase of diuresis. 5. Pulsatilla pratensis probably belongs to the class of narcotico-acrids, since it acts as an irritant to the skin, the gastro-intestinal tube, and the kidneys, and paralyzes the medulla oblongata, the spinal cord, and the sympathetic system. 6. The narcotic operation (stupor, palsy of the extremities, slow pulse and breathing,) depends upon the anemonin, and the irritant effects on the skin, intestinal canal, and kidneys, depend upon two acrid principles one of which is an acid, fixed resin, and the other is a volatile oil, analogous in every respect to oil of ranunculus. 7. Like the oil of ranunculus, the oil of pulsatilla is decomposed in close vessels into anemonic acid and anemonin. The plant loses by drying that part of its efficacy which depends upon the presence of the volatile oil, but it retains its narcotic powers, which depend upon anemonin, and a part of its acridity due to the acid resin. 8. The freshly-pressed juice of the pulsatilla and of the ranunculus sceleratus combines all the narcotic and acrid powers of both plants. 9. The two plants now examined are very analogous to one another in respect to the quality of their active principles, since they both contain anemonin, volatile oil and a resin. In the ranunculus sceleratus, the irritant principle prevails over the narcotic principle, but in pulsatilla pratensis, the narcotic prevails over the irritant, and this difference is due to the varying proportion of anemonin in the two plants.

V. *On the Uses of Sesquichloride of Iron in Medicine.* By Dr. Pleischl. (Zeitschrift der Gesellschaft der Aerzte zu Wien, July 10, 1858.)

Dr. Pleischl points out the use of the sesquichloride of iron in some cases in which it has not hitherto been generally prescribed. There are two preparations of this salt in the Austrian Pharmacopœia—namely, the crystalline sesquichloride, and the solution called oleum martis. The therapeutical operation of these preparations is astringent and styptic, and they are therefore applicable in cases of hæmorrhage and profuse secretions in the different passages, especially in the intestinal canal. Dr. Pleischl relates several cases in support of the efficacy of sesquichloride of iron in these affections.

In a case of Hæmoptysis in a young man, the attacks had been treated by alum, acetate of lead, and ice, without effect; but the hæmorrhage was speedily arrested by the administration of a mixture containing sesquichloride of iron. A second case was one of Hæmatemesis, in which the patient had lost from ten to twelve pounds of blood in three days. In this instance acetate of lead, laudanum, alum, and the internal and external use of ice, had been tried in vain; but on employing the mixture of sesquichloride of iron the bleeding ceased, and the patient recovered. The cause of the bleeding in this case was subsequently ascertained to be a granular liver. A third case was one of chronic diarrhœa in a young girl, to whom calumba, opium, and acetate of lead, had been administered without success; but after the employment of sesquichloride of iron the evacuations soon became more consistent, and in a few days the patient was completely restored to health. Another case was one of hæmorrhage from the intestinal canal, occurring after an attack of fever. There were fæcal accumulations in the lower bowel, and when these were removed with difficulty, a profuse hæmorrhage followed, and threatened to carry off the patient. The sesquichloride of iron was employed both internally and in the form of injection into the rectum, and under this treatment the bleeding ceased, and a cure resulted. As an injection, the sesquichloride has also been employed with much success in cases of uterine hæmorrhage from cancer and fibrous tumours of the uterus, and in one case of leucorrhœa, after solutions of alum and sulphate of zinc had been injected without effect, the employment of sesquichloride of iron as an injection was attended with a notable diminution of the secretion.

The sesquichloride of iron has the property of being easily absorbed and assimilated in the system, and its use is not attended, as often happens in the case of lead, with symptoms of poisoning; and moreover, in the sequelæ of hæmorrhages, the use of iron is attended with the best results in the removal of anæmia.

VI. *On the Therapeutical Action of Perchloride of Iron in the Treatment of Acute and Chronic Urethritis.* By M. Barudel. (Bulletin Général de Thérapeutique, May 15, 1858.)

Dr. Barudel, in his investigations upon the therapeutical employment of the perchloride of iron, found that this agent exercised a special influence on the genito-urinary mucous membranes, for, from the period when it was administered internally, the inflamed membranes were rapidly affected, and no longer secreted the abundant puriform fluids which accompany the inflammations of the genito-urinary tract. The perchloride appears to owe its efficacy to a double action, for it possesses a hæmostatic power, which retains the blood in the capillary vessels, and opposes its escape, and also a sedative power which rapidly restores the functions of the capillary circulation to a regular mode of action. When the perchloride of iron was administered internally, Dr. Barudel found that it lowered the pulse from seventy to sixty, and even to fifty, in the minute, and he therefore was induced to employ it in many cases where it was essential to reduce vascular excitement. The duration of urethritis was certainly shortened by this medicine, and cases which ordinarily required a period of thirty or forty days for their cure were successfully terminated in eight or twelve under the use of the perchloride, which was administered internally in cases of acute and chronic blennorrhagia, and was also employed in the form of a styptic injection to the affected membrane. The employment of this treatment has never been attended, in the hands of Dr. Barudel, with any unfavourable results, so as to induce him to discontinue it, although, as is well known, a multitude of serious complications often spring up in the course of blennorrhagia.

In all the cases treated by Dr. Barudel, the treatment was identical, except in some rare instances. In acute urethritis, an injection of iodide of lead was employed, this salt being almost insoluble and exciting no pain in the inflamed mucous membrane, the solution of perchloride of iron in injections was reserved for chronic blennorrhagia, the constriction which it exercises upon the urethral canal rendering it more applicable to the chronic than the acute affections. The internal treatment of all the cases consisted exclusively in the administration of a mixture containing twenty drops of tincture of perchloride of iron, taken every two hours. In general, at the end of three days a certain improvement was effected; but no aggravation of the inflammatory condition of the parts was ever produced by this internal and external treatment, either in the acute or the chronic cases. The cure was almost always completed in fifteen days. In chronic urethritis, the only modification consisted in the use of an injection made of tincture of perchloride of iron and distilled water; this injection was repeated three times a day, taking care that the fluid should remain at least ten minutes in the urethra. The general treatment was of a strengthening nature, wine being allowed to the patients. Dr. Barudel quotes two cases, successfully treated in the military hospital at Lyons by the means just described, and he draws the following conclusions from his researches: 1. That perchloride of iron may be used successfully, both externally and internally, in the treatment of acute and chronic blennorrhagia; 2. That this agent is endowed with well-ascertained hæmostatic properties, and possesses also a sedative action on the general circulation; 3. That several venereal affections, which resist ordinary remedies, may be safely treated by the perchloride of iron; 4. That the perchloride, given both in acute and chronic cases, appears to act, in the great majority, as a specific; 5. That the perchloride of iron is sufficient in the treatment of simple urethritis; but that when the syphilitic element is associated with this disease, mercurials must likewise be employed; and, 6. That the most important auxiliary to the internal use of iron is the employment of an injection, the iodide of lead being applicable to acute, and the perchloride of iron to chronic, urethritis.

VII. *On the Inhalation of Carbonic Acid in Granular Pharyngitis.* By Dr. Willemin, of Vichy. (L'Union Médicale, July 15, 1858.)

The therapeutical application of the inhalation of carbonic acid has been made only in late years and principally in Germany. Bischoff and Ennemoser have made some experiments with carbonic acid, and have come to the conclusion, that when the gas is not breathed pure, nor for too long a time, it produces no unpleasant effects; but, on the contrary, that the respiration of the patients becomes more easy, and the expectoration more abundant. Germany possesses, in the present day, a great number of establishments where the inhalation of carbonic acid, more or less mixed with air or steam, is methodically practised. The gas is collected by means of an apparatus to which a caoutchouc tube is adapted, permitting the gas to be directed into the interior of the mouth, on the neck, or on any other part. At Pyrmont, a bell-glass has been placed over the spring from which the gas is disengaged, and three tubes carry away the carbonic acid. At Franzensbad a large basin has been constructed, and the patients descend into it by several steps, at the bottom of which the gas is disengaged. The superior stratum of air contains fifteen per cent. of carbonic acid; and in proportion as the patients are accustomed to this atmosphere, they approach nearer to the orifice from which the gas escapes.

The principal effect of the use of this gas, according to the German physicians, is to increase the activity of the circulation, and to depress the functions of the nervous system. The indications for this mode of treatment are thus drawn up by Lersch: it is especially serviceable in cases of dyspnœa dependent on the accumulation of mucous secretion in the pulmonary vesicles, or on emphysema of the lung. Helfft considers that this gas exercises a stimulant action upon the respiratory tract, and he recommends its use in chronic inflammation of the larynx and of the pharynx, in bronchial catarrh, and humid asthma. M. Goin recommends its use in nervous affections of the respiratory passages, as in asthma, and also in intermittent fever. The contra-indications of this inhalation are said to exist whenever any affection of the respiratory passages is accompanied by excitement of the circulating system. The German physicians unanimously forbid this treatment in phthisical cases. According to Gräfe, it is absolutely injurious when cavities are formed, these being often surrounded by a zone of inflammation. Under the influence of the carbonic acid inhalations the expectoration is diminished, and the fetor of the secretions is corrected, but the inflammation of the pulmonary parenchyma is augmented. Its use must also be avoided in hæmoptysis.

Dr. Willemin draws the following conclusions upon the inhalation of carbonic acid—namely, that when mixed with air, this gas produces a more or less active excitement of the respiratory passages, the action being analogous to that of the same gas upon the skin, the eye, &c., and this effect ought to contra-indicate its use whenever there exists a disposition to acute inflammation.

That the excitement is followed by a sedative effect, which appears to depend on a special action of this gas on the nervous system, the respiration becomes easier, the cough is allayed, the circulation is retarded, and the redness of chronic inflammation diminishes.

That continued for a longer time, or made with a larger proportion of gas, these inhalations cause vertigo, relaxation of the limbs, and anæsthesia.

That, in addition to this general action, the gas possesses an anæsthetic power over wounds and parts denuded of epidermis.

That these inhalations are especially efficacious in chronic inflammations with atony of the mucous membrane and exaggerated secretion, and in nervous affections of the respiratory passages; but that they ought to be avoided in phthisical cases.

VIII. *On the Caustic Charcoal and its Chief Applications, especially in Diseases of the Uterus.* (Bulletin Général de Thérapeutique, May 15, 1858.)

M. Bonnafond has given directions for making a kind of stick of caustic charcoal, calculated to replace red-hot iron in all the cases in which it is deemed advisable to employ superficial cauterization. This physician has obtained some very favourable results from this agent in the treatment of shallow ulcers, such as simple or phagedenic chancres, and in making punctated cauterization in cervical adenitis. This caustic charcoal can be applied only on parts which are dry and well wiped; but on moist tissues, or those which require the disengagement of much heat, the charcoal is inferior in its application to red-hot iron. Applied upon the skin, and left in contact with it for a variable time, according to the depth which it is desirable to give to the eschar, a slight crackling sound indicates the combustion of the epidermis, and there remains an eschar of a yellowish-white colour. M. Bonnafond's caustic consists of a mixture of gum dragon, powdered vegetable charcoal, and nitrate of potash; these materials are dissolved into a paste, which is rolled into cylinders, and afterwards dried for use. These caustic crayons are used by lighting them with a match until the charcoal is partially inflamed, and a crayon of greater or less diameter is employed, in proportion as a shallow or very superficial cauterization is required. The most useful application of these caustic crayons is made in the treatment of engorgement and granular ulceration of the neck of the uterus. The crayon possesses this advantage over red-hot iron, that it spares women the sight of the preparations for the actual cautery, while its effects are equally powerful with those obtained by the metal.

IX. *On a Nervous Cough, probably caused by Chorea of the Diaphragm, cured by Tartarized Antimony in Large Doses.* By Dr. Notta. (L'Union Médicale, July 15, 1858.)

The patient was a girl, aged seventeen, who had suffered for three years from a short, dry, and almost continual cough. She had been bled without relief. The chest was perfectly free from disease, as was ascertained by percussion and auscultation, and the uvula was well formed. Belladonna was given with considerable advantage to the patient, but the cough returned without appreciable cause. When she was examined in June, 1858, she was suffering from constant cough in the daytime, but she slept well at night; the throat, tonsils, and uvula were all healthy; there was no fever; she was well developed and stout; the chest was healthy, menstruation regular, appetite good. Belladonna was again prescribed, but without any good effect. A mixture was then ordered, containing 25 centigrammes (about 5 grains) of tartarized antimony in 115 grammes (about 3½ ounces) of water, a tablespoonful to be taken every hour. The mixture caused vomiting, but the cough ceased entirely. The dose of the tartar emetic was subsequently increased to 40 and 50 centigrammes (8 or 10 grains) in the same mixture, and under this treatment the patient completely recovered.

Dr. Notta considers that the cough was produced, in this case, by a spasm of the diaphragm, resembling chorea in its nature. But whatever may have been the exact cause of the cough, he considers that the case proves the antispasmodic power of the tartarized antimony in large doses, and he recommends physicians to make experiments with this drug in other nervous affections, besides chorea, in which it has succeeded so remarkably.

X. *On the Anæsthesia produced by the Passage of an Electric Current during the Extraction of Teeth.* (L'Union Médicale, October 14, 1858.)

At a recent meeting of the Académie Impériale de Médecine M. Robert related the results of some experiments made at the Hôtel Dieu, in reference to the anæsthesia produced by the passage of an electric current during the extraction of teeth. One of the poles of the battery was placed in the hands of the patient, and the other was placed in contact with the instrument which was to seize the tooth, and the instrument itself was isolated from the hand of the operator by a wrapper of silk. Of ten cases, two were completely negative, for there was as much pain, if not more, than if the electrical apparatus had not been employed. Four cases were favourable, and the patients did not complain of any painful sensation; but it is doubtful whether there was true anæsthesia in these cases, or whether the action of the electricity merely masked the pain. It is certain that a sudden surprise may prevent the pain of a slight operation from being felt. A box on the ear, for instance, given to a patient at the moment of opening an abscess will mask the pain of the knife. Of the whole ten cases, two were negative in their results, four were favourable, two were doubtful, and the remaining two were inconclusive, as the conditions of the experiment were changed. "Allow me to add," continued M. Robert, "that I have tried the electrical current in other small operations. Having occasion to open a collection of matter in the palm of the hand, I applied one of the poles of the battery along the course of the ulnar nerve, and the other in contact with the bistoury; the pain was more acute than it would have been in ordinary circumstances, and the painful shock felt all along the nerve was superadded to the pain of the puncture." M. Moreau spoke in favour of the anæsthetic operation of electricity in tooth-drawing, but M. Velpeau stated his opinion as follows. Many of the patients under the care of that surgeon were subjected to the influence of an electrical current during the performance of some minor surgical operations. One of these patients, who had previously had an abscess opened, and who said nothing, not because he felt nothing, but because be was courageous, complained of a second opening made under the influence of electricity, and declared that he not only felt the pain of the bistoury, but also the pain of the electric current, in his hand. A second patient was operated upon for a cancroid growth on his nose, and felt nothing. A third suffered much during the operation for an ingrowing toe-nail, which was performed for the second time; he declared that he suffered more than on the first occasion without electricity. A fourth complained of pain during the opening of an abscess, and said that he suffered at least quite as much as without electricity. A fifth, on whom a hydrocele was punctured, complained more than patients generally do under that operation; and the same was the case with a sixth, who had an abscess opened. "I conclude from these facts," says M. Velpeau, "and from others observed at the Hôtel Dieu, the Hôpital Neckar, and elsewhere, that there is no anæsthesia produced, and that there is some peculiarity in the American teeth in which they differ from the French teeth."

XI. *Notes on some of the Principal Indigenous Tonics of India.* By EDWARD J. WARING, Esq., of the Madras Medical Service. (Indian Annals of Medical Science, July, 1858.)

In a very able paper, Mr. Waring points out a number of the indigenous productions of India which are endowed with tonic properties, and which might advantageously be substituted for imported medicines of the same class. Some of the most valuable of these products can be procured for the mere expense of collection, as for example, the Colotropis gigantea, the Hemidesmus indicus, the Hydrocotyle asiatica, the Cocculus cordifolius, &c., which grow wild in the immediate vicinity of the large cities, and other medicinal herbs may be procured in most of the bazaars.

Mr. Waring divides the tonics into three classes—the alterative, the bitter, and the astringent tonics.

Alterative Tonics.—Of all the alterative tonics, sarsaparilla has long held the highest position, and no better proof of its popularity can be adduced than the fact that above 100,000 lbs. weight of the article are supposed to be annually consumed in Great Britain alone. The chief supply is derived from the Western Hemisphere; Jamaica, Honduras, the Brazils, and Vera Cruz, being the principal localities from which it is supplied, and the species which yield it being principally Smilax officinalis, S. medica, and S. papyracea. But several species of smilax are indigenous in India, the plants being found all along the foot of the Himalayas, along the Malayan peninsula, and one species being found in Ceylon, namely, the S. Zeylanica. The number of Indian species of smilax is large, the principal being the S. ovalifolia, S. prolifera, S. lancifolia, S. glabra, S. roxburghiana, S. oxyphylla, S. rigida, S. ferox, and others. The Indians generally do not attach much value to the medicinal effects of the roots of the smilax, probably owing to the absence of the sensible properties of bitterness and odour, to which the natives attribute the chief activity of vegetable products. Some exceptions, however, are found, and the roots of some species of smilax are employed by the natives of some parts of India in the cure of sores and syphilitic complaints. Mr. Waring has found the roots of the S. prolifera highly valuable as an alterative, mild tonic, and diaphoretic; it was very advantageously employed in chronic rheumatism, especially when connected with a syphilitic taint, in general cachexia, in the coughs of old age, and in some obstinate cutaneous affections. Under its use the patient rapidly gains strength and flesh, the appetite increases, the health improves, old cutaneous affections heal, and rheumatic affections disappear. It was given in some cases in combination with nitric acid or iodide of potassium; in others, it was given warm in decoction, with milk and sugar, when it can be distinguished with difficulty from ordinary tea. It should be used fresh, and it can always be had for the expense of collecting. The Smilax china was introduced into European practice in 1535, and is indigenous to China, Cochin China, and Japan. It was especially lauded as a remedy in syphilitic affections, but it afterwards fell into disrepute. It appears that this root really possesses some valuable properties, but it should be used when fresh and sound, and not in the old, worm-eaten state in which it is mostly found in the present day in the Indian bazaars.

As a substitute for sarsaparilla, the natives of India employ the roots of several plants, as for instance, the Ichnocarpus frutescens and the Spermacoce hispida, the first belonging to the natural order of Apocynaceæ, and the second to the Rubiaceæ. Both these roots appear to possess alterative and tonic properties. The fragrant roots, which are known under the name of "country sarsaparilla," are obtained from a small twining or climbing milky plant, the Hemidesmus indicus, belonging to the natural order of Asclepiadaceæ, being the same as the Periploca indica and the Asclepias pseudosarsa of former botanists. Mr. Waring considers this root as one of the most generally useful tonics and alteratives in India, and generally superior in efficacy to the sarsaparilla imported from Europe. In all cachectic affections, constitutional syphilis, and some of the chronic forms of rheumatism, and in debility after fevers, this remedy may be advantageously employed, and in the words of the 'Bengal Pharmacopœia,'—"it is in every respect a perfect substitute for sarsaparilla." The efficacy of the remedy appears to depend upon a crystallizable principle called hemidesmine. In the same natural order is another plant, or rather family of plants, which possess alterative properties—namely, the Calotropis gigantea, C. procera, and C. Hamiltonii. The parts of these plants which are used, are the root, the bark of the root, and the inspissated milky juice. Two other plants, indigenous to India, have also attracted considerable attention as remedies in leprosy, scrofula, and secondary syphilis—viz., the Hydnocarpus odoratus or Chaulmoogra, belonging to the natural order of Pangiaceæ, and the Hydrocotyle asiatica, of the natural order of Umbelliferæ. The former, or Chaulmoogra, is a tree indigenous in Sylhet, Assam, Upper Bengal, and in China; the Hydrocotile asiatica is a small, low-growing plant, common in moist localities throughout the peninsula of India, and extending to Assam, Japan, Hong-Kong, Java, Ceylon, Mauritius, and the Cape of Good Hope. Both these plants are undistinguished by any uniform, well-marked, or sensible effect on any of the functions, and the evidence of their operation consists in producing a gradual restoration of the health, tone, and vigour of the system generally; and it may be presumed that they both act by correcting an abnormal or diseased state of the blood, perhaps acting primarily on the digestive organs. Mr. Waring's experience warrants him in stating, that the Hydrocotyle asiatica is surpassed by few remedies, not even excepting the iodide of potassium, in the treatment of secondary and constitutional syphilis, and in chronic cutaneous diseases. Among other alterative tonics indigenous to India, and deserving of notice, are the sassafras of Nepal, the Camphora glandulifera, together with the wood of the Camphora parthenoxylon; the Cacali kleinia, the fresh leaves of which are given in decoction in the class of cases for which sarsaparilla is usually employed; the Bryonia pigæa, the roots of which are used by the natives as alterative; the Chenopodium album, the leaves of which are said to be tonic and alterative; the Dalbergia sissoo, the wood of which is used in leprosy, constitutional syphilis, and many blood diseases; the Euphorbia tirucalli, the dried inspissated juice of which is given in syphilis; and the Plumbago rosea, the dried root of which is administered in secondary syphilis.

Bitter Tonics.—The two natural orders, Gentianaceæ and Menispermaceæ, comprise the largest number as well as the most valuable of the bitter tonics employed in medicine; of the natural order of Gentianaceæ it is computed that about sixty-eight species are to be found in the East Indies. The most important of these is the Ophelia chiretta, which yields the well-known chiretta of commerce. It is a valuable bitter and tonic, and an excellent substitute for gentian, and may be procured at a cheap rate in almost all the bazaars of India. Dr. Wallich pronounces it one of the most valuable medicines which the materia medica of India possesses. According to Professor Royle, another plant belonging to the natural order of Gentianaceæ yields a variety of chiretta—namely, the Exacum tetragonum; and bitter tonic properties are also said to exist in the Exacum bicolor, Ophelia elegans, and Adenema hyssopifolia, all belonging to the gentian family. Another Indian species of gentian deserving of notice is the Gentiana kurroo, the roots of which are used in the same way as common gentian, and another plant of the same order, the Chironia centaurioides. As a substitute for gentian, the natives frequently employ the bitter root of Picrorrhiza kurroo, belonging to the natural order of Scrofulariaceæ. Of the Menispermaceous plants growing in India, the most important, perhaps, is the Cocculus cordifolius, a twining shrub, common over the greater part of India.

The root, stem, and leaves of this plant abound in bitterness, and hold a high place among the indigenous tonics of the East. It also acts mildly as a diuretic, and is deemed febrifuge by the natives. Another twining shrub of the order of Menispermaceæ, deserving attention as a bitter tonic, is the Coscinum fenestratum, indigenous in Ceylon and the southern portions of the Indian Peninsula. It has been recently ascertained that this plant contains a considerable portion of berberine. Two other plants of this order, possessing bitter and tonic properties, are the Clypæa hernandifolia and the C. burmanii. Closely allied to the Menispermaceæ, both in a botanical and therapeutic sense, is the natural order of Berberaceæ, the Indian species of which have latterly attracted considerable attention for their tonic and antiperiodic properties. Of these the Berberis asiatica, B. lycium, and B. aristata, are considered the most active. The parts employed are the wood, the bark of the root, and an extract prepared from these. The natural order of Simarubaceæ contains but few species indigenous to India, the most important being the Nima quassioides. Among other plants belonging to other natural orders, are the Guilandina bonducella, which is a prickly shrub, yielding a bitter fruit; the Andrographis paniculata, which is an excellent substitute for quassia, gentian, and other imported bitters, and is a pure tonic; the Coptis tecta, the roots of which are employed as a bitter and tonic remedy; the Thalictrum foliosum, the root of which is tonic and febrifuge; the Aconitum heterophyllum, the root of which is used as a general tonic; the Aristolochia indica, the root of which is bitter and tonic; and the Azadarichta indica, the margosa or neem tree, which is inferior to none of the preceding plants as a bitter tonic and antiperiodic.

Astringent Tonics.—Without even attempting to describe the indigenous astringents of India, Mr. Waring directs attention to a few indigenous plants, which, while they possess more or less astringency, are classed among the general or constitutional tonics. The natural order of Cedrelaceæ contains two trees belonging to this class—namely, the Soymida febrifuga and the Cedrela toona, the barks of both of which are astringent and tonic. The natural order of Apocynaceæ contains some trees, the barks of which rank high among the astringent tonics of India. Among these is the Wrightia antidysenterica, the bark and seeds of which possess the same properties; the Alstonea scholaris, the bark of which has for a long period been esteemed for its astringent and tonic properties by the natives of some parts of India, and has also been reported to act as an anthelmintic; and lastly, the Hymenodictyon excelsum, a tree belonging to the natural order of cinchonaceæ, which, from the physical properties of its bark, may be confidently expected to prove a valuable astringent, tonic, and antiperiodic.

XII. *On the Employment of Instillations of a Tepid and rather Concentrated Solution of Chlorate of Soda into the Trachea of Children affected with Croup.* By M. Barthez, Physician to the Hôpital Ste. Eugénie. (Bulletin Général de Thérapeutique, May 30, 1858.)

In a letter written to the above journal, M. Barthez calls attention to some cases in which he has employed local applications to the false membranes produced in croup. For about two months, an epidemic of croup had prevailed near the Hôpital Ste. Eugénie, and had assumed some peculiar characters, being attended with the production of a false membrane in the bronchi, trachea, and larynx, seldom in the pharynx, and hardly ever in the nasal fossæ. The general symptoms of blood-poisoning were generally absent, and after tracheotomy, the children died with the symptoms of slow asphyxia; they all died, and in the greater part of them, the trachea and the bronchi were filled with false membranes.

M. Barthez had previously made some experiments on the comparative effects of chlorate of soda and chlorate of potash, when these salts are applied to the false membranes. Two portions of false membrane, of nearly equal dimensions, were placed in contact respectively with a concentrated solution of chlorate of potash, and a concentrated solution of chlorate of soda. The false membranes were gradually altered in character: they softened and lost their opacity; their tissue became less compact, more transparent, and afterwards diffluent, and their membranous form disappeared without losing all its cohesiveness. The only difference observable in the two solutions was that the changes began later in the chlorate of potash than in the chlorate of soda, and required a much longer time for their completion. The same membrane immersed in water only, preserved its natural appearance for many days.

Guided by these experiments, M. Barthez instilled through the canula a tepid solution of chlorate of soda in some cases of tracheotomy, in the hope that he might thus effect the softening of the false membranes, and consequently the more easy destruction of their adhesions, and their more rapid and complete expulsion. The results of these experiments were very encouraging, for all the patients previously treated had died, but when the new system had been introduced, three out of seven patients recovered. Judging from this result, M. Barthez is convinced that the instillation is innocuous, and that it is even useful. In order to ascertain the different effects produced by solution of chlorate of soda and by pure water, a patient was treated by instillations of water only, but although the instillation was very frequently repeated and cough was excited, yet no other liquid but water was thrown out. At last, solution of chlorate of potash was substituted for the water, and it was instilled every quarter of an hour; and at the end of about an hour a remnant of false membrane was evacuated, and then other portions in succession until the next day. The suffocation diminished in proportion, and the patient recovered completely.

M. Barthez relates five cases in which the chlorate of soda was employed in instillations in the trachea; and although he admits that the cases are not sufficient to establish incontestably the efficacy of this treatment, yet he thinks that the beneficial action of the soda-salt is very probable. Tracheotomy ought not to be performed unless there is reason to suppose that a false membrane exists in the trachea; even when the false membrane exists, it is useless to employ instillations when there are symptoms of diphtheric poisoning, or when the expectoration of false membranes is abundant; but when tracheotomy has been performed, and it is determined to use instillations, then the chlorate of soda appears to assist the child in discharging the false membranes which oppose the entrance of air into the lungs.

XIII. *On the Employment of Oil of Turpentine and Opium in Large Doses in the Treatment of Severe Puerperal Diseases.* By Dr. E. Bonfils. (Bulletin Général de Thérapeutique, May 30, 1858.)

M. Trousseau has lately employed, with considerable success, a method of treatment, proposed originally by Dr. Graves, in puerperal diseases. This treatment consists in giving opium and oil of turpentine in large doses to women in child-bed who are attacked with metro-ovaritis, peritonitis, uterine phlebitis, &c. Among other cases, M. Trousseau has treated in this manner, and with success, a woman attacked with peritonitis and double pleuro-pneumonia. He also employed this plan in another case of a woman attacked with general and very severe peritonitis, which was very rapidly checked and afterwards cured; but although the cure appeared to be permanent, the patient was unfortunately seized with hectic symptoms of an insidious character, and sunk under what appeared to be a putrid infection. In the first case the opium was prescribed in pills and the turpentine in injections. At first five centigrammes (about one grain) of opium were given in five pills, to be taken daily; then the dose was gradually augmented till it reached about two grains a-day. The opium was continued for thirteeen days. The turpentine was administered at first in the dose of ten grammes (about two drachms and a half) in two glysters, one in the morning and the other in the evening; then the quantity was progressively augmented to thirty grammes (about seven drachms and a half). In the second case the opium was also given in pills, in the dose of five centigrammes (about one grain) for three days. The oil of turpentine was administered by the mouth in capsules, each containing one gramme (about the fourth of a drachm) of turpentine; six of these capsules were taken every day, and they were continued for six days.

XIV. *On the Treatment of Inflammation by Digital Compression.* By M. Vanzetti, Professor of Clinical Surgery in the University of Padua. (Giornale Veneto di Scienze Mediche, April, 1858.)

From the success which has attended the treatment of aneurisms by manual compression of the arterial trunk, M. Vanzetti was induced to apply the same method to the treatment of inflammations, in those cases where the artery leading to an inflamed limb is accessible to the finger. He has several times had recourse to digital compression of the femoral, brachial, or subclavian artery in cases of phlegmon, arthritis, and inflammation of the fingers, and he has obtained such decided effects by this treatment, that he has adopted it in all cases where it could be practised. Although this method is of course only applicable in certain cases, yet it is found that compression will not only quickly cure incipient inflammations, but even when the inflammatory process has made some progress, it may be arrested by a patient and persevering use of the same means. This plan of digital compression had been already proposed theoretically by some authors, but it was never employed by any of them, and it had fallen into oblivion. They have also proposed the use of instruments for the purpose of compressing the artery, without at the same time interrupting the venous circulation, but M. Vanzetti recommends that manual compression should alone be used, as being preferable to all others. Although this method of compression presents some difficulties in its performance, it is only because one or two persons are sometimes required to apply it exactly; the surgeon ought, in urgent cases, to make compression himself for two or three hours; this period will sometimes be sufficient to diminish sensibly the acuteness of the inflammation, and thus to save a seriously diseased limb. Most frequently the patient can himself perform the compression of the femoral, or the humeral, or even of the subclavian artery in cases of very considerable swelling of the arm; he will be able very easily to continue the compression for eight or ten minutes, and then to leave off and re-commence after he has rested. These short intervals present no obstacle to the desired effect.

M. Vanzetti records two cases treated by digital compression in the hospital at Padua. The first was a severe case of phlegmonous erysipelas of the left arm, and the second was one of acute arthritis of the right wrist. In the first case the limb was enormously swollen, and a thread was placed around it to measure any change in its dimensions; digital compression was exercised on the subclavian artery for fifteen continuous hours, after which, there was decided relief of the disease and diminution of the swelling, and although there was subsequently extensive suppuration, yet the patient recovered completely. M. Vanzetti thinks that suppuration might have been prevented if the patient had come earlier under treatment, as he was advised to do. The second case was one of arthritis, and was treated by compression of the brachial artery, which was performed sometimes by the pupils of the hospital, and sometimes by convalescent patients, properly instructed. The pain and swelling of the joint were distinctly relieved, and the patient entirely recovered without the adoption of any other treatment whatever. In this case, the patient was able to distinguish whether the compression was properly or improperly practised by the amount of relief which he experienced.

XV. *Veratrum Viride as an Arterial Sedative:* a Mutual Paper made up of Contributions from the Members of the Middlesex East District Medical Society, Massachusetts. (American Journal of the Medical Sciences, October, 1858.)

This plant was introduced to notice as a medicine in the year 1835, and in 1856 the Middlesex East District Medical Society in Massachusetts had its attention invited to its properties. Since that time it has been constantly in use, with results which are believed to be very important, its chief employment being as an arterial sedative in most inflammatory affections. This species of veratrum grows rather abundantly in swamps, moist meadows, open woods, and along the banks of mountain streamlets from Canada to Georgia, putting forth its leaves in April and beginning to flower at the end of May. The officinal part is the root, which has a sweetish bitter taste, and produces a burning sensation in the mouth, on the tongue, and in the fauces and throat, together with a sensation of dryness and heat. When dried and powdered it acts as a powerful sternutatory, and applied locally it causes irritation, rubefaction, and even vesication of the skin. In relation to its chemical composition, it is found to contain gum, starch, sugar, bitter extractive, fixed oily matter, colouring matter, gallic acid, an alkaloid substance identical with veratria, lignin, and the salts of lime and potassa. The alkaloid principle is nearly insoluble in water, more soluble in ether, and entirely soluble in absolute alcohol. The tincture is the preparation employed in practice. It is made by slicing the root, and drying the pieces over a furnace, and then triturating them, after which the powder is macerated with strong spirit of wine. The proportions employed are about four ounces of the powdered root to a pint of spirit. This tincture has been employed successfully as an arterial sedative; the dose is one-half to two drops for infants, two to five drops for children, and three to ten drops for adults. The first indication of a sufficient quantity having been given is the occurrence of nausea and diaphoresis, and then the dose must be diminished, or the use of the medicine suspended for a time. Although the *veratrum album* is a drastic cathartic, experience has shown that the *veratrum viride* very seldom, if ever, purges. The evidence brought before the Massachusetts Medical Society shows that the tincture of veratrum viride possesses great efficacy in the treatment of diseases attended with excitement of the circulatory system, as, for instance, in inflammatory affections and palpitation of the heart. It has been used in pneumonia, pleurisy, erysipelas, and scarlatina, and its use is always attended with a diminution in the frequency of the pulse. In acute rheumatism it is reported as being a medicine of great efficacy. Thirty-four cases are recorded as having been treated by this medicine, including cases of palpitation of the heart, organic disease of the heart, puerperal fever, pneumonia, pleurisy, bronchitis, acute rheumatism, intermittent fever, and scarlatina, and in all the cases its employment appears to have been attended with beneficial results.

Dr. John Bell, in a Report on Materia Medica for the year 1857, published in the 'North-

American Medico-Chirurgical Review' for September, 1858, thus sums up his opinion upon the effects of the veratrum viride: "After this survey of the different uses to which veratrum viride has been put in the treatment of disease, it is easy to see that much yet remains to be known concerning its therapeutical capabilities. So far, its use would seem to have been confined to febrile and inflammatory diseases; but, if we can attach any importance to the analogy which this substance bears to the class of sedatives belonging to the vegetable kingdom, we must believe that its greater range and variety of application will be found in subacute and chronic affections, in which the nervous system and nutrition are implicated,—as in the neuroses, and in scrofula and the secondary stages of syphilis, as well as in chronic rheumatism and gout. May it not deserve, in various cardiac affections, especially in hypertrophy, the epithet applied to digitalis, of its being the opium of the heart? Beyond the sedative action of veratrum viride, we have very little accurate knowledge of its action on the organism. Experiments, both physiological and clinical, are wanting to show to what extent it acts as a diaphoretic or expectorant, although these properties are claimed for it by its eulogists. The natural tendency of a febrile paroxysm to end in sweating, and of pneumonia to relieve itself, after a few days, by copious expectoration, should make us slow to admit the efficacy of an agent as a diaphoretic or an expectorant in these diseases, merely because diaphoresis and expectoration follow its use. The operation of veratrum viride in either of these ways has yet to be tested by its trial in a much wider circle of disease, and with more guards against fallacies than have yet been had recourse to. We have heard nothing of its action on the kidneys; and in this respect it differs greatly from digitalis. As an adjuvant and corrigendum to purgatives, especially to those of the resinous and drastic order, much good might be expected from veratrum viride, which in this, as well as in some other particulars, will be found to resemble hyoscyamine."

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

By Edward H. Sieveking, M.D.

Fellow of the Royal College of Physicians, Physician to, and Lecturer on Materia Medica at, St. Mary's Hospital.

I. *Clinical Illustrations of the Pathology and Treatment of Delirium Tremens.* By T. Laycock, M.D., &c. (Edinburgh Medical Journal, Oct. 1858.)

This paper illustrates by precept and example the error of the system very commonly, though not universally, pursued of treating delirium tremens by the administration of spirits and opium. Dr. Laycock argues that the withdrawal of the accustomed stimulus in habitual drunkards is not the ordinary cause of an attack, but that it is commonly brought on by a prolonged debauch, giving rise to an intensely alcoholized state of the blood. His own cases certainly bear out this view; in only one of twenty-two cases treated by the author in the Edinburgh Infirmary during the past summer was there any shadow of evidence of the delirium having been due to a withdrawal of the alcohol; in all the others the patients, when admitted, were under its immediate influence. If the disease be due to the blood being overcharged with alcohol, Dr. Laycock argues that it would be illogical to increase the force of the disease by multiplying the cause, and in support of this theory brings forward statistics, furnished by his own experience and that of other observers, to prove positively the advantage of the entire absence of alcoholic stimuli in delirium tremens, and, negatively, the pernicious effect that results from their administration. Of 403 cases treated in 8¼ years in the Royal Infirmary, according to the routine system, with spirits and opium, 101, or 25 per cent., died; of Dr. Laycock's 24 cases, treated in the Infirmary, none died, and he quotes Dr. Peddie's method of treatment without opium as equally successful, 80 patients who were under that gentleman's care having all recovered. With reference to the usual argument in favour of narcotics in delirium tremens, drawn from the patient's sleeplessness, Dr. Laycock states that experience abundantly shows that sleep, and "therewith return to health, will come on naturally in delirium tremens without the use of any narcotics, or even any drugs whatsoever." In addition to his own cases, he refers to the experience of Kühn, Esquirol, Calmeil, and Ware. The latter "treated 29 cases on the expectant method, 1 died; 12 by emetics, 1 died; 8 with opium, 4 died."

The state of the patient being one of great excitement, he should be placed in circumstances favouring repose, and the elimination of the alcoholic poison by the natural emunctories be promoted; food is necessary, because in almost all cases the absence of proper food has been one of the causes of the supervention of the seizure, and the complications, which mainly consist in sub-inflammatory states of the stomach, duodenum, liver, or kidneys, should be attacked by mild sedatives and depurants. The chronic inflammation of the chylopoietic viscera, so commonly associated with delirium tremens, is treated by Dr. Laycock with small doses of calomel, nitrate of silver, and morphia, combined in the form of pill; but where no such disorder exists, he finds the hygienic treatment sufficient to restore the balance of the circulating and nervous systems, and sleep ensues without the administration of medicinal narcotics.

II. *Is the Chorea of Abyssinia, or Tigretier, a Distinct Form of Disease?* By Dr. Le Roy de Méricourt. (Archives Générales de Médecine, Août, 1858.)

Those who have read Hecker's 'Epidemics of the Middle Ages' will remember that he classes Tigretier with the dancing mania, and gives the sanction of his authority to the description of an affection under that name by Nathaniel Pearce, an Englishman, but not a medical man, who lived in Abyssinia from 1810 to 1819. Dr. De Méricourt does not appear to have had any opportunities of witnessing the phenomena under discussion, nor to have been in Abyssinia: but, after analysing the details given by Pearce, who was not a medical man, and examining the accounts of more recent travellers in that part of the globe, especially of Lefebvre, Petit, and Dillon (two of whom were medical men) he arrives at the following conclusions:—1. There is not in Abyssinia, nor in the Tigré country, a special endemic disease which deserves a separate place in nosology under the name of Tigretier. 2. The description of Mr. Pearce does not prove that he was witness of a species of mania analogous to the dancing mania of the Middle Ages. 3. His description presents no symptom characteristic of the neurosis known as chorea. 4. It is nothing but a more or less faithful account of the superstitious practices employed in Abyssinia for the cure of diseases generally, and often doubtless used as means of fraud.

III. *Memorandum of New Facts relating to Epilepsy consecutive upon Lesions of the Spinal Cord.* By Dr. E. Brown-Séquard. (Journal de la Physiologie, Numero iii., Juillet, 1858.)

Our readers will remember that Dr. Brown-Séquard has demonstrated the occurrence of epileptiform seizures in animals in whom a horizontal section of part or the whole of the spinal cord has been made; section of the portion intervening between the seventh or eighth dorsal and third lumbar vertebra being particularly liable to induce the phenomenon: The author now adds, that having been doubtful whether in these cases there was a complete loss of consciousness, he has since found that in certain cases no signs of pain or reflex action can be elicited during the attacks—viz., when the transverse section of the spinal cord is made on a level with the last dorsal vertebræ; he therefore concludes that the convulsive disease thus produced is genuine epilepsy.

Dr. Brown-Séquard next discusses whether there is a form of epilepsy which deserves the name of spinal epilepsy, and he maintains that there is a variety of epilepsy which deserves this name, but which is distinct from that described under the term by some authors. He supports this statement by reference to experiments and to cases observed by himself and others in man. He observes that when the spinal cord has been nearly or entirely divided in the dorsal or upper lumbar region, the reflex movements that occur in the posterior paralysed portion of the body put on an alternately tetanic and epileptiform character. The two posterior extremities will become perfectly stiff, and subsequently be affected with violent, irregular clonic convulsions. These attacks last from two to eight minutes. The tetanic stiffness is sometimes so great that in attempts to overcome it forcibly, Dr. Brown-Séquard has broken the bone. The two posterior extremities are not affected in the same way at the same time; one may be in a state of rigid extension, while the other is violently agitated by clonic convulsions. If the anterior portion of the body is attacked with epileptiform convulsions, the posterior extremities are also involved in the convulsions.

Dr. Brown-Séquard, in the same paper, adverts to a third point of importance connected with the production of epilepsy, the occurrence of a contraction of the vessels of the cerebral hemispheres, and the circulation of black blood after this contraction has ceased. He intends shortly to publish new researches on these and allied points, but makes the preliminary announcement that he has seen the contraction of the cerebral vessels in epileptic animals at the commencement of a seizure, and that this contraction has been absent when the cervical sympathetic has been divided.

IV. *On the Practical Use of a Laryngeal Speculum in the Diagnosis and Treatment of Diseases of the Tongue.* By Dr. Semeleder. (Zeitschrift der k. k. Gesellsschaft. der Aerzte zu Wien, July 12, 1858.)

Garcia,[*] Czermak,[†] and others, have employed a speculum for the examination of the fauces and the neighbouring parts; a strong light is necessary, which, whether it be sunlight or artificial illumination, must be thrown on to the speculum while the observer protects his own eye from the glare by a shade, similar to that of the ophthalmoscope. The speculum is to be warmed, to prevent its being dimmed by the patient's breath, and its temperature may be tested by being applied to the observer's cheek. In patients whose velum palati is not adherent, the soft palate should be pressed firmly upwards with the mirror, in order to prevent it from slipping down behind the speculum, and also because by this means the fauces will not be irritated and reflex action excited. The patient's tongue offers numerous impediments, and some management and experience is requisite to manipulate properly. When making an observation, the patient should quietly take a deep breath, and hold it as long as possible. The slanting direction of the speculum will enable the observer to see the root of the tongue, the epiglottis, and the introitus laryngis. Within the larynx the superior thyro-arytenoid ligaments, the chordæ vocales, and through them, with a good light, the inner surface of the trachea may be seen. Dr. Semeleder agrees with Dr. Czermak, that occasionally the bifurcation of the trachea may be recognised. The amount of control which patients can exercise over different parts of the fauces varies, and accordingly the extent of surface reflected in the speculum varies also.

The case which Dr. Semeleder reports is as follows:—A girl, aged fourteen, came as an out-patient, with ulcers of the soft palate and fauces, and was cured with a solution of iodine in glycerine, applied locally. Ten weeks after, she returned, with a hard tuberculated nodule on the middle of the tongue, two lines long, covered with enlarged vessels, which in a fortnight was converted into a large ulcer, with hard, thick edges. Mercurial treatment only aggravated the disease; improvement took place on returning to iodine treatment locally and generally.

"On the 8th of April the author commenced the use of Garcia's laryngeal speculum. In the middle of the tongue was a large nodule, and in front a small one; on the right side of the nodules was a deep, ulcerated fissure, curving posteriorly towards the mesian line, and terminating three-quarters of a line behind the point of union between the anterior papillæ vallatæ, like a button-hole or loop-hole, in a hole which was three lines in diameter; from this hole asteroid fissures were given off, which were partly lost at the sides, partly disappeared behind the epiglottis. The papillæ vallatæ of the right side occupied this fissure, and could not therefore be seen. The epiglottis and epiglottic folds, as well as the chordæ vocales, were somewhat reddened."

Dr. Semeleder found that he was able to make the local application of the solution of iodine in glycerine[‡] with much more precision by the aid of the speculum, and that the curative process was thus much accelerated. Fresh granulations formed, and a gradual contraction and cicatrization of the cavity ensued.

Dr. Semeleder remarks that negative results—i.e., the proof of the parts shown by the speculum being in a normal condition—are also useful, and concludes his paper by observing that by proper manipulation the use of the speculum faucium may be continued for a considerable period at a time without giving rise to any inconvenience to the patient, whereas the awkward introduction of the instrument would at once give rise to retching in the same individual.

Since the appearance of the paper from which the foregoing extract has been made, Dr. Semeleder has, in the same journal (19th July, 1858), described an improved illuminating mirror instrument for examination of the fauces, to be worn as an eyeglass or a pair of spectacles by the observer. It is described as follows: A metallic, highly-polished concave mirror, eight centimetres in diameter horizontally, and with a focus of twenty to twenty-five centimetres, is provided with a central opening, to the posterior surface of which a metallic nut is attached, with a perforation corresponding to that in the mirror. The nut is held by two cups, which are pressed against one another by two springs given off at the back of the mirror; the nut is moveable on two axes, but is sufficiently fixed to retain any given position. A concave disc corresponding to the circumference of the eye is attached to the posterior surface of the cups, and fitted into the frame of an eyeglass or spectacles; this disc is also perforated, so that there is a continuous channel from the mirror to the eye of the observer.

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[*] See British and Foreign Medico-Chirurgical Review, Jan. 1856.

[†] Sitzungsbericht der Math. Physik. Classe der k. Academie Wissenschaften. 1858.

[‡] ℞ Iodinii gr. iij. potassii iodidi gr. xvj. glycerinæ ℥j.

V. *Remarkable Case of Aneurism of the Thoracic Aorta.* By John H. Packard, M.D. (The American Journal of Medical Sciences, July, 1858.)

This case is related by the author because, as he thinks, the sternum had perforated, and become enclosed within, the aneurismal tumour. It occurred in a coloured porter, aged thirty-eight, who in February, 1858, came under Dr. Packard's care, with a lump as large as a fœtal head at the upper part of the sternum, somewhat to the right of the median line, which had first begun to form in July, 1857. The treatment, of course, was palliative, as there was no doubt in regard to diagnosis. An eccentric or expansive pulsation was visible all over the tumour; but there was no bellows-murmur nor anything like a thrill. The clavicles seemed to disappear at about an inch and a half from their sternal extremities, but the sterno-cleido-mastoid muscles could be traced on each side, passing over the upper part of the tumour as if to its point of insertion. The man continued at his work till March, towards the end of which month a rapid increase in the tumour ensued. On the 2nd of May it measured eleven inches transversely and ten inches and a quarter from above downwards in semi-circumference. The upper part was discoloured, and on the 6th of May rupture took place externally, followed by severe hæmorrhage, and death in the ensuing night.

Autopsy ten hours and a half after death, the tumour only examined. The anterior wall of the sac was extremely thin, the skin was easily separated from it, except at the point where the sterno-cleido-mastoid were attached; their lower part was converted into fibrous tissue, and their bony connexions were destroyed. The sac contained an enormous clot, very firm behind and below, but the consistence gradually diminishing, and its red colour deepening towards the external orifice. On introducing the hand into the cavity, Dr. Packard found several pieces of roughened bone. "Two of these, evidently remnants of the sternum, lay free, surrounded and supported by half-coagulated blood; they were irregular in shape, bare, and eroded, as if by a solvent action of the blood; the line of separation between them was irregular, oblique, and much like a fracture. The end of the right clavicle, and the upper two ribs on the same side, in a similar condition, projected through the wall of the sac; both sterno-clavicular articulations were absorbed. Above the second rib the sternum was entirely gone, except a slender strip which had formed its border on the left side; the inner edge of this strip was bevelled off internally by absorption. The outer and front surface of the first rib on the right side was laid bare, and slightly hollowed out by the pressure of the aneurismal sac. It would seem that the dilatation had gone on so rapidly that the bones, so to speak, were not absorbed quickly enough, and the walls of the sac, even in the act of giving way to them, formed adhesions round them." The aorta was the vessel affected from about an inch above its origin; the trachea was flattened, and the lungs perfectly healthy.

The author has examined the records of the profession without meeting with a similar case. He explains the occurrence thus; "The periosteum covering the inner surface of the upper piece of the sternum was absorbed beneath the pressure of the tumour, adhesions meanwhile forming between the sac and the periosteum around the edges of the bone; then the anterior wall of the sac itself being broken down, the bone was loosened from its anterior periosteal layer by the insinuation of the contents of the sac between them, and dropped into the cavity, the anterior wall of which thenceforth consisted of two layers—skin and periosteum—the latter becoming lined by a sort of serous membrane, perhaps by the organization of coagula."

VI. *Ulceration of the Aorta.* By A. N. Talley, of Orangeburg, South Carolina. (Charleston Medical Journal and Review, Sept., 1858.)

In a female about fifty years of age, who was under Dr. Talley's care on account of pulmonary phthisis, and in whom death ensued suddenly and unexpectedly, the autopsy revealed, in addition to the disease in the lungs, the following conditions. The pericardium was greatly distended and of a dark chocolate hue, it was occupied by a large coagulum surrounding the heart and moulded to it. The heart itself was healthy in appearance, but on opening the aorta a circumscribed ulcer was found as large as the end of the middle finger, which had perforated the vessel. From this point to the origin of the vessel, a distance of an inch and a half, the lining membrane was of a dark red colour, coated with lymphy deposits. The semilunar valves were free from disease. From the character of the ulcer, its indurated edges, and the general aspect of the lining membrane of the vessel, Dr. Talley concluded the disease to be of long standing. The coats of the aorta were somewhat thickened close to the seat of ulceration, but the calibre of the tube was not at all increased, so that the disease presented nothing resembling aneurism. The symptoms having been eminently those of the third stage of phthisis, the author's attention was not directed specially to the state of the heart during the patient's life, so that he is unable to inform us whether the condition of that organ revealed by the post-mortem was attended by any peculiar phenomena during life.

VII. *Aneurism of the Hepatic Artery*, By Dr. HEINRICH WALLMANN. (Archiv für Pathologische Anatomie und Physiologie, Band xiv. Hefte 3 und 4.)

The extreme rarity of aneurism of the hepatic artery justifies our placing the following case briefly before our readers. Mrs. P——, widow, a native of Bohemia, aged thirty-six, had always enjoyed good health until three months before she came under Dr. Wallmann's care. Since then she had frequent severe attacks of pain in the epigastrium, with free intervals, without jaundice or disturbance of the secretions or catamenia; she lost flesh, and the severity of the pains increased. On admission to the clinical wards of the Joseph's Akademie, Feb. 3rd, 1858, she was much emaciated; heart and lungs normal, the spleen enlarged, the liver projecting three-finger breadths below the ribs, and with hard edges; no ascites or fever; she had never had a blow in this region. The paroxysms of pain occurred daily, and at the time the epigastrium and hepatic region were very tender, but not at other times. The fæces had previously been firm and of brown colour, and suddenly became whitey-grey, but there was no icteric hue. Soon after this change of colour, the distended gall-bladder could be readily felt as a smooth round tumour. Ten days prior to death the patient became intensely jaundiced, the paroxysms of pain continuing, and the liver increasing in hardness and size. Death occurred on the 27th February, consciousness being preserved to the last. We can only make room for the prominent points noted in the autopsy. The liver was partially adherent to the diaphragm, the peritoneal covering thickened, the organ large, soft, friable, and green; the biliary ducts of the liver distended, the branches of the vena portæ filled with fluid blood. The gall-bladder was much enlarged and distended with thick black bile, with numerous plates of cholesterin; the cystic duct was impervious, and the upper half of the hepatic duct distended; its communication with the cystic duct was interrupted by a tough, fibroid plug; the common duct was distended throughout. The stomach was contracted, so as to resemble a portion of intestine; the intestines themselves were collapsed. There was a pound and a half of sanguinolent serum in the pelvis; darkened coagula lay in the region of the transverse colon, in the left hypochondrium, and elsewhere in the abdomen. Between the upper edge of the stomach and the lower margin of the liver there was a tumour nearly as large as a child's head, so as to occupy a large portion of the space belonging to the lesser omentum. At the concave side of the liver, near the left margin, it was intimately adherent to the hepatic coat over a space 1¼ in. by 2 in. The tumour had an irregularly oval form, and at its lower convex end, close to the transverse colon, was a ragged opening an inch and a quarter long, from which a dark coagulum projected. This sac was filled with concentric layers of blood and fibrine, weighing altogether 1¼ pounds. The sac consisted of dense walls, from two to three lines thick, consisting of areolar tissue, with rough transverse bands bridging across the interior. The sac rested with a broad basis upon the hepatic artery, with which it communicated by a fissure one centimetre (0·393 inch) long, by four millimetres (0·156 inch) broad, the edges of which were hard and smooth. The hepatic and common ducts were partly inclosed in the walls of the sac. The portal vein was partly adherent to the posterior wall.

The author in his summary observes that the aneurism was probably due to a dilatation of all the arterial coats, which were gradually destroyed by pressure and other processes, because the arterial coats could only be demonstrated at the neck of the arterial tumour; they were not to be found at any other portion of the sac. The paroxysms of pain he accounts for by the irritation of the branches of the hepatic plexus enclosed in the sac, and the pressure exerted upon the solar ganglia and adjoining nerves. For the cause of the aneurism, Dr. Wallmann can offer no explanation.

VIII. *Some Facts in Relation to the Nocturnal Incontinence of Urine in Children.* By Dr. ADDINELL HEWSON. (The American Journal of the Medical Sciences, Oct., 1858. From Transactions of the College of Physicians of Philadelphia.)

In the House of Refuge (of Philadelphia ?) there are on an average 292 boys, and the habit of wetting the bed had become so prevalent, that Dr. Hewson, in 1857, was requested to suggest a cure. He found that no less than 78 boys were guilty of the practice, or a proportion of about 1 : $3\frac{3}{4}$. Dr. Hewson consequently subjected the whole establishment to a close critical inquiry, and continued his observations for five months. The results which he gives are drawn from the consideration of 63 of the 78 cases which remained in the house during the whole period; of these, 29 were whites and 34 negroes, and as the total number of whites and blacks was respectively 201 and 91, the affection was more than twice as prevalent among the blacks than the whites. The worst cases also occurred among the blacks. The average age of all was thirteen years, the extremes being seven and eighteen years; the greatest number affected at any given age was 9, the age being fourteen. The appetite was excellent in 53, poor in 10, though only 34 had the appearance of good health; 24 suffered from ascarides, including 5 of the 6 most inveterate cases. Herpes circinatus occurred in 7 white boys, and 1 had itch; 27 had clean, healthy tongues, 13 being white, 14 coloured; 19 had pale, putty-looking tongues, 8 being white, 11 coloured; 17 had a furred tongue, 8 being white, 9 black; 26 had 1 evacuation per diem, 16 from 2 to 4, blacks and whites being equal in both instances. One black boy had no control over his sphincter ani; 20 suffered from constipation, including 3 of the 6 worst cases, but the worst case of all was regular in his bowels. Twenty-seven of the cases urinated two or three times a day, besides morning and evening discharge; 28 passed water more frequently; 3 could give no account; 4 had no control over the bladder at all. The urine was normal in colour in 46, very pale in 17, acid in all. Average specific gravity among whites, 1016, among the blacks, 1020. Uric acid was deposited in 31 specimens, urate of ammonia in 8, urate of soda in 1, triple phosphate in 1. It does not appear that the urine of each boy was examined more than once. The prepuce and penis were discoloured, and the former elongated, either from frequently pulling or scratching or from masturbation, in no less than 46 cases, 21 being whites, 25 blacks. Eighteen boys confessed to masturbation; in 12 there was no suspicion of it, but the remaining 63 were strongly suspected, in spite of their denial. The greatest number were always reported as having wetted their beds on Wednesday and Saturday nights, the smallest always on Sunday nights; 2 did it every night without intermission, only 12 did it once a week, 3 not as often, and the remainder varied from 6 or 7 to once or twice a week. The diet on Wednesday and Saturday was salt pork, or fish and hominy for dinner for the coloured boys; the white boys had the same on Saturday only, and fresh boiled beef, soup, potatoes, rice, cabbage, and bread on the other days; and for the evening meal they had mush and molasses regularly every evening except Saturday and Sunday evening. On the former evening they had soup, on the latter bread and molasses. A sudden fall of the barometer or thermometer, or of both, always appeared to increase the number. Other influences at work were the better treatment of the white than the black boys, in regard to ventilation, exercise, and work.

The various remedies employed were first bromide of potassium in $2\frac{1}{2}$ to 3 grain doses thrice daily, with which 9 were cured; 2nd, tincture of sesquichloride of iron, from three to five drops thrice daily for six weeks (this failed entirely); 3rd, cantharides externally and internally (equally ineffectual); 4th, each boy suffering from constipation received a dose of magnesia, those who had worms one drachm of turpentine and five grains of bicarbonate of soda, three times a day, and all received five drops of Squire's juice of belladonna, prepared by Bentley's process; they also had all a dry supper of bread alone, as previously ordered, and the cold douche. Each boy was made to rise and micturate an hour after retiring at night. A sudden diminution followed, and two weeks after commencement of this plan, only four cases besides those who wetted their clothes in the day were reported, and on their supper being reduced (as a punishment), the former four also lost the bad habit, and those only remained uncured who had diurnal incontinence, though they were improved.

IX. *On the Presence of Inosit in the Urine in Renal Diseases.* By Dr. H. VOHL in Bonn. (Archiv für Physiologische Heilkunde. Jahrgang 1858, Drittes Heft.)

Inosit, a saccharine matter, stated by Lehmann to be incapable of vinous fermentation, has hitherto chiefly been found in the muscular tissue of the heart. In the present paper the author shows that grape-sugar may in diabetes mellitus be replaced by inosit, the formula for grape-sugar being $C_{12}H_{12}O_{12} + 2\ HO$, for inosit $C_{12}H_{12}O_{12} + 4\ HO$. Cloetta has recently shown that inosit ocurs in the urine of persons affected with albuminuria—a circumstance that suggested the inquiry to Dr. Vobl which has led to his discovery. In a case of diabetes mellitus in which the test for grape-sugar ceased to yield the usual result, Dr. Vohl subjected the urine to the following examination:—The residue obtained by evaporation of the urine in a water-bath was precipitated with caustic baryta, and the filtrate mixed with equal parts of weak spirits of vine (50 per cent.) again filtered, and the clear fluid mixed with strong alcohol, (90 per cent.) Chloride of sodium crystals were deposited on the walls of the glass cylinder, and also shining crystals resembling sulphate of lime, which on being dissolved in water and decolorized by animal charcoal, yielded on evaporation fine large tabular and columnar crystals. The crystals effloresced readily, and at a temperature of 100° C., yielded a residue of 16 to 17 per cent. Evaporated to dryness with nitric acid on platinum foil, and then treated with chloride of lime and ammonia, the reaction for inosit was obtained.

Dr. Vohl found that the increase of inosit was in the exact ratio of the diminution of the grape-sugar. At last, when the grape-sugar had altogether disappeared, he was able to obtain as much as 18 to 20 grammes of pure inosit from the urine passed in the course of a day. Dr. Vohl tried to convert grape-sugar directly into inosit, but this he failed in achieving.*

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E., London.

I. *On a New Elastic Bandage in Fracture of the Jaw.* By Prof. Bouisson. (Bulletin Général de Thérapeutique, tome lv., pp. 13 and 62.)

In this paper, Professor Bouisson, of Montpellier, describes a new form of bandage, which he has found very useful in the treatment of fracture of the lower jaw, and which being constructed of elastic material, allows of the combination of solidity with the amount of mobility requisite for carrying on mastication, insalivation, deglution, and speaking. The point of support is derived from the cranium, by means of an open skull-cap, constructed of broad strips of leather or jean. One of these bands passing in front of the forehead, encircles the cranium below, and another passes along its antero-posterior diameter above, being joined to the first at its two extremities, and by two transverse bands. Next, a sling, composed partly of leather and partly of caoutchouc, is nicely padded and adapted to the chin, and is then attached on each side by means of two leather straps and buckles, to the band of the skull-cap, a horizontal strap being attached behind, opposite the mastoid process, and a vertical strap in front, opposite the coronoid process. The amount of support may be easily regulated by means of the buckles, and its lightness, suppleness, and ease of application render the apparatus not only an effectual, but a comfortable means of adjustment. It secures effectual adaptation of parts, and with modifications may serve also in compound and comminuted fracture, as also in a variety of other accidents and diseases of the jaw.

II. *On Intra-Ocular Hæmorrhage after Extraction of Cataract.* By Dr. Rivaud Landrau. (Annales d'Oculistique, tome xl., pp. 129–137.)

In relation to a clinical lecture upon this accident, by Mr. White Cooper, Dr. Rivaud Landrau observes that in more than 2000 operations for extraction, performed during seventeen years' special practice at Lyons, he has only met with this complication in four instances, the particulars of which he furnishes in the present communication. He rejects Mr. Cooper's hypothetical explanation that the occurrence is produced by reason of the complication of the case by the existence of deep-seated disease of the eye. In his own cases he has found, notwithstanding their differences in other respects, there has always been an evacuation of vitreous humour, either taking place during the operation itself, or resulting afterwards as the consequence of a contusion. The portion of the humour which remains, being propelled forwards during the spasmodic contraction of the ocular muscles which ensues, becomes detached from the choroid, and the hæmorrhage is the consequence of the rupture of the minute vessels meandering in the hyaloid and between it and the choroid. It is impossible that the central artery of the retina should furnish the blood, as supposed by some authors, so considerable is the quantity sometimes. Mr. Cooper believes that the detachment of the vitreous humour is a consequence, not the cause of the hæmorrhage; but this gives no explanation of the rupture of the small bloodvessels, to which the hæmorrhage is due. This hæmorrhage is to be found whenever there is a considerable escape of the vitreous humour; and as the hæmorrhage is in the author's view but a consequence of such escape, its prevention is only to be sought in the measures which prevent the loss of this substance.

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* Want of space compels us to postpone several interesting articles.—Ed.

III. *On Mercurial Disease and Syphilis.* By Prof. LORINSER. (Wien Med. Wochenschrift, Nos. 19–21.)

Professor Lorinser observes, that ever since mercury has been employed in the treatment of syphilis, practitioners have never been wanting who protested against its use as something worse than the disease itself. Still, whatever its opponents might advance, mercury has always emerged anew after the contest, this arising, on the one hand, from the arguments opposed to its employment not being clear and incontradictable, and on the other, from its supporters being able to point to the apparent success of their mode of treatment. Two important discoveries, however, of the present time may assist in the scientific solution of the problem. The first is that of Melsens, who has shown that iodide of potassium is a means by the agency of which lead or mercury that may have lain concealed in the system for even years becomes excreted; and the other is, that the minutest traces of quicksilver may be detected in the urine, sweat, or saliva by means of the electrolytic test, so ably handled by Professor Kletzinsky. The author's attention had been already attracted by the fact of the extremely rapid influence exerted by the iodide in certain cases of pains in the bones, and optic and serpiginous cutaneous ulcers; and while certain experiments in treating syphilis were being carried on in 1856 at the Weiden Hospital, he had the opportunity of investigating the subject closely, and has continued so to do, the conviction becoming forced on his mind that the benefit derivable from the iodide is really due to its expelling mercury previously given, perhaps years since. The following are the conclusions he arrives at:

1. That mercury, whether administered internally or externally, may remain for years within the body, without a trace (under ordinary circumstances) being detectible in the urine. Numerous trials made upon patients to whom mercury had been given amply proved this; but it is not meant to be stated that where large quantities of mercury have been thrown in, that this may not be sometimes detected in the urine or sweat, as it is also in the saliva during salivation. 2. By the employment of the iodide of potassium the mercury contained in the body is expelled in the urine, so as to become detectible by means of the electrolytic chemical analysis. The expulsion does not always take place immediately, several days sometimes first elapsing. 3. All those forms of disease which exhibit a rapid decrease proportionately to the excretion of the mercury, and which disappear when this is entirely expelled, can only be regarded as mercurial disease, which can only be said to be completely cured when no more mercury appears in the urine under the continued use of the iodide. All the symptoms which thus can be referred to hydrargyrosis disappear with remarkable rapidity under the use of the iodide; while in other cases, when no mercury can be detected, the iodide exerts no effect. 4. The symptoms gradually produced by the long sojourn of mercury in the system differ essentially from those which immediately result from recent mercurialization. The symptoms of this *chronic* hydrargyrosis are indeed far less known than those of the acute form; one very remarkable thing is, that the symptoms of chronic hydrargyrosis may be thrown in the background, and seem for a length of time to have undergone improvement through the occurrence of a new and acute mercurialization. 5. The symptoms of chronic hydrargyrosis have hitherto for the most part been mistaken for those of syphilis, and sometimes for those of gout, or the ordinary nervous and abdominal affections; and only very rarely has their true nature been apprehended. 6. As hitherto chronic hydrargyrosis has not been clearly distinguished from syphilis, both conditions having been usually treated alike, all the cases of secondary syphilis on record are only of doubtful value; and new observations, founded upon a physico-chemical diagnosis, so as accurately to distinguish between hydrargyrosis and syphilis, are required.

IV. *Observations on the Treatment of some of the Symptoms of Syphilis.* By M. HERVIEUX. (Bulletin de Thérapeutique, tome liv., pp. 441 & 529.)

1. *Phagedænic Chancre.* M. Hervieux observes that it is very natural that a disease which produces such rapid local destruction should have been met by means rivalling it in energy and celerity of action, such as the butter of antimony, the various forms of caustic, the actual cautery, &c. But although all those means have been successful in some cases, it is certain that they have still oftener failed, or they would not have been so generally abandoned. There is one means, however, which, in the hands of M. Ricord, has proved of indubitable advantage—viz., the *carbo-sulphuric paste*, prepared by mixing sulphuric acid with powdered vegetable charcoal, in sufficient proportions to form a semi-solid paste. When applied to the chancre this soon dries, forming a black crust, which intimately adheres to the tissues, and only falls

off after several days, leaving a clean sore, or even, in some cases, a cicatrized surface. In the author's practice, pure *tincture of iodine*, applied at the commencement, has proved to be the best means of arresting the progress of the disease. It induces generally a burning pain, the intensity and duration of which are in proportion to the extent and depth of the chancre, as also to the sensibility of the individual and of the parts affected. Very well borne by some patients, the pain induces in others the most horrible torment. Chloroform would in such nervous and irritable subjects save this suffering. The pain, upon an average, lasts half an hour. In simple, uncomplicated cases, two applications, made by means of a pencil after an interval of twenty-four hours, generally arrest the progress of the blood. If, however, the chancre be complicated with gangrene, hospital gangrene, or diphtheria, four, five, or even six applications may be required. But when two or three of these seem to be without any effect, there is no use going on with the iodine, and a solution of nitrate of silver (five parts to thirty) should be substituted. When the iodine treatment has been followed, M. Hervieux has never known the worst form of phagedæna persist beyond a week.

2. *Suppurating Bubo.* The author has never himself treated bubo by small, single, or multiple openings, but he has met with cases which have been so treated, and which, two or three months afterwards, have exhibited fistulous tracks, extensive detachment, thinning and changes in the skin, together with an utter indisposition to heal. After waiting two or three weeks in vain for the spontaneous closure of these fistulæ, he has had to lay them freely open. The prevention of deformity by these small apertures, as proposed by Vidal, is frequently not attained, for not only may fistulous tracks become established, but the apertures themselves may become transformed into chancrous ulcerations. As a general rule, M. Hervieux makes a large opening, and that as early as possible, cicatrization taking place most rapidly under these circumstances. When the opened bubo is transformed into a strumous or chancrous ulcer, or the two combined, with the possible complication of phagedænism, he treats it by the application of the tincture of iodine or solution of the nitrate of silver, washing it out also with chlorine lotions several times a day; and he has never found any ulcer resisting treatment longer than six weeks, the majority becoming healed in from eight to fifteen days.

3. *Condylomata (Plaques muqueuses).* Although the author believes the practice he recommends under the former heads may require additional confirmation from more extensive practice than his own, in the matter of condylomata he can speak more positively. If the solution of nitrate of silver is not an actual specific, it acts with such rapidity, certainty, and efficacy, as to call for the highest recommendation. However confluent they may be, and whatever extent of surface they may occupy, however infectious the discharge they give out, and even when they have attained a certain amount of thickness, provided that they are not too hypertrophied and have not undergone some of the transformations they are susceptible of, they will wither, die away, and disappear in the course of some days, if every part be painted daily with a pencil dipped in a solution of the nitrate, five parts to thirty of water. Baths should be simultaneously used, seeing the part which dirt habitually takes in the production of this accident. Repeated trials have convinced the author that this success is quite independent of internal treatment. When, however, the condylomata have become transformed into a vast vegetating surface, of great thickness, the nitrate ceases to be of avail; and in one aggravated case mentioned, the pure nitric acid, repeatedly applied, was of service.

4. *Syphilides.* Under this head the author gives the results of his trial, in ten cases, of M. Cullérier's plan of treating syphilitic eruptions by blisters applied to the chest. Although at first prepossessed against it, he now speaks highly in its favour. Excluding the slight roseolar forms, which get well of themselves, the author oftenest employed blistering in the papular form of the disease, and that is the form in which the remedy best succeeded. A single blister will exert a notable modification on chronic papular syphilides, which have existed during several months. One case of syphilitic lichen, which had lasted a year, and for which all kinds of active internal treatment had been tried, disappeared in the course of a week, during which three large blisters were successively applied to the anterior and posterior surfaces of the thorax. The squamous form resisted their action more, but still in two cases of psoriasis undoubted amendment was observable, and in a fortnight the scales were detached. In the pustular form, some cases of syphilitic acne were rapidly cured. M. Hervieux has not tried blistering in syphilitic impetigo of the face and hairy scalp, having found the application of the nitrate of silver solution, after poulticing off the crusts, very efficacious, even in very inveterate cases.

V. *On the Treatment of Sprain by Friction and Shampooing.* By M. Girard. (Moniteur des Hôpitaux, No. 140.)

In this paper, laid before the "Académie de Médecine," M. Girard states that his attention was first directed to the plan of treatment he describes by the manipulations of an empiric.

So successful were these in a bad case of sprain, that he determined to investigate the subject, and as the result of numerous trials, he now proposes what he believes to be a very effectual and a rapid procedure, for the treatment of what too often proves a very tedious and serious affection.

No matter what the severity of the sprain may be, its treatment should be commenced by the gentlest friction, the points of the fingers scarcely touching the skin. After practising such frictions from below upwards for from ten to twenty minutes, it will be almost always found that a certain amount of pressure can be borne, and this is to be increased or diminished according to the sensations of the patient.

It is very rare that we can proceed in this manner for half-an-hour without the patient declaring that his pain is notably relieved. Arrived at this point, when the patient can bear the weight of the hand, we proceed to the shampooing. This is performed not only with the fingers (which, kept close together during the frictions, are now to be separated, so as to pass into the various sinuosities of the part,) but also with the palm of the hand, so as to embrace the entire joint and surrounding parts. The hand in both this and the former part of the procedure should be smeared with some fatty body, such as almond oil, so as to render its movements more soft and easy. The shampooing must be performed in the gentlest manner, without shocks, directed from below upwards, and acting not only on the painful points, but upon all those that are tumefied.

If pain is excited by an attempt at moving the joint, we must return to shampooing until new trials have proved that flexion and extension cease to excite painful sensations. Such movements would be very painful, or even dangerous, if performed at an early stage of the treatment. They do not, however, constitute any part of the treatment itself, and are only resorted to as a means of appreciating the results derived from shampooing. In several cases in which the cure had been considered as complete, the pains have returned next day, accompanied by more or less febrile reaction. A single re-application of the shampooing has sufficed to dissipate them, and in most cases, twenty-four hours' rest and the application of a bandage moistened with spirit of camphor, has sufficed for this. This bandage, indeed, is useful in all cases, and should be worn for two or three days.

This procedure is applicable to both old and recent sprains, and even when there has been present also a fracture of the fibula, shampooing has effected a remarkable diminution of pain and swelling, enabling the surgeon sooner to ascertain the exact nature of the case. Several cases of severe, recent, and old sprains are referred to, in which two or three hours of this shampooing process have effected an entire cure.

VI. *On the Treatment of Burns by the Permanent Warm Bath.* By Dr. PASSAVANT. (Deutsche Klinik, Nos. 36, 38, and 39.)

The occasion of this communication was furnished by the explosion of a firework factory at Frankfort, which gave rise to the loss of fourteen lives. Thirteen persons were taken to the hospital exhibiting almost every stage of burn, some of them scarcely aware, in the excitement of the moment, that they had been burned, although their sufferings after admission soon became excessive.

All the cases admitted were treated by the permanent warm bath where this could be applied, and where it could not, compresses dipped in warm water were substituted. This course was pursued in consequence of the success of former experiments made by the author, and the analogy prevailing between burns and other surgical affections in which the baths have proved so efficacious in the hands of Langenbeck.* The limb was placed in a suitable vessel, which was filled with warm water kept at a temperature of 27° R.; the water being usually changed twice a-day, and in the event of excessive discharges, oftener. If after some weeks maintenance in the vessel the patient's position became irksome, moistened compresses were substituted. The first effect produced by the bath was an immense abatement, soon to be followed by a complete cessation of the excessive pain—constituting in fact the completest anodyne—an advantage of sufficient moment, even supposing no other was derived. Besides this, however, the dried and hardened tissues became completely penetrated with water and thoroughly softened, and the destroyed parts were more easily separated and cleansed away. The wound was thus kept constantly clean and free from all sources of irritation, and the danger of absorption of pus and of pyæmia was diminished. The healing of the wound, too, takes place more rapidly under water, being promoted probably by the equable temperature and moderate compression of the water, and by the greater activity of the metamorphic and endosmotic processes.

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* See British and Foreign Medico-Chirurgical Review, vol. xvii.

VII. *On Calculous Diseases in Hungary.* By Prof. Balassa. (Wien Medicinische Wochenschrift, Nos. 25 and 26.)

This communication formed an answer to a circular sent out by Professor Gross of Louisville, U.S., for the purpose of obtaining statistical information relating to calculous disease. Nearly all the examples of this affection that occur in Hungary are brought to the Pesth Hospital: and during Professor Balassa's twelve years (1843-55) officiating in the surgical clinic he has met with 135 cases of stone. In regard to *age*, 21 of the patients were between one and seven years old; 32 between eight and fifteen; 47 between sixteen and twenty-five; 27 between twenty-six and fifty; 6 between fifty-one and sixty; and 2 between sixty-one and seventy. It is thus seen how large a proportion of the cases occur among the young, 100 of the 135 being less than twenty-six years of age; while a large proportion of the cases having been long neglected, might be referred back quite to childhood. We may add to these, 49 cases of stone treated at the Children's Hospital during the same period of time; so that stone in Hungary chiefly occurs in childhood and youth. Of the 135 cases, 82 occurred in the peasant class, and 39 among mechanics, the inhabitants of the country, therefore, much preponderating. Their especial prevalence amongst the poorer classes is farther shown by the experience of the author in private practice, he never meeting it among the children of the wealthier classes. This can only be attributed to difference in mode of life and alimentation, the wholesome food of childhood being frequently replaced by carboniferous food of difficult digestion. This view is farther borne out by the analysis of 83 of the calculi that were removed. A long account of these is given; but we have only space to state that great preponderance of the oxalates and urates was observable, especially of the former. The prevention of such calculous formation would be best sought by an amelioration of the regimen of the classes amongst which the disease almost exclusively prevails in Hungary.

Professor Balassa observes, that according to his experience catarrh of the bladder never accompanies the oxalates or urates; but in the case of phosphatic calculi it is always met with to a considerable and obstinate extent, the urine being acid in the former and alkaline in the latter case.

Of the 135 stone cases, in 13 no operation was performed on account of an extreme degree of consecutive disease of the urinary organs, and a disordered condition of the general system. In 122 cases operations were resorted to, although in some of the instances under very unfavourable circumstances, inasmuch as chronic nephritis, obstinate catarrh of the urinary passages, and various other ill consequences of the presence of calculus were present. Whenever by proper care and treatment the symptoms of these diseased conditions were ameliorated, and the state of the general health became materially improved, the operation was undertaken upon the old maxim, *anceps remedium melius quam nullum*, in several cases with success, but in 5 others with a fatal issue. In 2 of these cases there was found a high degree of eccentric hypertrophy of the calices of the kidney and ureters, and in two others renal atrophy. In 1 case the right ureter, excessively dilated, had become affixed to the cæcum through prior exudation; an intercommunication had formed between the two parts, and through this aperture a lumbricus had passed along the ureter into the bladder. That a diseased condition of the urinary organs should not always contra-indicate an operation, the Professor has had many opportunities of learning in the course of his practice. Even abundant purulent deposit may indicate disease of only one kidney, and the individual be saved by the operation, through the other kidney still performing its functions.

The neglected and complicated conditions of the cases that usually are brought to the author's clinic have led to his resorting to lithotrity in a comparatively small number, being compelled to abstain from it on account of the excessive hyperæmic irritability of the bladder, and the frequent attacks of inflammation. For the same reason his lithotrity operations have been less successful than the general statistics of the operation would lead us to expect.

Of the 122 operations, 92 were lithotomy and 20 lithotrity; 11 of the former cases (11·95 per cent.), and 5 of the latter cases (16·66 per cent.) dying. Of the 11 deaths after lithotomy, 5 took place from chronic disease of the kidney, and 1 from epidemic typhus, the remaining 5 succumbing to inflammation of the bladder and peritoneum coming on from the third to the fifth day after the operation. Strictly speaking, these were the only cases which died as the immediate consequence of the operation when performed in suitable cases, reducing the mortality in fact to 5·43 per cent. Of the 5 deaths following lithotrity, 1 took place from phthisis, 2 from chronic renal suppuration, and 1 from recent nephritis; 2 of the deaths alone were in fact directly referrible to the operation, giving the mortality of 6·66 per cent. Fistulous openings into the rectum, or infiltration of urine, did not take place after any of the *lithotomy* operations; but extraction, on account of the large size of the stone, being very difficult in several cases, peritoneal and vesical inflammation not infrequently followed, and in

5 instances terminated fatally. With the exception of 7 cases, the stone was extracted entire in all. The diameter of the largest stone in the collection measures two inches five lines, in 5 calculi it was somewhat above two inches, and in 24 it varied between one inch and one inch and a half. The heaviest stone extracted weighed ℥ iss. gr. x., and the lightest gr. xvii. It may be noted that among the 185 cases, 1 only was a female, and she was treated by lithotrity.

Professor Balassa recommends, after all bleeding has ceased, the application of some oiled strips of linen rag along the track of the wound, he having found it a good precautionary measure for the prevention of infiltration of urine, or the irritation of the lips of the wound by this fluid, especially when they have been much contused during the extraction of a large stone.

VIII. *On the Treatment of Hernia by Electricity.* By Dr. Clemens. (Deutsche Klinik, No. 34.)

This paper is the first of a series the author intends publishing upon the therapeutical application of electricity—a subject that has engaged his attention for some years past. He first employed this agent in the treatment of inguinal hernia in 1850, and has frequently had recourse to it since then. The hernia being reduced, and the patient placed in the semi-recumbent position, the ball of the conductor is carried as far into the hernial canal as possible, and the application of the electricity continued during five minutes, its power being increased day by day. After a few *séances* the mouth of the ring becomes diminished in size, the finger is introduced with more difficulty, and the hernia will not descend so easily as heretofore. The electricity, too, exerts a very beneficial effect upon the peristaltic intestinal motions, augmenting and regulating these, and thus preventing the same relaxed portion of intestine from always lying opposite the hernial aperture. A state of obstinate constipation becomes changed for one of regular action, and many old disordered conditions of the abdominal cavity become relieved. When the hernia has been recently produced, no means act with so much certainty and rapidity; and a case is referred to of a young man who acquired double inguinal hernia during an effort to raise a heavy burden, and which was completely cured after twenty *séances*, although these were not commenced until a week after the accident. Under its agency recent hernia is rapidly returned; but the author has not yet tried it in a case of complete incarceration. Among the 27 cases in which it has been resorted to, none have manifested the slightest ill consequences. Dr. Clemens prefers static electricity to galvanism, and adminsters it by means of the Leyden phial.

Another application of electricity by the author consists in a galvanic hernia truss, for a description of the construction of which we must refer to his paper. By its agency a feeble but constant galvanic stream is kept applied to the ring, and large hernias soon become easily retained which before had resisted the largest trusses and the strongest springs. Of late the author has constructed a pile of silver and copper coins, and the effects of so small an apparatus have often surprised him.

IX. *On Diphtheritis of the Palpebral Conjunctiva.* By M. Magne. (L'Union Médicale, No. 100.)

M. Magne, in this paper, calls attention to the affection described under the same name by A. von Graefe,* and which seems, like other diphtheritic diseases, to be acquiring prevalence of late years. Thus, M. Magne describes four cases that have occurred to him since 1853, while a thirty years' practice at a well-frequented clinic has furnished no other examples; and M. Sichel, during the same period of time, amidst the thousands of cases of ophthalmia that have come under his notice, only recollects two instances of pseudo-membranes forming in the palpebral conjunctiva, independently of the action of heat or caustics. M. Magne believes that the comparative frequency with which MM. Chassaignac and A. von Graefe are said to have met with the disease arises from a confusion in names. The affection they describe was not, in fact, characterized by a true pseudo-membrane, but by a mere mucous or muco-purulent concretion, which was so slightly adherent as to admit of removal by injection, sponge, forceps, or even the fingers, forming a juxtaposition rather than an adhesion. The concretion removed, the conjunctiva is red and sometimes granular, but its surface is always uniform. In the affection now described, however, which may be justly styled palpebral croup, there is a true pseudo-membrane, an albuminoso-fibrous tissue intimately blended with the conjunctiva, and which cannot be removed in a single piece, but must be scratched or scraped off, leaving the conjunctiva bleeding at the points whence it has been removed. It is also soon reproduced. It is a disease which especially affects children, and seems to be connected with a general condition of the system, rather than of a mere local nature. It would not seem to be contagious, for in the four cases here recorded but one eye was affected although the other eye was frequently exposed to the contact of the diseased secretions. It is a very severe affection, but it is curable. The treatment chiefly to be relied upon consists in the application of leeches, the injection of solution of nitrate of silver, and the internal administration of the chlorate of potass.

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* See British and Foreign Medico-Chirurgical Review, vol. xx.

X. *On a New Mode of Operating for Cancer of the Lip.* By Professor O'Shaughnessy. (Indian Annals of Medicine, July, p. 485.)

Professor O'Shaughnessy observes, that when cancer of the lip is confined to a limited spot, it is easily removed by the ordinary V-shaped incision, but that this procedure does not suffice when the whole lip, and perhaps one or both commissures, are involved in the disease. In a case which occurred in his own practice, the cancer not only occupied the whole lower lip, but the right commissure and a part of the upper lip also, on that side. "I thought nothing could be done in such a case, until the plan struck me of making a lip by detaching a triangular portion of the cheek on either side of the mouth, in the following manner:—The whole of the diseased lip to be removed by making two incisions meeting at a point in the centre of the chin, the cheeks then to be divided by two horizontal incisions extending from the angle of the mouth on either side, and continued backward as far as the masseter muscles; these to be joined, at their posterior extremities, by two oblique incisions carried upwards and backwards, from either side of the chin, leaving two triangular flaps to be dissected forwards, so as to admit of the apposition of the edges of the V-shaped gap left by the removal of the cancer." An excellent lip was in this way made, and most of the lines of incision had united by the first intention, when about the tenth day the patient was carried off from the effects of retention of urine.

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D. Lond.

PHYSICIAN TO THE ROYAL MATERNITY CHARITY.

I. Pathology of the Unimpregnated Female.

1. *On the Cure of Vesico-Vaginal and Vesico-Uterine Fistulæ.* By Dr. Gustav Simon. (Monatsschr. f. Geburtsk., July, 1858.)
2. *A New Procedure for Plugging the Vagina.* By M. Montamier. (L'Union Méd., Dec., 1858, and Gaz. d. Hôp., Sept. 1858.)
3. *Medicinal Trials of Carduus Mariæ, Carduus Benedictus, and Onopordon Acanthium.* By Lobach. (Verh. d. Phys. Med. Ges. zu Würsburg, viii., 3, 1858.)
4. *Ovarian Cyst in a Puerperal Woman cured by Spontaneous Rupture and Discharge through the Colon.* By Dr. Lumpe. (Zeitsch. d. Ges. d. A. zu W., No. 22, 1858.)
5. *Cancer of the Fundus and Body of the Uterus perforating the Rectum.* By Dr. Harris. (North Amer. Med. Chir. Rev., Sept. 1858.)
6. *Report of a Case of Inversion of the Uterus successfully reduced after Six Months.* By Dr. White. (Amer. Journ. of Med. Sc., July, 1858.)

1. At a meeting of physicians held at Darmstadt in 1857, Dr. Gustav Simon detailed the methods he had pursued in the treatment of vesico-vaginal and vesico-uterine fistulas, and submitted eight of the patients who had been operated upon, to examination. Of 19 fistulæ treated, 10 had been completely healed; in 5, cure was nearly complete; 1 was abandoned as incurable; 2 women died after operation. One case was a vesico-uterine fistula—that is, the fistula passed from the bladder into the cavity of the uterine neck, without injury to the vagina; the urine passed through the os uteri. The incontinence of urine was cured by the obliteration of the os uteri, which was effected by uniting the split and freshened

lips together by seven sutures. The woman retains her urine. The communication between bladder and the uterus persists; menstruation is entirely effected through the bladder. The woman remained well two and a half years after the operation.

In a second woman, the anterior lip of the os uteri was destroyed, with a part of the vaginal wall of the bladder, so that a deep vesico-utero-vaginal fistula resulted. This fistula was healed by using the posterior lip of the os uteri as a flap to be united to the walls of the bladder. In this woman also the menstrual secretion passes through the bladder. The woman is well three years after the operation.

In a third woman, a large fistula extended from the neck of the bladder to the os uteri. Dr. Simon split the os uteri and covered the loss of substance by means of the so-made moveable anterior lip, and united it to the walls of the bladder. This woman is well three years after the operation, and has borne a living child since.

In five other women, in whom vesico-vaginal fistulæ, varying from the size of a bean to that of a shilling, existed in various parts of the vesico-vaginal wall, cure was effected by the union of the walls of the bladder.

Others in whom the fissure was very small, were healed by cautery alone.

In three cases, the fistulæ were so large, that the deficiency included the entire *bas-fond* of the bladder to the urethra, so as to render the attempt to unite the borders of the fistulæ hopeless. In these cases Dr. Simon resorted to an operation to be presently described, which he calls the *cross-obliteration of the vagina.* Two women subjected to this operation have found their condition so much bettered, that both in sitting and at night they can hold their urine and void it at pleasure, and follow their occupations.

In the case of the only incurable fistula, the woman had a very large opening, the sphincter vesicæ being wanting.

The two women who died had fistulæ of medium size in the vicinity of the os uteri. In one who died of pyæmia seven days after the operation, the walls of the bladder had united. In the other, who perished seventeen days after the operation from suppuration in the cellular tissue between the bladder, uterus, and rectum, and consequent perforation of the peritoneum, the wall of the bladder had become united with the freshened anterior lip of the os uteri. In the operation upon the first patient, the uterus had been drawn down by Museaux's hook-forceps, in the second not.

A third woman died under Dr. Simon's care, after making a considerable division of a strong adhesion of the vaginal walls. She died on the sixth day of œdema of the lungs following on pyæmic pneumonia, before the operation proper for the fistula had been undertaken.

The operation performed by Dr. Simon consists in the free *freshening* of the edges of the fistula, and union by the knotted sutures.

We will now describe briefly his operation of *cross-obliteration of the vagina.* It consists in this—that the remains of the vesico-vaginal wall are brought into union with the freshened hinder wall of the vagina, or bladder with rectum in a transverse direction. Thus a receptacle for the urine is formed, which is embraced by the upper part of the vagina, the roof of the vagina and the defective bladder, and directs the urine into the urethra. A portion of the vagina below the seat of the operation remains as before. The operation is so carried out, in cases where the deficiency of the bladder is so very great, that of the hinder wall of the bladder only the urethra remains, that the upper edge of the remaining urethra is freshened over a space of two to three centimetres, and on the level of this edge, especially on the side and posterior wall of the vagina, a similar extent is freshened in like manner. The apposition of the wound is effected by six or seven sutures. With a very bent needle the wound made in the vagina is transversed and surrounded by one thread. The freshened part of the rectum-wall and the hinder edge of the urethra are thus united when the loops of the suture are brought together. The closure of the vagina forms a transverse—often, on account of the great yielding of the recto-vaginal wall, an arched line. Dr. Simon extols the advantages of this operation over the other methods, episiorrhaphy and transplantation for obliterating the vagina. It promises greater certainty in healing, it is free from danger, since only superficial mucous parts are divided, and so far answers the purpose, that it prevents incontinence of urine and preserves a portion of the vagina, and exerts no after bad influence. This operation he has performed three times, with so much success as to have only very small fistulæ remaining. By it the greatest deficiencies of the bladder may be remedied.

2. M. Montamier recommends a modified form of Gariel's air-pessary for plugging the vagina. He urges, and with truth, the objections to the simple air-pessary, that it is apt to slip, that it fails to compress perfectly the os uteri, and does not admit of the application of medicaments to the uterine neck. He covers the air-pessary with a linen-cap which admits of being tied on by a slip-knot; on the summit of the cap a layer of fine sponge is sewn.

The pessary thus armed completely fills the vagina, without its being necessary to over-distend it; and the sponge can be saturated with any liquid thought desirable.

3. M. Lobach relates his experience of the uses of the Carduus Mariæ, Carduus Benedictus, and Onopordon Acanthium. He says the seeds of the Carduus Mariæ have a surprising efficacy in arresting uterine hæmorrhage. Not only does this remedy check the discharge at the time, but by continued use prevents the return, and this in cases where krameria, sesquichloride of iron, phosphoric acid, tannin, &c., had failed. It especially operates in cases where the floodings are associated with portal obstruction, diseases of the liver and spleen, hemorrhoids, &c., but not where the flooding depends on wounds, ulcerations, or heterologous formations. It may be used in the form of decoction or tincture.

4. The case of ovarian dropsy related by Dr. Lumpe illustrates one of the modes in which this disease is sometimes spontaneously cured. A weakly pluripara had exhibited since her second labour pain in the left ovarian region. After the third week, the abdomen remained perceptibly enlarged, and an ovarian cyst that admitted of being grasped was formed. At the same time a smart peritonitis occurred. To diminish her suffering, a puncture was made, and some fluid withdrawn; this was greyish-green, curdy, distinctly smelling of fæces. Her condition mended, but soon the cyst grew to its former size. A second puncture was determined on, when, without obvious cause, diarrhœa set in, and daily increased with colicky pains. At first the evacuations were of a fæculent brown, gradually paler, and lastly of the same colour as the contents formerly drawn from the cyst by puncture. These discharges persisted for nearly five weeks, when the woman recovered, and might be considered completely cured. Rupture was apprehended. The diagnosis between retroversion and a descent of an ovarian cyst was doubtful. The appearance of the fundus uteri in the abdomen was especially opposed to the first supposition. In either case, the treatment appeared clear. If the tumour could not be pushed back above the pelvis, the trocar must be used to empty it, for a rupture seemed imminent. About eight o'clock on the 18th, the pains were violent and unintermitting. The patient was deeply narcotized by chloroform, and attempts made to push up the tumour. This, after great effort, was effected, and then another very smooth and elastic swelling came down into the pelvis; this was the bag of membranes, which soon burst, discharging a remarkable quantity of water, the two feet of the child immediately following. Delivery was soon completed. The fœtus was of six months' gestation; it lived for nine hours. The patient recovered favourably. After-examination revealed no sign of ovarian tumours. The fluid sac felt in the pelvis could only have been the posterior wall of the uterus distended into the form of a pouch. The designation retroversion seems misapplied.

5. Dr. Harris records an interesting and rare example of cancer affecting the fundus and body of the uterus. The subject was a married lady, aged fifty-four. The disease gave evidence of its presence more than four years before death. She suffered from sanguineous discharges, at a later period mixed with broken-down cancerous tissue and purulent secretion. For about two years and five months she suffered no pain, she lost flesh very slowly. For the last twenty-one months she suffered daily from severe lancinating pains in the iliac regions. When a vaginal examination was first permitted, which was only at an advanced stage, the uterus was found enlarged, its cervix very hard, but smooth upon its vaginal surface. Seven or eight months before death pus was found in the excrementitious matters from the rectum. About four months before death, fæcal matter began to pass by the vagina, and toward the latter period of life nearly the whole of the excrement passed through this channel. Death ensued from exhaustion.

Autopsy.—The omentum was joined to the fundus of the uterus; and in front of this adhesion the cavity of the womb communicated with the cavity of the abdomen by an irregular aperture, with very soft and rotten edges. The uterus filled the pelvis, with the exception of the space occupied by the contracted rectum and bladder. The ovaries were cancerous; the os uteri was oval, soft, patalous; the cervix short, and had lost its scirrhous condition. The posterior wall of the uterus was united to the rectum, and the cavities of the two viscera communicated by an ulcerated opening an inch wide and three inches long. The soft cancerous matter exhibited broken-down cells. The cancer-tissue proper was of a medullary character, easily breaking down under pressure; it contained an abundance of oval cancer cells, with nuclei and nucleoli. This cancer tissue appeared to the naked eye to be infiltrated in the tissue proper of the uterus, constituting *infiltrated medullary carcinoma.*

6. Dr. White, of Buffalo, relates two cases of inversion of the uterus in which forcible reduction was effected, in the one case after eight days, in the other after six months.

CASE I.—January 28th, 1856, a young woman was delivered of her first child. She was stated to have given birth to a male infant weighing ten pounds and a half. A large tumour descended into the vagina after the placenta. The hæmorrhage was "terrific," causing protracted syncope. A few days afterwards, when making an effort to evacuate the bowels, the tumour passed the os externum. When seen, the inverted uterus was as large as at the fourth month of pregnancy, inflamed and tender, hard and inflexible, the body apparently distended with blood from the constriction of the neck. An attempt was made to compress the uterus by the hand and return it; some blood was lost, and the patient became very faint. Fomentations were applied to the genitals; she had a severe chill in the night; pulse 144, feeble. Extract of belladonna applied to cervix. The next day (the eighth from the labour) reduction was again attempted under chloroform. Dr. White passed his right hand into the vagina, and firmly and continuously compressed the uterus; after some time it was found that the fundus could be "dimpled" by the thumb. Pressure upon this point was sustained until the hand became nearly powerless; a rectum bougie was substituted for the thumb, to give rest to the hand. Whilst pressing with the thumb on the fundus, the left hand was applied externally over the uterine tumour, to give counter-support. At length the fingers of the left hand, being pressed well down into the abdomen, seemed to fasten upon or hook over the anterior uterine lip and aid in its reflexion over the organ. Thus securely held between the two hands, the efforts at reduction were continued until the operator was nearly exhausted. The reduction was, however, effected. All hæmorrhage ceased from this moment. The patient died on the third day, having manifested great irritability of stomach.

Autopsy.—All tissues extremely bloodless. A little serous effusion within the peritoneum; and between some of the convolutions a very little lymph was exuded. Externally the uterus presented its normal shape and position, the tissues were not softened, nor was there any laceration. The examination revealed no cause of death, unless the anæmic condition of the tissues may be considered as such.

Case II.—A lady, aged thirty, was delivered in September, 1857, of her second child. The placenta adhered, but was removed in thirty minutes; this was followed by copious flooding, severe pain, and faintings. For three weeks she continued extremely weak. About this time she took an aloetic cathartic, which produced violent efforts at stool, and pains resembling those of labour; profuse hæmorrhage followed these efforts, and a large pear-shaped tumour made its appearance through the os externum. This was returned into the vulva. A physician who then saw her passed his hand high up into the vagina; and applied astringents and cold to restrain the hæmorrhage. During the succeeding three months she had occasional hæmorrhages. About the middle of January she had another severe attack of hæmorrhage, the tumour again presented externally, and was again returned. Prostration was very great. Nearly twenty-five weeks after the labour she was seen by Dr. White, who then proceeded to the operation of reduction under chloroform. The pressure employed was great and protracted. At length the tumour began to shorten *at its neck*, and the mouth of the organ to push upon the upper surface of the hand. No depression or dimpling of the fundus was at any time perceptible. The fundus finally passed out of the hand, and was easily pushed by a rectum-bougie (which had been maintained against the fundus) through the mouth and neck, up to its proper position. The bougie was left *in situ* till the next day. The patient recovered favorably.

II. Pregnancy.

1. *Remarkable Case of Retroversion of the Uterus in the Sixth Month of Gestation.* By Dr. Hecker. (Monatssch. f. Geb., Oct. 1858.)
2. *A Case of Extra-uterine Pregnancy: Fœtus extracted per Anum Four Years and Six Weeks after Natural Term.* By Christopher Johnston, M.D. (American Journal of Medical Science, July, 1858.)
3. *A Case of Extra-uterine Pregnancy which lasted Six Years, and was cured by Gastrotomy.* By Dr. Chévillon. (Union Méd. de la Gironde; and Monatssch. f. Geb., Oct. 1858.)
4. *A Case of Uterine Hydrorrhœa.* By George Shedd, M.D. (American Journal of Medical Science, Oct. 1858.)

1. Dr. Hecker records a case, described under the title of retroversion of the uterus, which has a special interest in a diagnostic point of view. A woman, aged thirty-five, had passed through several ordinary labours. In the pregnancy now described she suffered much from frequent impulsion to pass the urine, attended by pain and retention. On July 8th she began

to complain of pains in the hypogastrium, and took to her bed. The pains were of a cramp-like character, and seemed to threaten premature labour. This state lasted till the 12th, when the pains increased in severity. The fundus uteri was about the level of the umbilicus; strong contractions in it were felt. No distension of bladder; it had been freely and spontaneously emptied. The internal examination was by no means in harmony with these symptoms. No appearance of an os uteri could be felt until after repeated examinations of the patient, placed on her side, back, and knees, when on the level of the upper edge of the pubis a depression in the mucous membrane was perceived. No presenting parts of a child could be felt. Behind, the finger traced a smooth elastic swelling, as if distended with fluid, passing down the posterior wall of the vagina. This filled the hollow of the sacrum. With every pain this tumour was driven down, the distension being so great that spontaneous rupture was apprehended. The diagnosis wavered between a peculiar form of retroversion of the uterus, and an ovarian cyst propelled before the womb. In either case the course to be adopted seemed the same—to push back the tumour above the pelvis, and, if this could not be done, to puncture it with a trocar. At eight A. M., 18th, the pains were still more violent, and without intermission; the distension of the tumour was so great that rupture seemed imminent. The patient being put under chloroform, attempts were made to reduce the tumour; these at length were successful. At the same moment another smooth elastic swelling was felt entering the pelvis; this was the bag of the liquor amnii, which burst spontaneously with great force, ejecting a large quantity of fluid. The child was a female, weighing one pound five-eighths, of the period of six months. It lived nine hours. The placenta followed. The woman did well. An examination made after the delivery satisfied Dr. Hecker that the case was one of retroversion; the tumour consisting of a distension of the posterior wall of the uterus in a sacculated form. [If the fundus uteri were really felt throughout the labour at the umbilicus, then the pelvic swelling was probably, as conjectured, the stretched-out hinder wall of the uterus forming a pouch; but this can hardly with propriety be called "retroversion."—REP.]

2. Dr. Johnston's case of extra-uterine pregnancy furnishes an illustration of the many varieties of termination of extra-uterine pregnancy. On March 18th, 1858, M. H., aged thirty-nine, was admitted into the Baltimore Infirmary. She had supposed herself labouring under dysentery for two weeks previously; was now extremely emaciated, much annoyed by abdominal pains, and frequent small muco-sanguineous intestinal discharges, always offensive, and at times mingled with dark semi-fluid fæces. On examination the uterus seemed to expand suddenly from the neck into a considerable tumour, which could be felt through the abdominal walls in the right iliac region. Within the anal sphincter, the same tumour could be felt through the thickened wall of the gut; but at the depth of a finger's length a culminating point was found, and upon this an opening, an inch and a half wide, in which was impacted a mass of bony plates. The patient believed she had been pregnant four or five years previously, but as no child was born she had abandoned this idea. Dr. Johnston determined to remove the body. On May 8th, the patient being under chloroform, a pair of stout polypus forceps, guided by two fingers in the rectum, was used to dilate the aperture of communication between the sac and the rectum, and to extract the cranial bones piece by piece. The limbs and body were successively removed. No trace of cord or placenta was discovered. The fœtus was female, about sixteen inches long, and so much decomposed that the cranial and several other bones were denuded. The weight of the mass removed was about three pounds and a half. The sac contracted notably. About six ounces of blood, and a greater quantity of putrid ooze followed. The patient recovered favourably. Dr. Johnston satisfied himself by subsequent examination that the fœtal sac was distinct from the uterus, although adherent to it.

3. Dr. Chévillon relates a case of extra-uterine pregnancy cured after six years by gastrotomy. The patient had borne two children. In her thirty-sixth year she became pregnant a third time, in October, 1842; she received a blow from a fist, being then in her second month, in the right belly, which was followed by acute pains, inability to move, persistent dysuria, and great weakness. After four and a half months distinct fœtal movements appeared; and in nine months labour-pains, which, however, after eight days' duration, ended in nothing, so that the midwife concluded there was no pregnancy. The patient, however, still held to her belief, and felt the fœtal movements for two months longer, when the symptoms ceased. From January to March, 1847, menstruation appeared, then ceased; and the patient began to fall off in health. In July, 1847, she went into hospital with diarrhœa and hectic fever. Then was felt in the right abdomen, extending from the navel to the pelvis, a circumscribed tumour, but a fœtus was not made out. No internal examination then made. The patient left the hospital, and returned in November, 1847. At this time an abscess

opened spontaneously in the right iliac fossa. She again left the hospital in December, and did nothing until February 1, 1848, when Dr. Chévillon was called in. He recognised a fœtus in the abdominal tumour, saw fœtid pus escape, and a bone—a scapula—pressing against the opening. At the same time putrid masses were discharged by stool. The abscess had burst both laterally and into the rectum. Dr. Chévillon widened the opening, and withdrew the separate bones swimming in pus. The sac was large, with thick strong walls, and reached downwards into the pelvis. The point of rupture into the rectum could not be detected. Injections were made. The bones belonged to a mature fœtus. The fistulæ healed, and the patient completely recovered. It is conjectured that the child lived two months beyond maturity; that the abdominal gestation was probably secondary. [Looking at the history of many of these cases of extra-uterine gestation, several of which have been published from time to time in these reports, it appears desirable to consider whether the operation of gastrotomy might not more frequently be resorted to with advantage.—Rep.]

4. Dr. Shedd's case of hydrorrhœa uterina is an interesting contribution to what must still be regarded as an obscure pathological phenomenon. The patient had given birth to five children. In her first labour the waters "broke" on Monday morning, and continued to pass away gradually and daily, until Friday night, when she was delivered. She was not conscious that any bag of water broke during her labour. In her second pregnancy she carried her waters till the expulsive stage of labour. In her third, she lost her waters three weeks before delivery. At first the discharge was sudden and abundant; it continued to pass away gradually quite to the time of labour. At labour the usual bag of waters presented itself, and broke under the operation of Dr. Shedd. Child healthy, weight ten pounds and a half. The fourth gestation and labour normal throughout. In her fifth pregnancy everything seemed right until February 9th, thirty weeks from the last catamenia, when her waters broke and passed away quite freely. On the 11th and 13th, waters also passed largely. During this time she suffered much pain in the back. No farther discharge took place until March 4th; then the waters broke again and gushed away, at first in large quantities, afterwards gradually. Considerable pain in the back; premature labour anticipated. The os uteri was not dilated or dilatable. Two weeks passed; then another still more copious discharge, saturating thoroughly three or four sheets. Pain in the back considerable. Three days later another discharge. On March 28th another bursting of the floods. On the 30th the os uteri was dilated to the extent of about two inches; very soft. A very abundant show was also present. Felt pain in her back, but unlike normal pain of labour.

Previous to all the above discharges the patient became very much enlarged, "bloated" as she thought, about the abdomen. After each evacuation the "bloat" subsided, except her proper enlargement. No other evidence of ascites or anasarca. At this time ballottement several times performed with great ease and distinctness. On April 7th, having been a week without discharge, the "bloat" was very large, but lower in the abdomen. Health good. On the 10th another bursting, more violent than ever; fifteen minutes afterwards a strong labour-pain set in. She was delivered of a healthy child weighing ten pounds. She recovered speedily.

III. Labour.

1. *A New Midwifery Forceps, having a Sliding Pivot to prevent Compression of the Fœtal Sheath.* By George J. Elliot, Jun., M.D. (Pamphlet, 1858.)
2. *On the Contractions of the Uterus.* By John Christie, M.D. (Edinburgh Med. Journ., December, 1858.)
3. *Complete Rotation of the Fœtus on itself during Extraction; Child living.* By Dr. G. Lauth. (Gaz. Méd. de Strasbourg, No. 8, 1858.)

1. Dr. Elliot, of New York, describes a new midwifery forceps, one character of which deserves attention. In order to obviate compression upon the child's head, he has contrived a sliding pivot, which is easily moved along the handles, admitting of being fixed so as to keep the handles at any desired distance apart. With this pivot it is of course impossible, by any amount of pressure on the handles, to approximate the blades beyond the point at which they are separated by the intervening head. As Dr. Elliot contends for a far more liberal range of forceps-operations than is admitted in this country, and as he further contends that the chief use of the instrument is as a tractor, the introduction of his contrivance to prevent compression is peculiarly desirable to him.

In order to render the forceps applicable in a great variety of cases, especially when the head is floating above the pelvic brim, and when even the os is undilated, Dr. Elliot prefers, with some modifications, Dr. Simpson's form. The instrument he describes is $15\frac{1}{4}$ inches long; extreme width between blades $2\frac{11}{16}$ in.; length of blades, which have a slight pelvic curve, $6\frac{3}{4}$ in.; the width of the fenestra only $\frac{13}{16}$ths of an inch. The blades are very thin, the handles long and powerful. In one of the cases recorded in illustration the use of this instrument seems not to have been justified. The patient had convulsions, with albuminuria; an absolutely unyielding os and cervix. The convulsions were kept in abeyance by chloroform; three gallons of warm water were injected against the os, but it remained "as rigid as a board." The os was now incised on one side, and one blade of a rather heavy forceps passed through, the other blade could not be passed. Dr. Elliot then, having divided the other side of the os, introduced his forceps and delivered. The child was dead, and the mother died the same night. By craniotomy, with or without incision of the os uteri, in all probability the mother's life might have been saved.

2. The mode in which the uterus contracts to expel the child is still a subject of controversy. Dr. Christie, who has made numerous careful observations during labour, by keeping a finger in the os uteri, the other hand on the fundus externally, denies altogether the accuracy of Wigand's view, adopted by E. Rigby and Tyler Smith. He denies also the "peristaltic" nature of the contraction. He says that the whole organ is in contraction simultaneously. So far from the contraction beginning at the cervix, as Wigand says, Dr. Christie describes as follows the order of events that constitute a true pain. First, the fundus contracts. Secondly, the contraction, which each instant becoming stronger in the fundus, passes into the body of the uterus, thirdly, terminates by becoming manifest in the cervix. Coincidently with the beginning of contraction of the fundus uteri, the bag of membranes begins to be distended; and this, wrongly construed, it is which led Wigand and others to imagine that the head "at first rises." This, Dr. Christie says, cannot occur, because the persisting contraction of the fundus presses the head down.

3. Dr. Lauth, called to deliver a woman in labour with her fourth child, finds one foot outside the vulva. It was the right foot, with the heel looking directly forwards and upwards. He concluded that the back was directed forwards. M. Lauth therefore drew down gently by this leg in the direction of the axis of the pelvis; no progress, however; he pulled down with more force, and indication of descent was perceived. He was preparing to disengage the left thigh from under the pubes, when to his great surprise a quite opposite movement took place: the leg he held turned gradually upon itself, the heel went backwards. This rotation continued until the heel came round to the pubes again. At this time the left thigh had come down. The groin was seized, and delivery completed. The child was very large; the head remained sometime at the outlet, but respiration was perfectly established.

MEDICAL INTELIGENCE.

Medical Peerages.

The question of elevating medical men to the Peerage, which has recently begun to be ventilated both in our own periodicals and in political journals, is one that affects society at large, even more than the medical profession. Were it only a selfish consideration,—did the realization of what many of us desire in this direction only involve the immediate advancement of personal or professional ambition, we should be content to leave things as they are. Neither social eminence nor wealth are the allurements which induce the young man to join the ranks of our profession, and neither social eminence nor wealth will obtain for the country greater zeal, greater self-sacrifice, greater purity of motive, than are to be found among the twenty thousand medical practitioners that form the army of offence and defence against the incursions made by disease upon our fellow-countrymen. Nothing but an abiding love of their calling could have secured to the British public the ill-requited services of the men whose names grace the roll of our profession. The records of our hospitals and dispensaries, the annals of our workhouses and of our poor, bear witness that it is something besides honour and emolument which cheers the toil or rouses and animates the hope of the medical man. Were it not for the intrinsic interest that attaches both to the study and daily practice of the healing art; the ever-changing phases under which we observe man in every sphere of life; the intimate relations between the spiritual and corporeal which, ever and anon, are brought under our observation; the opening up of the secret springs of human action; none would be found but the meanest to submit to the drudgery of the doctor's life. Our very professional existence is a proof of the futility of the charge which would brand us with materialism. It is not the materialist who devotes the best years of his life to his country, for a pittance which scarcely serves to preserve that life, without the cheering prospect of the goal which the members of other professions may look to; it is not the materialist who sees and feels that there is a reward in the danger warded off from him who did not even appreciate its presence, in the smile or pressure of the hand of him who possesses no other means of acknowledging a service rendered. Our readers know that we are not presenting them with an overdrawn picture; but, though we may work from a higher motive than that of any immediately contingent reward, that in no way diminishes our claim to that reward if it is deserved. We may doubt whether the bestowal of Peerages will create a larger amount of zeal for science, or a warmer glow for the best interests of our countrymen. But we feel assured that our sphere of utility and our power of doing good may be enlarged by the admission of medical men into the legislature. We hold that our countrymen owe to the medical profession a large debt of gratitude. We are strongly of opinion that to keep back from us a share in the highest honours which it is in the power of the State to bestow, is an act of injustice which ought to be annulled. We are firmly convinced, not that we shall be personally benefited by the admission to the House of Peers of any number of our body, but that, as every just deed brings fruit to the doer, so the State will receive ample return for apportioning to the medical profession a definite position in the first legislative body of the kingdom. Highly as we should approve of the bestowal of a Peerage upon any eminent member of the profession upon personal grounds, we conceive that, however much this might reflect honour upon the body from which he had risen, everything would conspire to direct his energies into a new channel. It is rather because the body politic requires information upon sanitary principles; it is rather as a recognition of the important political sphere of medical science, that we should wish to see life Peerages created, bearing a similar relation to the medical as the episcopal bench do to the clerical profession. Who can deny that the advancement of civilization is largely dependent upon the just appreciation of those subjects which it is the province of the medical man to study and develope? Who that peruses our parliamentary debates, with any knowledge of these questions, but feels the want of some authoritative expression of laws, which science has learned to read, but which lack expression in our Houses of Parliament? It is vain to reply that the House of Commons is open to the medical profession as it is to the law. Gladly as we should hail the more frequent presence of medical men in the Lower House, we need not stay to point out the circumstances which so much facilitate the combination of the legislative and professional functions on the part of the busy lawyer; we altogether dissent from the principle expressed in the Epilogue to the last Westminster play,

> Juncta Foro Curia semper est!

still, the admission into that House is a matter of individual enterprise, to which it must in a great measure be left. This is not so in regard to the House of Peers. Besides, were we at liberty to enter more fully into the question, we should have many arguments to adduce to show that the dignity of science, and the objects to be gained in regard to sanitary legislation, would be better consulted by the creation of a certain number of Medical Peerages than by the election of a large number of medical M.P's.

The admission to the House of Lords must be a free act of the Sovereign. Were members of the medical profession to become recipients of that honour, it might be rendered at once a graceful act of recognition of personal services, no less than an acknowledgment of the just claims of the medical profession as a body; it would be a victory of modern civilization over barbaric precedent; it would be regarded by twenty thousand members of an arduous and enlightened profession as a proof of the solicitude with which the Queen views the manner in which the medical men of Great Britain guard the interests confided to their care.

The Council of Medical Education and Registration.

The first acts of the Medical Council have been of a kind to inspire full confidence in the wisdom of that body. It is evident that while the spirit that animates its members is one of zeal for the truest interests of the profession, they fully appreciate the necessity of recognising the ruling principle of British freedom and progress.

Their first duty necessarily consisted in the election of a President, and they do not appear to have hesitated as to the propriety of placing at their head the man who has long virtually occupied the first rank among us, and now holds that position by the free choice of the representatives of medical science belonging to these realms, a choice that has been fully ratified by their constituents. The second act of the Council was a proof of the aptitude for business possessed by the members, inasmuch as they appointed a committee to estimate the funds likely to be at their disposal, and the expenditure necessary to carry out the provisions of the Act; the third initiated the preparation of a British Pharmacopœia, thus giving to the whole public the assurance of the earnestness of their determination at once to carry out the provisions of the Act; and the fourth, perhaps the most important of all, was to determine upon the motion of Sir Charles Hastings, seconded by Sir James Clark: "That the minutes of each meeting of Council as well as all notices of motions be printed, and transmitted to each member of the Council." This resolution is a guarantee for the permanent and successful development of the Medical Council. There was no discussion about first principles, but resolute and prompt action. There was no pledge to secrecy—that certain handmaid of misrule and despotism—but a frank and open challenge to their countrymen to criticise and discuss the acts of the Council. These noble-minded men felt that any attempt at concealment would frustrate all chances of growth and strength, and lay the seeds of endless discontent and suspicion, fraught with trouble and bitterness. We do not think that this great act of the Council—an act from which they never will be able to recede—has received that praise which it merits; it disarms malevolence, and it commands respect, trust, and gratitude. The next important act of the Council was the appointment of a Registrar; and here too we cannot but yield our approval, for among the many eminent men who offered their services, none could be regarded as superior to the gentleman selected in professional standing, and probably none equalled him in intimate acquaintance with medical legislation. Where so much was done wisely and well, it may seem invidious to raise any objection; but we cannot but express a regret that the salary of the Registrar has been fixed at a sum which must be below anything like an equivalent for what will be expected of him. After appointing an Executive Committee, consisting of the members resident in London, the General Council adjourned to August 3rd, 1859. Anxiously shall we watch the labours of the executive and other committees which are appointed for the transaction of business in the interval. In thus briefly alluding to this first phase of the Medical Council, we would only express a hope that they may continue to work in the same spirit in which they have begun. As a matter of justice we put upon record the names of the eminent men who have composed this first Medical Council of the United Profession of Great Britain:

President	Sir Benj. Brodie, Bart.
For the Royal College of Physicians of London	Dr. Thos. Watson.
The Royal College of Surgeons of England	Jos. Henry Green, Esq.
The Apothecaries' Society of London	John Nussey, Esq.
The University of Oxford	Dr. H. W. Acland.
The University of Cambridge	Dr. H. J. H. Bond.
The University of Durham	Dr. Dennis Embleton.
The University of London	Dr. John Storrar.

The Royal College of Physicians, Edinburgh	Dr. ALEXANDER WOOD.
The Royal College of Surgeons, Edinburgh	Dr. ANDREW WOOD.
The Faculty of Physicians and Surgeons, Glasgow	Dr. JAMES WATSON.
The Universities of Aberdeen and Edinburgh	JAMES SYME, Esq.
The Universities of Glasgow and St. Andrew's	Dr. J. A. LAWRIE.
The King's and Queen's College of Physicians in Ireland	Dr. AQUILLA SMITH.
The Royal College of Surgeons of Ireland	Dr. ROBERT C. WILLIAMS.
The Apothecaries' Hall of Ireland	Dr. CHARLES H. LEET.
The University of Dublin	Dr. JAMES APJOHN.
The Queen's University of Ireland	Dr. JOHN D. CORRIGAN.
Nominated by her Majesty, with the advice of her Privy Council	Sir JAMES CLARK, Bart. Sir CHARLES HASTINGS. WILLIAM LAWRENCE, Esq. T. PRIDGIN TEALE, Esq. Dr. ROBERT CHRISTISON. Dr. WILLIAM STOKES.

Dr. FRANCIS HAWKINS, *Registrar.*

The Obstetrical Society of London.

Dublin and Edinburgh have acquired some scientific fame through the working and teachings of their Obstetric Societies. Down to the close of 1858, London, with its teeming population, yielding upwards of 80,000 deliveries annually, with its thirteen schools of midwifery, its numerous eminent obstetric practitioners, had no society devoted to obstetrics. It can hardly be doubted that the great metropolis has, owing to this want, lost somewhat of the *prestige* to which she is entitled as a foremost school of obstetrics. At length, Dr. Tyler Smith applying the spur, almost all the leading obstetricians of London and the provinces have determined to unite and form an English Obstetric Society. The new year witnesses its inauguration. On the 16th December last, a strong gathering took place at Freemasons' Hall, Dr. Rigby in the chair. The first resolution, declaring the expediency of instituting a Society for the promotion of knowledge in all that relates to obstetrics and the diseases of women and children, and that such a Society be now founded under the name of the Obstetrical Society of London, was moved by Dr. Tyler Smith and seconded by Dr. Granville. It was cordially and unanimously received. Dr. Granville explained why a former Society, established in 1825, had ceased to exist. That Society had for its paramount object the social and professional emancipation of the practitioners in midwifery, who then occupied a very unworthy position. That object in great measure obtained, the organization broke up, the interest taken by those who were the active movers in the scientific progress of their art being too small to induce them to keep the society alive for this purpose. Now, however, the prospect is far different; scientific activity is the predominant feature; and with the unparalleled field of experience which London displays, there seems no reason to doubt of the success of the new Society.

The following is a list of the officers appointed:—

Honorary President:—Sir Charles Locock, Bart., M.D.

President:—Edward Rigby, M.D.

Vice-Presidents:—Robert Barnes, M.D.; Samuel Berry, F.R.C.S.E., Birmingham; Lawson Cape, M.D.; A. B. Granville, M.D.; J. C. W. Lever, M.D.; Edward Murphy, M.D.; Henry Oldham, M.D.; Thomas Radford, M.D., Manchester; W. Tyler Smith, M.D; Charles Waller, M.D.

Council:—James Allen, Esq., York; E. Batty, Esq., Liverpool; Edgar Barker, F.R.C.S.E.; C. Metcalfe Babington, M.D.; I. Baker Brown, F.R.C.S.E.; Walter J. Bryant, F.R.C.S.E; J. Butler, F.R.C.S.E.; Jos. Cholmondeley, Esq.; J. Hall Davis, M.D.; George D. Gibb, M.D.; S. W. J. Merriman, M.D.; F. W. Mackenzie, M.D.; J. T. Musgrave, Esq.; W. O. Priestly, M.D.; C. H. F. Routh, M.D.; Spencer Wells, F.R.C.S.E.; R. U. West, M.D., Alford, Lincolnshire; James Whitehead, M.D., Manchester.

Treasurer:—W. Tyler Smith, M.D.

Honorary Secretaries:—Graily Hewitt, M.D.; T. H. Tanner, M.D.

BOOKS RECEIVED FOR REVIEW.

Thirty-first Annual Report of the Directors of James Murray's Royal Asylum for Lunatics, near Perth. June, 1858.

Ophthalmic Hospital Reports, and Journal of the Royal London Ophthalmic Hospital. No. IV.

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Annual Report of the Grant Medical College, Bombay. Twelfth Year. Bombay, 1858.

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A Guide to the Treatment of Diseases of the Skin. By Thomas Hunt, F.R.C.S. Third edition. 1858. pp. 216.

An Expository Lexicon of the Terms, Ancient and Modern, in Medical and General Science. By R. G. Mayne, M.D, Part VII. London, 1858.

Report on the Progress of Ophthalmic Medicine and Surgery. By Furneaux Jordan, M.R.C.S. Birmingham, 1858. (Reprint.)

Dr. Lucas Championnière's Journal of Practical Medicine and Surgery. English Edition, translated under the direction of the Chief Editor. By Dr. M'Carthy, M.D., and A Spiers, M.D. Paris, June 30, 1858.

Report on Sewage and Sewage Gases. By Henry Letheby, M.B., F.L.S. London, 1858.

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On Dropsy connected with Disease of the Kidneys. By W. R. Basham, M.D. London, 1858. pp. 241.

Illustrations of Difficult Parturition. By J. H. Davis, M.D. London, 1858.

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Nutrition in Health and Disease. By James Henry Bennet, M.D. London, 1858. pp 210.

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THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

APRIL, 1859.

PART FIRST.

Analytical and Critical Reviews.

Review I.

History of Civilization in England. By Henry Thomas Buckle. Vol. I.—*London*, 1857. pp. 854.

It is with the more real satisfaction that we commence this notice by congratulating Mr. Buckle on the talent and learning displayed in the volume before us, because, with this general and merited approbation, there is yet, if not in the entire novelty of his views, at least in the new lights in which he disposes them, as well as in the difficulties and the wide suggestiveness naturally inherent in his subject, and the bold and uncompromising way in which he considers it, much which may be deemed well fitted to lead afterwards to the expression of minor dissents. Mr. Buckle has startled the literary world by laying before it the first, and still only fragmentary, instalment of an undertaking so gigantic in its design and proportions, that we shall with difficulty find its parallel in these our degenerate days, when men are prone to run after distinction too hastily to bear heavy burdens, or to contemplate severe labours, in its pursuit. A few brief months ago, and no name lay in deeper obscurity than that of our author: now, both the continents ring with it familiarly, and the leading periodicals, as well of Europe as of America, have joined in bestowing upon his volume that attention which is a tribute justly due to the genius displayed in it, though doubtless still without that unanimity of assent to its doctrines which he resolutely claims for them, but which it would be sanguine indeed to anticipate. To his mother he simply dedicates this the first volume of his first work; but it is a work for which he had made earnest preparation, and it has achieved fame for him at once.

It is essential to such a body of facts and doctrines as that evolved in the volume of Mr. Buckle, that these should be, and possibly under no narrow sense, differently interpreted by different classes of readers and thinkers. In attempting an abstract of them, it shall be our endeavour in the first place to present them under that aspect in which we conceive that the writer himself actually regarded them; thus giving a view of them unwarped, if that be really possible, by our own subjectivity, or by the confusion which would infallibly be introduced through a premature balancing between the notions or definitions of conflicting metaphysical systems. This accomplished, we shall be entitled to permit ourselves a freer range; and, having allowed the author to enunciate his opinions, we shall be able to follow them by our brief comment, weighing the value of his doctrines, and testing them by such illustrations and objections as occur to us. Even at this early stage, it may be proper to premise that the portion of Mr. Buckle's work which he has now issued, although entitled a 'History of Civilization in England,' is, in fact, only introductory to that subject; and that so comprehensive is his scheme, and with such cautious consideration does he contemplate its treatment, that he announces his intention of occupying still two other volumes with merely preliminary facts and discussions, in order that, the necessary basis of principles and generalizations being first fully established, the reader may finally enter upon the main topic with a just understanding of the scope and spirit of his design. What may be anticipated as to the bulk and solidity of an edifice, the foundations of which are laid thus deeply and extensively, we are as yet scarcely in a position to determine. Its conception, however, looms before us magnificently in the future; and if, along with the idea of grandeur in its proportions, and beauty and strength in its materials, there appears meanwhile an air also of dreaminess and indistinctness in its outline, we must attribute this to the vastness of a project, for the successful execution of which the existing stores of human learning seem almost neither sufficiently ripe nor abundant, even could a single human intellect be hoped to prove fully adequate to the task of wielding them. Unquestionably, when we express our earnest desire that Mr. Buckle may be enabled to complete his important work, on the scale in which he has commenced it, this is tantamount to wishing him no ordinary amount of mental vigour, continued through no limited duration of effort. We trust, however, that he will achieve the utmost possible success in his labour; and assuredly, when he has happily arrived at its completion, and has at last committed it to posterity in that weighty and eloquent peroration with which we know that it will conclude, there is, notwithstanding all its perhaps necessarily inherent defects, no other modern author, on any kindred subject, who will be able to look back at a nobler consummation.

Mr. Buckle introduces his subject by remarking, that of all the great branches of human knowledge, history is that upon which most has been written, and which has always been most popular. Glancing at the description and quality of the facts which have been customarily used for its illustration, the author proceeds to stigmatize the defective manner in which these have been ordinarily bent to their purpose. Thus, hardly any one has attempted to combine the parts of history into a whole, and to ascertain the way in which they are connected with each other. Since the early part of the eighteenth century, indeed, a few great thinkers have arisen, who have made something like a systematic effort to raise the subject to a higher value, by investigating it according to those exhaustive methods which in other branches of knowledge have proved successful, and through which alone empirical observations can be raised to scientific truths. Still, he proceeds, little more has been hitherto accomplished than to open up more cheering prospects; while scarcely anything has been actually effected towards discovering the principles which govern the character and destiny of nations. Our acquaintance with history being thus so imperfect, while our materials are so numerous, it seemed desirable that something should be done on a scale far larger than had hitherto been attempted; and that a strenuous effort should be made to bring up this great department of inquiry to a level with other departments, in order that we may maintain the balance and harmony of our knowledge. It is in this spirit that the work has been conceived. Whoever is at all acquainted with what has been effected during the last two centuries must be aware, the author further tells us, that every generation demonstrates some events to be regular and predictable, which the preceding generation had declared to be irregular and unpredictable;

so that the marked tendency of advancing civilization is to strengthen our belief in the universality of order, method, and law. Hence, the expectation of discovering regularity in the midst of confusion is so familiar to scientific men, that amongst the most eminent of them it becomes an article of faith; and if the same expectation is not generally found among historians, it must be ascribed partly to their being of inferior ability to the investigators of nature, and partly to the greater complexity of those social phenomena with which their studies are concerned.

The author next proposes to inquire into the foundation of what he announces as the common opinion, that history must always remain in its present empirical state; and with reference to this he cites the existence of two doctrines, which appear to represent different stages of civilization. According to the first doctrine, every event is single and isolated, and is merely considered as the result of a blind chance: according to the second, men believe that every event is linked to its antecedent by an inevitable connexion, and that thus the whole world forms a necessary chain, in which, indeed, each man, playing his separate part, may hold his place, but can by no means determine what that place shall be. Tribes in the agricultural state, perceiving that what they sow, that likewise do they reap, are the first to take cognizance of this regular uniformity of sequence, through which gradually arises a dim idea of the stability of events; and thus begins to dawn upon the mind a faint conception of what at a later period are called the Laws of Nature, an increasing perception of which, in the ordinary march of society, destroys the doctrine of Chance, and replaces it by that of Necessary Connexion. From these two doctrines of Chance and Necessity have arisen, in all probability, the subsequent dogmas of Free Will and Predestination; the doctrine of Chance, in the external world, corresponding, according to the view of the author, with that of Free Will in the internal world, while the other doctrine of Necessary Connexion appears alike analogous to that of Predestination; the only difference, he subjoins, being that the first is a development by the metaphysician, the second by the theologian. There is, however, it is added, a growing opinion among the more advanced European thinkers, that both doctrines are wrong, or, at all events, that we have hitherto no sufficient evidence of their truth.

The idea of the supremacy of the free will, the author continues, involves two assumptions, of which the first is possibly true, the second unquestionably false. These assumptions are, that there is an independent faculty called consciousness, and that the dictates of that faculty are infallible. But it is by no means certain that consciousness is not rather a condition of the mind than a faculty; while, if it be a faculty, we have the evidence of all history to prove its extreme fallibility, at least as to the truth, if not as to the actuality, of its testimony. The doctrine of predestination he summarily sets aside as a barren hypothesis, because, being beyond the province of our knowledge, we have no means of ascertaining either its truth or its falsehood. Pursuing his argument, he adduces this uncertainty regarding the existence of consciousness, as an independent faculty, and the manner in which that faculty, if it exist, has contradicted its own suggestions, as two of the many reasons which have long convinced him that metaphysics will never be raised to a science by the ordinary method of observing individual minds: but that its study can only be successfully prosecuted by the deductive application of laws which must be discovered historically; that is to say, which must be evolved through an examination of the whole of those vast phenomena which the long course of human affairs presents to our view. He considers it fortunate, however, for the object of his work, that the believer in the possibility of a science of history is not called upon to adhere to either the doctrine of predestined events or that of freedom of the will; limiting the latter, somewhat singularly, to the idea of a cause of action residing in the mind, and exerting itself independently of motives. He expects, on the other hand, at this stage of the inquiry, the concession of the following positions: That, when we perform an action, we perform it in consequence of some motive or motives; that those motives are the results of some antecedents; and that, therefore, if we could become acquainted with the whole of the antecedents, and with all the laws of their movements, we could with unerring certainty predict the whole of their immediate results. Inferring, then, that the actions of men, being thus determined solely by their antecedents, must have a character of uniformity—that is to say, must, under precisely the same circumstances, always issue in precisely the same results, he arrives further at the conclusion that, as all antecedents are either in the mind or out of it, all variations in the results, or, in other words, all the changes of which history is full, must be the fruit of a double action; an action of external phenomena upon the mind, and another action of the mind upon the phenomena.

Such, according to Mr. Buckle, are the materials out of which a philosophic history can alone be constructed. We have, on the one hand, man modifying nature, and, on the other, nature modifying man, while out of this reciprocal modification all events must spring; the problem immediately before us being to ascertain the method of discovering the laws of the double modification. This leads to a collateral inquiry, as to which of the two modifications is the more important; that is to say, whether the thoughts and desires of men are more influenced by physical phenomena, or whether the physical phenomena are more influenced by them. Meanwhile, as confirmatory of the views already enunciated, the author offers a few of what he deems the most decided proofs, which we now possess, of the regularity with which mental phenomena succeed each other. These proofs are gathered from statistical facts, bearing upon the entire moral conduct of a given society, and acquired through the accumulation of masses of details, and the application of the doctrine of averages. Thus, the apparent regularity of the ratios of the recurrence of murder, of the numbers accused of other descriptions of crime, the fluctuations in which have been found to have been actually smaller than the synchronous fluctuations in mortality, and of the prevalence of suicide, are all held by the author as parallel chains of evidence, jointly forcing upon us the conclusion that the offences of men are the product, not so much of the individual offender, as of the state of society into which that individual is thrown. Nor is it merely the crimes of men which he adduces as marked by this uniformity of sequence. Marriages, as having a fixed and definite relation between their number and the price of corn, or even the aberrations of memory, as indicated by the number of letters forgotten to be directed when committed to the post-office, are given as other proofs of that regularity of events which is at once the key and the basis of history. But, beyond the proofs of our actions being regulated by law, and which are thus derivable from statistics, there are other powerful means of establishing truth, through which history may be cultivated; and especially, we ought not to infer that, because the physical sciences have not yet been applied to it, they are therefore inapplicable. On the contrary, since history deals with the actions of men, and since these actions are merely the product of a collision between internal and external phenomena, it becomes necessary to examine the comparative importance of these phenomena; to inquire into the extent to which their laws are known; and to ascertain the relative resources for future discovery possessed by these two great classes, the students of the mind and the students of nature.

The fundamental views of the author thus formalized, he proceeds to inquire into what those physical agents are by which the human race is most powerfully influenced, and he finds that they may be classed under four heads: viz., Climate, Food, Soil, and the General Aspects of Nature. The three former affect primarily the general organization of society: the last exerts a powerful influence, producing its principal results by exciting the imagination. Through the agency of these, spring up those important differences which are usually held to constitute original varieties of race, the existence of which, as sources of distinction, he holds to be altogether hypothetical. Grouping together the effects produced by one or other of the three first agencies, he points to the accumulation of wealth as the earliest and most important; because upon wealth follows leisure, and without leisure there can be no knowledge. The degree of rapidity with which this ensues, will be solely regulated by the physical peculiarities of the country; for these will modify, firstly, the energy and regularity with which labour is conducted, and, secondly, the returns made to that labour by the bounty of nature. While climate, moreover, exerts an influence, either by enervating the labourer or invigorating him, it produces a further effect on the regularity of his general habits. Thus, the long winter of the North breaks the chain of industry of the people, which suffers relatively an equivalent interruption in the parching heats of the Southern summer; so that, continued industry being impracticable in either, the national temperament acquires a fitful and capricious character, the appropriate examples of which he finds in the populations of Norway and Sweden on the one hand, and of Spain and Portugal on the other. Turning to Asia, he remarks that civilization has always been confined to that vast tract, stretching from China to the eastern shores of the Mediterranean, where a rich alluvial soil has secured that wealth which is the first element of intellectual progress. In Africa, again, the portion irrigated by the waters of the Nile, and enriched by its deposits of fertile loam yielding the most abundant returns to labour, made a like accumulation a necessary sequence, and gave rise to the civilization of ancient Egypt, the only country which has been able to emerge from the barbarism imposed upon the rest of that continent by the penury of nature. It appears thus manifest, that, of the primary causes of civilization, the fertility of the soil is that which has exercised the most influence in the ancient world. But in European civilization it is not soil, but climate, the other great cause, that has been the most powerful: in the former case, the effect depending on the relation between the soil and its produce; in the latter, upon that between the climate and the labourer. Of these two classes of relations, the first, being the less complicated, is the less liable to disturbance, and therefore was brought the sooner into play. Hence it is, that, in the march of civilization, the priority is unquestionably due to the most fertile parts of Asia and Africa. But this form of civilization, although the earliest, is not the best or most permanent; for the only progress which is really effective can be shown to depend, not upon the bounty of nature, but upon the energy of man. It follows, in this way, that the civilization of Europe, which, in its earliest stage, was governed by climate, has shown a capacity of development unknown to those civilizations which were originated by soil.

As to the distribution of wealth, that our author is also inclined to attribute entirely, in a very early stage of society, to the influence of physical laws. Wages being the price paid for labour, the rate of wages must, like the price of all other commodities, vary according to the changes in the market. Supposing, therefore, that in any country there is a given amount of wealth to be divided between employers and workmen, every increase in the number of the workmen will tend to lessen the average reward each can receive. But of all the physical agents by which the increase of the labouring classes is affected, that of abundant food is the most active and universal. As to the quality of food, men in a hot country require a smaller amount of non-azotized, or rather of highly carbonized, food to keep up their heat; and less also of azotized food, because there is less habit of bodily exertion to cause waste. In cold countries, on the other hand, men are compelled to eat more than in hot ones; while their food, from its necessary quality, is less easy to obtain, and requires a greater expenditure of labour. This implied degree of exertion, under a rigorous climate, in itself imposes the necessity of a diet of a more abundantly carbonized and nutritious quality, in order to sustain heat and compensate attrition; while the amount of toil thus enforced necessarily enhances the cost. There is thus a strong and constant tendency in hot countries for wages to be low, in cold countries for them to be high. Now, in all the more ancient civilizations, seated in hot climates, among which the author includes those in America, the rate of wages being very low, the condition of the labouring classes was very depressed. Selecting Hindostan as a first example, he adverts to the great heat of the climate there as bringing into play that law, by virtue of which the ordinary food is of an oxygenous rather than of a carbonaceous character; thus compelling the adoption of a diet rather from the vegetable than from the animal world, the chief constituent of which was in this instance rice, a grain ranking, according to the author, as the most nutritive, while one of the most productive, of all the cerealia. Hence a rapid growth of the population, a redundance of labour, and that consequent inequality in the distribution of wealth which tends to render the upper classes enormously rich, and the lower classes miserably poor, fitting the latter to be only either slaves themselves, or to be led to battle to make slaves of others. This slavery in India, the author takes occasion to designate afterwards as an abject, eternal slavery, which, while it constituted the state of the great body of the people, was a state to which they were doomed by physical laws utterly impossible for them to resist. Thus, the annals of this people furnish no example of their having turned upon their rulers.

There is not an instance on record, Mr. Buckle continues, of any tropical country, in which, wealth having been extensively accumulated, the people have escaped this fate; no instance in which the heat of the climate has not caused an abundance of food, and the abundance of food an unequal distribution, first of wealth, and then of political and social power. Nor was it until civilization arose in Europe, where everything worthy of the name has originated, that other physical laws came into operation, and therefore other results were produced. Hence, the line of argument which he has just applied to India, he considers as equally applicable to Egypt, Mexico, and Peru. The date, as well from the quality of its nutriment as from the profusion of its returns, is the analogue in Africa to the rice in Asia. But in the Said, the most fertile part of Egypt, there is an additional food, the dhourra, which is even more productive than either of the others. Thus liberally provided, there was no restraint to the growth of a population on the banks of the Nile, and therefore the laws already noticed came into uncontrolled operation. Independent of the testimony of Herodotus and Diodorus Siculus, the only two ancient writers cited by Mr. Buckle as speaking of Egypt from personal knowledge, and who are both agreed respecting the rapid increase of the people, and the servile condition into which they had fallen, we have, he holds, in the mere appearance of those huge and costly buildings which are still standing proof enough of what was the state of the nation that erected them. To raise structures so stupendous, and yet so useless, as the pyramids, there must have been tyranny on the part of the rulers, and slavery on the part of the people. Of the extent of this slavery, the debasement with which it was accompanied, and the reckless prodigality of life which it promoted, the author adduces proofs which he considers sufficiently ample. Turning now to the New World, he meets with fresh testimony to the accuracy of the preceding views. The only parts of America which, before the arrival of the Europeans, were in some degree civilized, were Mexico and Peru; to which may probably be added the long tract which stretches from the South of America to the Isthmus of Panama. The accounts we possess render it at least highly probable, that we have, in Central America, the ancient seat of a civilization in all essential points similar to that of India and Egypt. But of Peru and Mexico, our details are more positive and abundant; and here, too, we have a highly interesting degree of further proof of the extraordinary, and indeed irresistible, force with which the fortunes of man have been controlled by the powers of nature.

The first circumstance by which we must be struck, Mr. Buckle continues, is that in America, as in Asia and Africa, all the original civilizations were seated in hot countries, the whole of Peru proper being within the southern tropic, the whole of Central America and Mexico within the northern tropic. Now, besides the result effected by a hot climate of diminishing the requirements of a people, and thus producing a very unequal distribution of wealth and power, there is another way in which the average temperature of a country affects its civilization, and this may be more clearly illustrated in America than elsewhere. Indeed, in the New World, the scale on which Nature works, being much larger than in the Old, and her forces being more overpowering, it is evident that her operations on mankind may be studied with greater advantage than in countries where she is weaker, and where, therefore, the consequences of her movements are less conspicuous. In Brazil, these forces are too exuberant, and transgress their just balance; but in Peru they are confined within manageable limits, and the result becomes commensurate with what has been observed in Asia and Africa.

Looking, then, not merely at the exuberance, but also what may be called the manageability of Nature, the author finds that it is in Mexico and Peru where this combination has most happily occurred, while at the same time the heat of the climate has brought into play those other laws by which, as he has attempted to show, all the early civilizations were influenced. Here the place of rice and of dates is taken by maize—a grain corresponding with these, he remarks, because eminently the product of a hot climate, and distinguished there by its extraordinary productiveness. A people who derived their sustenance from a plant of such singular fecundity, and aided besides by the produce of the banana, had little need to exercise their industrial energies, while at the same time they had every opportunity of increasing their numbers. Hence, the physical conditions being similar, the civilizations of Mexico and Peru became strictly analogous with those of India and Egypt; and there arose the same despotic power on the part of the upper classes, with the same contemptible subservience on the part of the lower, leading alike to the erection of immense yet useless buildings, such as no country could produce unless the labour of the people were ill-paid and ill-directed. Such, Mr. Buckle exclaims, with reference to this part of his subject, is the wonderful regularity which history, when comprehensively studied, presents to our view.

The influences of similar conditions of climate, food, and soil having been thus simultaneously passed under review, and proved, according to our author's belief, to be as identical in their nature and effects as they were irresistible in their powers, wherever predominating, the degree of importance to be attached to his fourth head, the General Aspects of Nature, is next considered. As it has been seen that the three former agencies mainly concern the accumulation and distribution of wealth, and therefore the material interests of man, so the latter will be found to concern the accumulation and distribution of thought, and therefore man's intellectual interests. The Aspects of Nature, the author considers, may be arranged into two classes: those fitted to excite the imagination, and those addressing themselves to the understanding. There is a prevailing disproportion between the action of these faculties, which advancing civilization tends to remedy; but which is still conspicuous in the greater sway of the imagination, as shown in every country, not more in the superstitions of the vulgar than in the poetic reverence for antiquity of the educated classes. Man is timid before the force and majesty of Nature, but confident where her works are feeble. It is in proof of this that the author instances with approbation the remark, that the grandeur of the mountain scenery in the Tyrol has been observed to have imbued the minds of the natives with fear, and to have caused the invention of many superstitious legends; while he appeals for testimony besides to the remarkable fact, that all the great early civilizations, keeping in view the quality of these civilizations, were situated within and immediately adjoining the tropics, where Nature's aspects are the most sublime, dangerous, and terrible. Earthquakes are among the most striking of the physical events which give to man the lesson of insecurity; and, reverting to Europe, in the relative prevalence of these in the Spanish and Portuguese peninsula, as well as in Italy, he finds a ground, not only for the dominance of superstition among the inhabitants, but for their distinction in the arts and polite literature, rather than in the sciences, the imagination being stimulated under these circumstances beyond the intellect. Thus pre-eminently oppressed and harassed, the old tropical civilisations, suffering besides under a greater precariousness of sickness and mortality, had to struggle with innumerable difficulties; and there were naturally associations engendered in the mind which allowed the imagination to run into license, infused into the people a spirit of reverence instead of a spirit of inquiry, and encouraged a disposition to neglect the investigation of natural causes, and ascribe events to the operation of supernatural ones. In short, everything conspired here to increase the authority of the imagination, and weaken the authority of the reason.

But in Europe, on the other hand, we find a law diametrically opposed to this, by virtue of which the tendency of natural phenomena is, on the whole, to limit the imagination and embolden the understanding. The author illustrates this by an appeal to the literature, religion, and art of Greece, in contradistinction to those of India. The works of Nature, which in India are of startling magnitude, are in Greece far smaller, feebler, and in every way less threatening to man. The difficulties of life, which in the one only appeared soluble by referring to supernatural causes, were less intrusive, dangerous, and mysterious in the other. Thus in Greece everything tended to exalt the dignity of man, and in India to depress it. Hence, for the first time in the history of the world, we find in Greece the imagination in some degree tempered and confined by the understanding, though it may remain still doubtful whether or not the balance was accurately adjusted. A more graceful mythology, too, takes the place of the religion of the Hindu, which, like that of Central America, was based upon a system of complete and unmitigated terror. Even in the style of architecture the same principle was at work, the dangers of the tropical civilization being here more suggestive of the infinite, while the safety of the European civilization was more suggestive of the finite.

These principles established, Mr. Buckle proceeds to a closer view of the influences regulating the early growth of civilization in Europe. The powers of Nature having worked immense mischief in extra-European civilization, he finds that great relative advantages attended its advance within our continent; the tendency being here to subordinate, he repeats, nature to man, as out of it he alleges the reverse. This is to him the basis of the philosophy of history, since it suggests the important consideration, that if we would understand, for instance, the history of India, we must make the external world our first study, because it has influenced man more than man has influenced it. If, on the other hand, we would understand the history of a country like France or England, we must make man our principal study, because, nature being comparatively weak, every step in the great progress has increased the dominion of the human mind over the agencies of the external world. The pressure of these is still immense, even where man's power has reached the highest point, but it diminishes in each succeeding generation, because our increasing knowledge enables us, not so much to control nature further as to foretell her movements. How successful our efforts have been is evident from the fact, that the number of inevitable dangers have become constantly fewer; plague, leprosy, and famine in its worst forms have ceased, and the average duration of life is extended.

Hence follows a leading corollary, that, if the measure of civilization be the triumph of mind over external agents, then of two classes of laws, the mental class must be more important than the physical; and the discovery of the laws of European history becomes resolved into the discovery of the laws of the human mind. But the methods hitherto pursued for the latter purpose have been universally faulty, for metaphysicians have heretofore studied only the individual operations of their own minds, while their course should have been to have so enlarged their survey, extending it over many minds, as to have eliminated the intervening disturbances. Two methods of metaphysical investigation have been adopted, both of which are equally obvious, yet both of which lead to entirely different results. According to the first method, or sensationalism, the inquirer begins by examining his sensations. According to the second method, or idealism, he begins by examining his ideas. But these methods inevitably lead to conclusions diametrically opposed to each other, while they exhaust at the same time the customary resources of metaphysics; for both parties agreeing that there is nothing in the mind which is not the result either of reflection or sensation, the sole final choice they have to make is between subordinating the results of sensation to the laws of reflection, or else subordinating the results of reflection to the laws of sensation, no middle term being attainable, and the cause admitting of no umpire. Hence it is that metaphysics, in our author's opinion, have proved so barren of results, and that they have verified no principle of importance, unless a very few of the laws of association, with possibly the modern theories of vision and of touch.

It appears, then, that the metaphysical method of investigation, used in accordance with the present state of our knowledge, is inadequate to the task of discovering the laws which regulate the movements of the human mind, so as to fit us to understand, through them, the conditions which govern the movements of the human race. We are therefore driven to another, and the only remaining method, according to which mental phenomena are to be studied, not simply as they are evolved in the mind of the individual observer, but as they appear in the actions of mankind at large. As an illustration of the resources possessed by the latter plan for the investigation of truth, the author selects what he designates as a beautiful instance of the regularity with which, under the most conflicting circumstances, the great laws of Nature are able to hold their course; the example to which he refers being that of the proportion kept up in the birth of the sexes. Towards the establishment and elucidation of this law, physiologists have done absolutely nothing, their success having been no greater than that of metaphysicians with reference to the laws of history; yet, at the present day, by the employment of what now seems a very natural method, that of the statistician, we are in clear possession of the truth respecting it. In suggesting an analogous method for ascertaining the laws of mental progress, the author points to the nature of this progress as twofold—Moral and Intellectual; the first having more immediate relation to our duties, the second to our knowledge. This double movement is essential to the very idea of civilization, and 'includes the entire theory of mental progress. But a question arises, now of great moment, namely, which of these two parts or elements of mental progress is the more important. It requires but a superficial acquaintance with history to be aware, that while men for the most part conform to the standard of morals and of knowledge common to the age and country in which they live, this standard is perpetually changing; and relevantly, therefore, the basis upon which it rests must also want the attribute of stability. But the primary dogmas of the moral system have in themselves undergone remarkably little change, for the thousands of years during which they have been as thoroughly understood as now; while intellectual truths, on the contrary, have been making surprising progress. If, then, all great moral systems which have exercised much influence have been necessarily, and at all times, fundamentally the same, and all great intellectual systems have been necessarily fundamentally different, the inference to be derived is immediately obvious. Since civilization is the product of moral and intellectual agencies, and since that product is constantly changing, it is evident that it cannot be regulated by the stationary agent. The only other agent is the intellectual one, and this accordingly is the real mover.

Such is the main argument by which the author's view of this part of his subject is supported, but he suggests also other and collateral circumstances as well worthy of consideration. Of these, one is, that the intellectual principle is not only far more progressive than the moral principle, but is also far more permanent in its results. Great truths are stored up; good deeds are of a more private and retiring character, and are therefore less capable of transmission. Even great public charities invariably fall into abuse and decay. Religious persecution, again, it must be admitted, is a great evil, yet, in a moral point of view, the motives of religious persecutors have been often unimpeachable, the great antagonist of intolerance being not humanity, but knowledge. Another great evil, second only in our author's opinion, to religious persecution, is war; and with regard to the moral relations of this, we know nothing now which has not been known for many centuries. Yet it is evident, our author holds, that this barbarous pursuit is steadily declining with our advance in knowledge, as must be obvious to the hastiest reader of the history of Europe; in which, until the late commencement of hostilities with Russia, peace had been maintained for nearly forty years, a circumstance unparalleled, he adds, either in our own or in other leading countries. It is, besides, highly characteristic of the actual condition of society, that this peace of unexampled length should have been first broken by two uncivilized nations. Finally, in thus alleging the paramount influence of the intellect, he does not deny that in successive generations the moral qualities may, in so far, constantly vary; but experience, he asserts, nevertheless teaches us, that as our different passions are always antagonistic to each other, they are thus so held in balance by the force of their own opposition that through them no permanent change is effected.

From all this Mr. Buckle maintains the, to him, inevitable inference, and that upon which he takes his stand, that if we wish to ascertain the conditions which regulate the progress of modern, as contradistinguished from ancient, civilization, we must seek them in the history of the amount and diffusion of intellectual knowledge; and we must further consider physical phenomena and moral principles as causing, no doubt, great aberrations in short periods, but in long periods correcting and balancing themselves, and thus leaving the intellectual laws to act uncontrolled by these inferior and subordinate agents. He even limits this scant admission with reference to the power of morals, by immediately subjoining, that although moral principles do affect nearly the whole of the actions of the individual, it is only as individual; but that we have incontrovertible proof that they produce not the least effect on mankind in the aggregate, or even on men in very large masses. Holding this to be established, Mr. Buckle now proceeds to ascertain what are the fundamental conditions of that intellectual progress to which he assigns so absorbing an influence. In pursuing this subject he introduces the three topics of religion, literature, and government, as all of vast importance, and, in the opinion of many, the prime movers of human affairs. This opinion, however, he maintains to be altogether erroneous, and he undertakes to demonstrate that each of these great forms of human development arises only as an effect of civilization, but is not a cause of it.

Into this branch of the subject we shall not attempt to follow him, by even a very cursory analysis; because the medical inquirer cannot assume its discussion as in any degree a part of his own peculiar field, however intense and vital may be its interest otherwise. For the same reason we shall be constrained to content ourselves with passing rapidly over the remainder of the volume, merely noticing that the author, after a brief inquiry into the origin of history, takes up, as more immediately bearing upon what is to be his ultimate subject, the state of historical literature in the middle ages. This topic then conducts him to an able survey of the history, more exclusively, of English literature, for a period from the middle of the sixteenth to the end of the eighteenth century. A similar outline of the history of the development of the French intellect follows, but for a period extending only to the accession to power of Louis XIV.; and this again is succeeded by a sketch of the results of what he designates as the "protective spirit" towards literature, with a comparison of these as then observable in France and in England. A subsequent chapter traces the same subject to certain of its alleged issues, and leads to a contrast between the feuds of the Fronde and the coeval English rebellion. The investigation is further continued by an examination into the influence of the same protective spirit as wielded over literature by Louis himself, reviewing the consequences of the alliance thus fostered, or imposed, between the governing and intellectual classes, and laying important mischiefs to its charge. The reaction which followed upon this, after the death of Louis, next engages his attention; and, noting the nature of its effects, the author is led to the indications which foretokened the approach of the French Revolution. After a preparatory consideration of the state of exclusively historical literature in France during the two centuries preceding that great event, the volume concludes with an inquiry into its more immediately proximate causes, as developed in the narrower period after the middle of the eighteenth century; including among these the intense intellectual and scientific movement for which France was then remarkable. The other introductory volumes, which we have mentioned as designed to follow, and which will be occupied by a survey of the civilizations of Germany, America, Scotland, and Spain, will complete the comprehensive plan of Mr. Buckle's proem; the object of which, as we have said, and as will be seen, pretends, expanded as it is, to nothing more than to discover the fundamental laws to be applied afterwards to the history of England.

We have been thus particular in our analysis of at least the initiatory part of this remarkable volume, not only because the topics on which it treats are in themselves among the weightiest that concern humanity, but because the physician, as of all men the best entitled to appropriate the celebrated *humani nihil à me alienum puto* of Chremes in Terence, must find in their discussion much that interweaves itself closely with his own special studies and pursuits. And if the physician, moreover, have any claim to those liberal attainments, or that love for scholarship, which Sir William Hamilton* somewhat splenetically denies to our profession in this country, or at least in Scotland where the learned professor chiefly observed it, the wide range traversed by Mr. Buckle will present abundant occasion for challenging his energies; and the earnest eloquence of the writer will either demand from him often an implicit acquiescence, or afford him the opportunity of showing the reason for the faith that is in himself. It would be unjust to style Mr. Buckle dogmatical; but he writes with an air of so apparently thorough conviction, is so unhesitating and forcible in his averments, and so sanguine and confident in his illustrations, that he must sometimes read warily who is unwilling to be subdued into an assent which a calm consideration afterwards would not always justify. Yet, assuredly, beyond this, his work contains many and great truths, often in all their purity, wisely and manfully enunciated, pregnant with instruction, and educed with remarkable facility and power of logical distinction. If we meet occasionally with doctrines which are little better than assumptions, or allegations of fact which are alike manifestly misapprehensions, yet still find both urged with all the ardour which should have been the attribute of something better, there should be no reprobation in the demur or denial with which we encounter them, for we are bound to consider deferentially the vastness of his theme, and the complexity and abstruseness which are inseparable from its discussion. With such topics, especially as reviewed by mental idiosyncrasies such as that which is apparent in Mr. Buckle, in the midst of much that is safe, and just, and true, there is prone to intervene a vague and dreamy yearning after the unlimited and unattainable in knowledge, which entices onwards with all the brilliancy, but with all the unsubstantiality, of a meteor. Upon the whole, nevertheless, we have to thank him for a deeply interesting and deeply suggestive work; and it is in this spirit that we subject a few of its parts to a more close examination.

If we were to attempt to distinguish between what is new in reality in Mr. Buckle's treatise, and what is merely new in position and inference, we should enter upon but an ungracious and unprofitable task. The influence, we need hardly state, of climate, soil, and food, and even of the aspects of nature, upon mankind, have long been familiar to the medical inquirer. Not one of these topics is neglected among the concise details of the father of medicine, in his familiarly-known treatise, Περὶ ’Αέρων, ‘Υδάτων, Τόπων: and, since the time of Hippocrates, down to the present day, all our leading physiologists and physicians, as well as our ethnographers, and especially all our writers on hygienics, have acknowledged the importance of the agency of this class of conditions upon man and his capabilities. If it has not been our province to apply, in any immediate way, to the mere history of civilization, the knowledge which a constant and extended observation has caused to accrue to us on this subject, the higher duty has been fulfilled of giving value to the materials of that history, through the profitable results of our experience in the investigation and supervision of man's physical relations; and through our efforts, unceasingly exercised, towards directly advancing his progress, by showing what conditions were requisite to the full development and exercise of his powers, whether with reference to the sound mind or to the sound body. Neither has the political philosopher nor the general historian always so entirely neglected this department of observation as Mr. Buckle, in certain of his preliminary remarks, would lead us to infer. When Cicero, in his 'Oration on the Agrarian Law,' tells us that the manners of men were implanted less by innate peculiarities of race, "quàm ex iis rebus, quæ ab ipsa natura loci, et à vitæ consuetudine suppeditantur; quibus alimur, et vivimus;"* and adds his illustrations of this from the character of the Carthaginians, and of the inhabitants of Campania and Liguria, he was but applying briefly, from the comprehensive stores of his knowledge of the philosophy of his age, those doctrines which Mr. Buckle has here extended over so large a space. In far more recent times, Montesquieu has shown, in his celebrated 'Esprit des Lois,' how fully he too recognised these agencies as modifying the character and spirit of laws and forms of government; and Condorcet, whose 'Esquisse d'un Tableau Historique des Progrès de l'Esprit Humain,' we are surprised not to find in Mr. Buckle's list of authors quoted, because it seems to us easy to detect its influence, direct or indirect, in his pages,† is equally decided in proclaiming their efficiency in developing and moulding the mind of man. Filangieri, too, in his remarkable treatise, while he has pointed out that Montesquieu had been himself in some measure anticipated in his views on this topic by Fontanelle, Chardin, Dubos, and Bodin, has discussed the influence of climate and soil, and local position on the social condition of mankind with conspicuous soundness and moderation; admitting for them, as he justly might, a decided, yet only a concurrent, and never an absolute power of causation.‡ As to the historian, it is seldom that, when he is treating his subject generally and scientifically, he has wholly neglected this department of inquiry, in proof of which we may refer, passing by many others, to so familiar a book as that of Rotteck,§ the thirteenth edition of which, published in 1838, i now lying before us. Yet while we thus seem to encounter in Mr. Buckle's volume long, and everywhere, acknowledged facts and doctrines, we soon discover, under his guidance, an expansiveness and originality in his method of considering them, which stamps, nevertheless, the plan and its results as peculiarly his own. It is less his merit, therefore, that he deals with new materials, than that he arranges these frequently under new aspects, and makes them the subject of unexpected, if, now and then, of somewhat questionable, inferences.

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* Discussions on Philosophy and Literature, p. 681.

Among the many excellences of Mr. Buckle's able volume, we do not hesitate at this stage to single out what appear to us three leading defects. The first is a disposition, certainly, however, neither peculiar to himself nor to any special age or school of philosophy, towards isolating causes, which can only exist as con-causes; and of imagining certain fundamental laws as separate and primary agencies, which, if they subsist at all, can yet only subsist in relation to other laws, as essential as themselves, and through a tissue of actions and re-actions so complicated and so varied, that it becomes impossible to define rigorously the influence of each. The second defect is, that Mr. Buckle's method being mainly deductive, the mental bias thence naturally ensuing, renders him prone to neglect some facts, and to misinterpret others; so that thus the stability of his generalizations is not rarely shaken bythe illustrations he himself adduces in their support, when these illustrations are subjected to a rigid scrutiny. The third defect is rather one of result than of method, from which, however, it springs consistently; and it appears in Mr. Buckle's attempt to ignore the influence of moral agencies, and even of religion and government, in ruling the course of human events, while he assigns an almost sole supremacy to the progress of the intellect, as a power apart, in promoting the advance of civilization. Beyond this, we could have wished that Mr. Buckle had seen fit to have joined, to his conspicuous familiarity with physical and metaphysical studies, a greater knowledge and love of those of the archæologist, which was indeed required for an adequate treatment of his subject. But Mr. Buckle, though necessarily drawing many of his illustrations from antiquity, has confessedly little sympathy with the past, and sees no greatness but in the future. Yet reverence for the one is but the converse of anticipation for the other; and, remembering that each can stand only on the pedestal of his own age, we should be slow to deny to our progenitors that tribute of attention, and of respect where due, which we desire from posterity; while we ought to be careful that, whether bestowed or withheld, it should only be on just grounds. We could have wished, moreover, that the influence of the ambitious phraseology of the school of Auguste Comte, or of the so-called Positive Philosophy, of which Mr. Buckle professes himself an ardent admirer, had been occasionally less apparent; for we cannot but feel how prone are many of its formulæ, with all their affected rigour of expression, to assume the aspect of hollow generalities, which palter in the ear what they cannot convey to the understanding. The tenor of what must now be only our casual remarks, will show sufficiently our grounds for these observations.

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* Oratio de Lege Agraria, contra Rullum, § 35.

† We may refer especially to pp. 7, 14, 17, and 44, of the second edition of Condorcet's treatise, where we seem to trace the indications, sometimes almost literal, of those dogmas regarding the accumulation of wealth, its tendency to the production of leisure, and the resulting division of labour, with the constancy of the general laws of nature as enabling us to foretoken the progress of coming generations, and the relations of these to the science of history, all of which Mr. Buckle has so amply developed.

‡ La Scienza della Legislazione, libro i. cap. 14–16.

§ Allgemeine Geschichte, Band i. pp. 56, 61, 62, 259.

Looking back to the early dawn of civilization, Mr. Buckle denies to the wandering tribes, living by hunting and fishing, the capacity of discerning, or even suspecting, anything like method in the arrangements of nature; and it is only, he says, when such tribes advance into the agricultural state, that they perceive, in the uniformity of sequence in their crops, the first idea of a stability in events, and the first conception of what at a later period are called the Laws of Nature. Now Mr. Buckle cannot mean here a stability of events with reference to a determining power in man's agency, for such a power it is the whole course of his subsequent argument to deny; and most of all, he denies it with reference to an inter-tropical civilization, which he reasonably admits to be the earliest arena of progress. But in whatever sense we regard the first idea of a stability in the order of events, it is very manifest that it cannot have originated in the order here imagined. The savage, still unversed in his relations to the world around him, but with the divine germ of intelligence implanted in his mind by the Creator, must necessarily detect first, in the succession of the spontaneous gifts of the soil, the regularity of that sequence which he afterwards renders available to supply his wants. It could not be till he had marked, time after time, the blossom following the bud, and the fruit following the blossom, with, perhaps, the seed sprouting into the plant, in the free lap of nature, that he could approach the idea of utilizing the result of his experience for his own advantage. But long before this, and in a far less remote and intricate sequence, he had learned the great idea of order and succession in events, in the motions of the planets, the alternations of day and night, the ebb and flow of the tides, and the revolutions of the seasons; and these were not more patent to the tiller of the soil than to the wandering denizen of the forest. In truth, and it is an important truth, but it is one which the entire scope of Mr. Buckle's views compels him to disregard, there seems to have been, among all primitive peoples, and throughout all varieties of soil and climate, one original phase of civilization, through which, as a necessary grade, all mankind, from their constituent intrinsic nature, were constrained to pass; and this identity of primary civilization, irrespective of those peculiarities of physical condition to which our author ascribes so paramount an importance, is evinced over a field of observation so wide, and in features so palpable, that it would be mere wilfulness to misread the lessons they present.

As all living is merely a constant struggle against dying, to be undeviatingly decided at the last, so we find in the form, and in the enduring quality of the materials, of certain instruments used by man to secure his sustenance or his safety, as well as in the final memorials which mark his modes of disposal of the dead, the surest and most intelligible of all early traces of his existence, for the simple reason that they are those which are the least separable from the conditions of his being. There is scarcely a country in the globe, where the arrow-head and hatchet of flint and of stone, closely identical in shape as in substance, have not been discovered as the conspicuous tokens of an originally wide-spread conformity of primitive life, at whatever era of the world, and under whatever circumstances of variation in soil or geographical position. In America, from Cape Horn to the cold North; in Europe, beside the *fiords* of Norway, and on the plain of Marathon; in Africa, in Asia, and in Australasia; weapons and implements so similar have been picked up, that we might suppose that only a few hands, with as little diversity of purpose as of skill, had shaped them all. We have a still more conspicuous, and an equally widely diffused reminiscence, in the barrows everywhere heaped up in commemoration of the dead. The tumulus of the ancient Briton, in England, has no marked difference from that of the aboriginal Scythian, scattered abundantly throughout Russia, South Siberia, and Tartary; and both of these have their near analogues in lands as remote from each other as Etruria and Java; while we find them equally closely represented in the barrows of North America, or in those farther south, in Chili and Peru. The New Zealander still rears his sepulchral heaps just as they are still raised by the Hindoo in Rajasthan; and in Yucatan, the skeleton is still found with the vase of rude pottery at its side,* as the European archæologist encounters it in Scandinavia, France, or Britain. It may be worth noting also, that in North America the Indian wayfarer has been observed to throw customarily his stone on the commemorative cairn, like the Highlander in our own country. The indications of this description of coincidence, or more than coincidence, might be widely extended, for the materials are abundant; but it is at least interesting to note in conclusion, that when the old Norse chronicler described the burial of Thorolf,† a mercenary under our King Athelstan, who fell in battle in this country, and related how his brother, Egill, took the body and washed it, and, in the fashion of the time, hollowed out the grave, and placed there Thorolf, with his dress and weapons, adorning him with his gold armlets, and finally surrounding him with stones, and heaping up earth over all, he was but detailing a manner of interment which might, in almost all its main features, have been as appropriately detailed of a chieftain in any other country, at a given stage of its civilization. The mound described as raised over Beowulf, the hero of the well-known Anglo-Saxon poem, probably of the ninth century, suggests the like associations.

It is obviously not necessary, at least for our present purpose, to extend to this parity of early customs the force of a proof of an original affinity, among races so far apart from each other in time and in position. It seems more probable that the direct nature of the recourse had of itself, in each instance, secured its adoption: and that hence the inference becomes almost irresistible, that there has existed everywhere a common gradation in civilization, through which each people has been constrained to move, however remote its period or diversified its location; with the further inference, that, therefore, the causes of this civilization must have depended, in the main, rather upon intrinsic than upon extrinsic forces, though doubtless often with modifications from a reciprocal action. At all events, any other view than this must be challengeable as a mere assumption, and rests upon no tangible evidence. That, from the earlier stages of civilization, there should be a progress onwards, is a natural result; for each generation, mingling with the survivors of that preceding it, commences with the advantage of the experience and the guidance of its fathers, and is earlier introduced to a higher training than if left to its own unaided efforts, gaining thus the opportunity of advancing ultimately to a higher perfection. This, repeated through an adequate series, introduces gradually the qualities of an improved culture as a necessary effect; if, indeed, the progress be not wilfully arrested by the self-imposition of some arbitrary standard, beyond which it may be considered a dereliction or a supererogation to pass. Thus, the American savage inculcates the lesson of a contempt for the indulgences of civilized life; and the Polynesian is slowly taught to feel the want of them. The Asiatic, on the other hand, too often limits the just growth of improvement by his habitual neglect of morality, honesty, and justice. Apart, however, from the consideration of this wilfully arrested development, we seem easily to trace the course of ordinary civilization, from the state of the hunter and fisher, through the pastoral and the agricultural condition; and thence onwards, beneath the gradually approaching lights of tradition and history, with a singular uniformity throughout in all essential particulars, at least in the early stages.

Yet even when we reach the age of tradition, in however plausible and consistent a form it may present itself, a temperate balancing of doubts continues, for all beyond this, to be the sole prudent course for the inquirer; and it is only when we gain the safer ground of contemporary historical record, that such precaution can be in some degree relaxed, though never slighted as wholly superfluous. The least uncertain of the marks of the advancing civilization of a people, after the lapse of its ruder stages, but before the age of its literature, exist in the more or less perfect remains of its artistic and architectural monuments, and the consideration of these significant and positive testimonies has not been neglected by Mr. Buckle. It is in Hindostan that he seeks the earliest proofs of the operation of physical laws upon a population, through the agencies of a hot climate and a liberal soil; but we shall ourselves prefer to turn first to Egypt, his second field of illustration, because there the relics of a remote civilization are infinitely the more striking and important, and their lessons, therefore, the more easily read. And here we have to complain of an instance of that determination to view facts only in a particular light, and to see nothing apparently but what is consistent with his own preconceptions, which we have signalised as the second of Mr. Buckle's defects; yet which we have by no means any inclination to attribute to a spirit of wilful misrepresentation, but believe to be honestly, however unfortunately, the result of his peculiar ardour and intrepidity of discussion. A population, urged into redundant growth, and profusely and easily supplied with food, suffered here rapidly, he alleges, through the necessary consequence of an unequal distribution of wealth, and the mass fell naturally into a nation of slaves, lorded over tyrannously by a few masters. The proof of this he finds in the existence of such structures, at once costly and useless, as the pyramids; and he refers besides, as we have noted in our analysis, to the authority of Herodotus and Diodorus Siculus, as having had alone, among ancient writers, the opportunity of judging of the condition of the country from personal knowledge, and as thence affording confirmation of his views.

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* Stephens' Yucatan, vol. i. pp. 276, 279; vol. ii. p. 341.

† Egill Skalligram's Saga: "Egill jardadi Thorolf."

We shall scarcely pause to defend the motives which gave origin to the stupendous memorials of the pyramids, which, regarding them as unquestionably sepulchral monuments, are to us merely the culminating phase of that early usage of rearing tumuli, often exceedingly vast, over the dead, of which we have already spoken. Neither shall we attempt to estimate closely what may be the importance, in a moral sense, of such structures, though we are far indeed from admitting that a building may not have an immense æsthetical value, independent of any notion of mere utility. We shall not even inquire too nicely into what may be the worth of the testimony of the Greek authorities cited by our author, as to the special civil condition of the builders of monuments, regarding the precise era of certain of which chronologists, including Diodorus Siculus himself,* vary to the extent of more than two thousand years: but we cannot meanwhile admit that the picture which Herodotus presents of the Egyptians of his own day, is one of the gross and slavish degradation which the hypothesis of Mr. Buckle renders it necessary for him to deduce from it; and as to the past history, we are expressly told that, till the close of the reign of Rhampsinitus, there was a perfect distribution of justice through all Egypt, which prospered greatly,† only two subsequent reigns being especially stigmatized for their oppression; while, with regard to the great works in architecture and engineering conducted under Sesostris, many of which deserved no depreciatory brand, but were of the highest utility, it is distinctly mentioned that the labourers were not an enslaved native population,‡ but were the prisoners brought in multitudes from the countries the monarch had subdued.

Mr. Buckle's other authority, Diodorus Siculus, we find no less distinct in stating that the first kings of Egypt were not despotic, or without restriction in their government; that they were, on the contrary, bound by the general laws; that their personal attendants were neither slaves, nor even ordinary servants, but the sons of the chiefs of the hierarchy; that the Egyptian husbandmen were not rack-rented, which Mr. Buckle conceives for them as an absolute sequel, but held their lands at easy rates from the sovereign, the priests, and the military, and were the then most skilful agriculturists in the world; and that the whole people, in short, flourished under a just rule, lived in the height of happiness, grew exceeding rich, and beautified their country, a truth we must at least still recognise, with countless magnificent works.§ Reading the social state of a people in the extensively preserved remains of its artistic labours, we have difficulty in believing that there could be anything really grovelling in the position of those whose grandeur of conception and skill of execution produced, among a host of others, the splendours of the temple of Karnac; and we can better reconcile these with our apprehension of the tenor of the testimony of Herodotus and Diodorus, than with that which our author has found means to discover in them. Doubtless, the Egyptians possessed slaves; but so did almost all the world long after their era, and slavery has been no badge of any distinct order of civilization. We need not ourselves look back with any peculiar satisfaction to the once debased condition of the serfs and villeins within our own country; or own anything else, even now, than a feeling of regret, that we should still have brothers, speaking our own tongue, who boast themselves of the freest nation of the earth, and who yet fret with resentment when invited to shrink from a similar contamination. The Spaniard, by the way, is no favourite with Mr. Buckle; yet no serf, it is worth remembering, or mere *adscriptus glebæ*, ever existed in old Leon and Castile.

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* Bibliotheca Historica, lib. i. cap. § v.

† Euterpe, ii. § 124.

Ibid. § 108.

§ Bibliotheca Historica, lib. i. cap. vi.

But to revert now, for an instant, to Hindostan, which Mr. Buckle has selected, doubtless justly, but not unchallengeably, as having a right to boast the most ancient civilization of Asia, we would ask why the analogous physical conditions here have not led earlier to the development of a description of architecture, such as he has construed as their proper characteristic in Egypt. It is true that India possesses now its remains of a vast and singular, and sometimes stately and ornate, style of architecture; but it is not less true that we have there no remains to which it is safe to assign an earlier date than the third century before Christ; that even then these remains are few and insignificant; that it is not till seven or eight centuries later that the principal Hindu style, never so massive and durable as the Egyptian, sprang up to the south, in the countries mainly occupied by the Tamul races; and that in the valley of the Ganges, Mr. Buckle's selected spot for the elaboration of a primeval civilization, there are, with a few exceptions, and those not extending beyond the limits of one or two remoter provinces, no genuine Hindu buildings which can be referred to a date before the period of the Mahometan conquest. Where, then, are the proofs here of that early and tyrannical abuse of labour, which Mr. Buckle alleges as the essential concomitant of the luxuriance of inter-tropical culture? And now let us turn to Peru and Mexico, and examine into the consistent bearing of his illustrations there. In the singular forms of architecture in these countries, he finds, too, his fitting themes for exposition; and into these we willingly follow him, led by our own differently directed views of their quality and import. The Mexicans seem, like the early Egyptians, to have risen spontaneously from the idea of the tumulus of earth to that of the pyramid of a better defined outline; and to have had their corresponding conceptions of grandeur in construction, not in the element of form, but alike in that of duration, though with a very inferior success to the other, as well from defect in artistic skill as in strength and solidity of material. Yet they were able to rear structures sufficient, in their perfect condition, to strike with astonishment the early Spanish adventurers, familiar as they were with piles so stately as the minsters of Seville and Toledo, in their own country; and still great enough in their ruins to justify this admiration in the eyes of the modern traveller. In Peru, again, we encounter a style of architecture essentially different. The pyramid of the Toltec finds no exact analogue in the more southern country; but the remains of other descriptions of buildings, among which we may specify those of the fortifications of Cuzco, the ancient capital, containing stones which weigh each from fifteen to twenty tons, attest admirably the skill and energy of the former population. There was some approach to justice, then, in alleging here a similar condition of early civilization to that of the old Egyptians; and Mr. Buckle, in sweeping, doubtless, his eye across the planisphere, and marking a tropical, or nearly tropical position for both, or possibly seduced by a like inadvertency in Montesquieu,* springs easily to a conclusion for which he was prepared by his preconceptions, that the causes of the similarity lay in the nearly identical physical conditions to which the people were subjected.

Now, perhaps it will be difficult to find elsewhere an instance in which a really able, extensively accomplished, and profoundly reflecting author has been allured so easily into an error so palpable. But we suspect we have here, after all, only another example of the common mistake of plunging deeply in search of what lies floating on the surface. It has never occurred to Mr. Buckle to consider in how great a degree the conditions of climate are merely relative; and that geographical position is here but one element, which the closer relations of topographical sight are able to modify within almost every imaginable limit. If we have thus the minor variations of a maritime, an insular, a littoral, and an inland, or continental, climate within the same geographical range, we have, more marked than all, the gradations dependent upon different degrees of elevation above the ocean level.

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* Esprit des Lois, lib. xvii. cap. 2.

Thus the torrid, the temperate, and the frozen zones may be encountered in rapid transition into each other, in an exceedingly near contiguity of space. And this it is, unmarked by our author, which at once removes the condition of the ancient Toltec, or the Peruvian, from all identity with that of the Egyptian and Hindu; and therefore from all comparisons or inferences which are deduced upon so baseless a foundation. We know that the city of Mexico, situated on the table lands which constituted the centre and seat of the ancient empire, and having within it, or near it, the ruins of the great temple of Mixitli and those at Tezcuco, stands at an elevation of between seven and eight thousand feet above the level of the sea; while the imposing remains of the pyramid at Cholula are only a few hundred feet lower. These, and the similar table lands, are designated by the present inhabitants as the Cool Countries (*Tierras frias*), and have a temperature never exceeding 75° in the shade, even in summer. Clavigero, who lived thirty-nine years in the country, describes it, and especially what he styles the temperate region of the Mexican valley, as enjoying the advantages of the colder lands without their inconveniences, and of the hotter without their annoyances;* and he speaks elsewhere of all those countries of the interior, which were the main seats of the population, as blessed with a mild and benign climate, though with occasional, but exceedingly temporary, frost and snow in the months of December, January, and February.

If we turn to Peru, again, we find a still more marked divergence from the physical conditions of the seats of Eastern civilization. The ancient Tahuantinsuyu, the country of the Incas, occupied an elevated table land, now known as the Sierra, raised at a mean height of about twelve thousand feet above the sea-level. Quito, placed at an altitude of nine thousand five hundred feet, stands at one extremity of that immense causeway, assuredly not a useless construction, the work of the old Peruvians, which stretches thence to Cuzco, the original capital of Manco Capac, lying at its other extremity; the latter city being situated at the greater height of eleven thousand three hundred and eighty feet, and thus within four thousand feet of the line of perpetual snow. All the important relics of the people are on this mountain region, and the most extensive are at Cuzco. If Mr. Buckle had chosen to advert to the older historians of the Spanish conquerors, he would have found, among the rest, in Augustin de Zarate,† a view widely different from his own of the soil and vegetation, with the personal qualities, habits, &c., of the natives of the table land of Peru; while Francisco de Xeres would have told him how Pizarro and his troops, very naturally, suffered on one occasion from severe cold in that elevated region: "Que en Castillo, no hace maior frio, que en esta Sierra." Where, then, is the resemblance here to the valleys of the Nile and the Ganges, which could justify the allegation of a common causation dependent upon climatic influences? Nor are we more content with the rigid accuracy of many other points in Mr. Buckle's parallel. We have no right, for instance, to speak of the civilization here as possessing that antiquity which should have characterized it, had it been the constrained result of the operation of solely physical causes, such as he has imagined them. It may be impossible to affix an exact date to all those architectural remains in Mexico, from which alone we gather our least doubtful notions of the pristine condition of its people: but, using what authorities we have, we can scarcely assign to the works of the Toltecs at Cholula an age exceeding twelve hundred years; while the city of Mexico is said to have been founded only in 1325, and the great temple to have been completed there by the Aztecs, so late as 1486. In Peru, on the other hand, the records do not pretend to an earlier origin than about from three to four hundred years before the arrival of the Spaniards towards the commencement of the sixteenth century. Nor, further, with reference to those pitiless human sacrifices which Mr. Buckle, in so far justly, denounces as once prevalent, have we the slightest grounds for believing that they were ever practised by the Toltecs, or by their immediate successors the Chichimecos, or that they were perpetrated by even the Aztecs prior to the fourteenth century. The benevolent Las Casas, indeed, assures us that they occurred in a few parts only;* and in Peru, we have many authorities, besides the excellent one of Garcilaso de la Vega,† to prove that they were utterly unknown. Thus of three centres of ancient civilization, selected by Mr. Buckle as identical in their types because dependent upon closely similar physical conditions, the two older were really somewhat analogous in their conditions, yet have differed materially in their development; and the later has been wholly different in its conditions, yet may be admitted to have made a singularly near approach in its development. We might be permitted to refer further, perhaps, to that part of Africa, now the seat of our Cape Colony, which bears nearly the same relation to the Southern, that Egypt bears to the Northern Tropic; and where there is manifestly no such difference in the soil and climate otherwise, as can explain the immemorial existence of the grovelling debasement of the Hottentot and Bosjeman in the one, and the past magnificence of the realm of Rameses and Sesostris in the other. In Eastern Asia, again, utilitarian China has its own peculiar, but well-marked, discrepancies. With this in part negation, and in part contradiction, we may now judge, therefore, with what special extent of good fortune or caution it is that Mr. Buckle, pointing to these American countries, and referring back to the East, exults, as we have seen, in the wonderful regularity which history presents to our view when comprehensively studied. Desisting even from our impugnment otherwise of the entire accuracy of the materials of his illustrations, we have plainly demonstrated, and in his own fields of research, that what he considers as closely identical forms of civilization may subsist, and have subsisted, under the most widely diverging conditions of soil and climate; while, beyond this, similar conditions of soil and climate have been the seats of the most opposite states of civilization.

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* Historia Antigua de Megico (trad. por J. J. de Mora), tomo i, pp. 11, 71.

† Historia del Descubrimiento y Conquista de la Provincia del Peru, lib. i. cap. viii.

As to the author's fourth directing influence, or that of the General Aspects of Nature, we must own that the same necessity occurs to us of impeaching, as well the substantialness as the appropriate quality of certain of the selected illustrations. When the author tells us, for example, that the mountain-scenery of the Tyrol, and, we assume, of the rest of the Alpine districts, has been observed to imbue the minds of the natives with fear and superstition, we begin to wonder if a Hofer has really ever existed: or abandoning, however unwillingly, the story of Tell and his comrades as a myth, we feel constrained to doubt if the proud memories of Morgarten, Sempach, and Murten, where the mailed chivalry of Austria and Burgundy went down before the strokes of the mountaineers of Switzerland, be anything else than a dream. We are inclined to question, with the same kind of uneasiness, whether the country of Calvin, and the home of Beza, can be really of necessity the peculiar seat of superstition; or whether, to retreat to remoter times, the legends of the ancient dalesmen demanded among them a wilder credulity than that of the believers elsewhere in Dis or Teuth, or in Thor and Odin. Again, when, as another and a different illustration of the effects of the grand and terrible in nature, he points to the frequency of earthquakes in Spain and Portugal, and in Italy, as rendering superstition rife, imagination predominant, and intellectual vigour rare, we are compelled to demur on the double ground that destructive earthquakes are neither common nor broadly felt over these countries, and that in Italy, at least, the roll of scientific men has been ever as proud in fame as it has been wide in duration. As to Spain, we have noted that the author, in a prior passage, also further depreciates this country, and then along with Sweden, on the score of the parching summer heat of the one, and the long winter of the other, leading to irregularity of industry, and hence to a generally capricious instability and fickleness of the national character. But Spain, in the days of Ferdinand and Isabella, as well as of their immediate successor, took the lead of Europe in energy and enterprise; and there was little instability of mood in those hardy adventurers in the new Continent, who, to use the words of their historian, De Solis, "led to their field of duty a few troops of soldiers, styling them armies, and not without justice, when we consider at what they aimed and what they accomplished." As to Sweden, though Mr. Buckle's view of the Scandinavian character may be not altogether unjustified, it is at least greatly exaggerated. Europe will not soon forget the audacity and perseverance of the Norse Vikings, and the impression made by their prowess on so many kingdoms; and still less will the achievements of Gustavus Adolphus and his followers, or the scientific glories of a Linnæus and a Berzelius, become speedily an unheeded record. Such a population has been more competently judged by its native historian, Geijer (*Svea Rikes Häfder*), when he shows us, in an eloquent passage which we would willingly give at greater length that only a hardy, laborious, and intelligent race could maintain its existence among the vales and mountains of Scandinavia, and points to their history as proving the moral strength and buoyancy which their habits of life engender and sustain. Not less strong, or less thoroughly grounded, and therefore not more partial, is the corresponding testimony of writers so judicious as Strinnholm and Af Forsell. Dissatisfied with this description of comment, we would gladly have seen, sometimes, that an ational memory, or a national character, could have availed itself better with Mr. Buckle against the force of a dogma.

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* Controversia entre el Obispo don fray Bartolome de las Casas y el Doter Gines de Sepulveda, p. 107. Sevilla, 1552.
† Commentarios Reales de los Incas, lib. 1. cap. 8.

We are not more inclined to agree with our author, that the force of the imagination, comparing, as he does, the manifestations of the intellect of Greece with those of the intellect of India, and accepting both as the criteria for Europe and the East, is stronger in the latter than in the former. On the contrary, the difference, which is a striking one, is to us chiefly one of quality and direction, and not of power or extent; or, if we are to establish the latter kind of gradation between them, it would be certainly to the imagination of Greece that we should assign the superiority. The graceful mythology of the Hellenes, drawing within its range all human passions, all aspirations, and all the more beautiful and striking among natural objects, and striving at an individual conception of the Godhead in each, was infinitely wider and higher in its scope than the mythology of India, and more prodigally and genially inventive in its forms. Nor, in poetry, need we fear to compare the creative genius of a Homer with that of a Kalidasa; while, if the warm East have loved to pour forth its scientific notions in verse, neither have we always wanted here our poet-physicians and our poet-philosophers. But we think our author errs in placing generally the imaginative or inventive faculty in so direct an antagonism with the intellectual or logical one, or at least he errs in applying to an era what may be only true of an individual; for, though the qualities rarely meet in perfection in a single mind, they may, which is more to our purpose, and not seldom do, advance collaterally in the same period of culture. There was no wide interval of time or local position between a Bacon and a Shakespeare, a Milton and a Hobbes, a Newton and a Dryden, a Descartes and a Corneille, a Kant and a Göthe, and many others; and we have imagination enough here to cast into the shade the brightest creations of the East, where much, too, ever verged upon that grotesquely monstrous, which, if it belong to the fancy, belongs to it only in its lowest manifestation. Claiming, therefore, for Europe a twofold supremacy in this respect over the Asiatic civilization, or a superiority in imagination as well as in intellect, we proceed to consider what has been the effect of this upon the general progress; keeping in view our author's division into a moral and an intellectual progress as something prospectively different, and his concluding allegation, that the influence of the latter becomes ultimately paramount, and that of the former, zero. In approaching this, and only briefly, as our last topic for remark, we necessarily pass over much besides to which we would willingly have directed more especial attention; just as we have left unsaid much of what crowded upon us with reference to those parts which have been already rapidly surveyed. But Mr.

Buckle's themes are so diversified, so widely suggestive, and so uniformly interesting, that we must be constrained to a narrow principle of selection, if we would not extend our comment to the limits of his text.

Our attention is immediately and naturally arrested here by the too absolute separation instituted by the author between the operations of the moral and the intellectual faculties. He scarcely gives us room to suppose that these can be considered as integrant parts of one harmoniously acting mind; or that, if it be indeed possible to conceive the intelligence acting independently of the moral power, as unhappily we may, to conceive the moral power acting independently of the intelligence, is at least utterly impossible. Hence morals, so far from being merely now, as Mr. Buckle maintains, what they have been from all appreciable antiquity, differ not only materially in their diffusion, in as far as they may be confined to the few or extended over the many, but also in their practical range, in as far as they necessarily multiply and complicate themselves with the increasing and intermingling interests and duties of a growing civilization. Throughout this range it is manifest that the rules and principles of morality must be incessantly and necessarily referred to the intellect, which alone can give them an efficient existence; and moral science or art, term it as one will, ranks only as a branch of psychology. In the first stage of society, and therefore in the oldest of all forms of morals, these, so far from being in perfection, will exist in their lowest purity; for the primary individual duty of self-preservation, so immediate as to be peremptory, and so strong as to be instinctive, will then naturally predominate. Indeed, truth and justice, being the basis of ethics, while dissimulation is often the necessary element of self-preservation, and force its instrument, it follows that, in the savage, or barbarous, or imperfect states of civilization, morals, in their true sense, can scarcely be said to exist. When duty, however, rises beyond that relating to self, first to include that to family, then to society, then to the state, and lastly to comprehend the relations between one state and another, or when, from merely private, we begin to recognise also social, political, and international duties, if we are still assuredly only within the field of morals, they have become far more pure and expansive: and it is finally nothing more than the art of applying them which constitutes the basis of our systems of laws, for laws are but morals in action. Once admitting these stages, and their admission is unavoidable, we must admit also an infinitude of gradations between each, ere the paramount and absorbing influence of the first be relinquished, and the happiness of the individual be known and felt in the happiness of the race. It is not, therefore, the bare recognition of moral truths, but the extent of their diffusion at a given period, that tells upon civilization. That the leading axioms were anciently as thoroughly understood by a few as they are now, might be true of a certain era; but it would be ridiculous to assert that they have been always alike prevalently recognised among the people at large, among the doers and not the thinkers, as among the schools of the philosophers. And even now, with our riper civilization, to state that the rules of morality are the same everywhere and at all times, that they are diffused among all with the same unvarying purity, and modified among all by the same qualifications, would be equally opposed to the most obvious realities: and yet all this, and even more than this, would be required to substantiate the extraordinary dogma in the treatise. Thus the few rules of ethics referred to by Mr. Buckle are neither the whole of ethics, nor the first form of ethics. If, on the other hand, for their due practical application the intervention of the intelligence be indispensable, the question as to whether morals or intellect have the greater influence, where both have thus an essential influence, loses all its importance. We have here, as with all mental phenomena, a conjunct, as well as a consecutive action; and we plainly ought not to separate that arbitrarily, and for the sake of discussion, which we can neither separate in reality, nor even conceive as separated.

Where there is this straining after isolation and precision, to anything but the advantage of precision, in the elaboration of Mr. Buckle's principles, we need not be surprised to find a similar defect in the accuracy of his illustrations. Nothing, to revert to another of his opinions, he tells us, is now known with reference to the relative justice or injustice of wars which has not been known for many centuries; yet, he remarks, till the late hostilities near the Euxine, we had remained at peace for the unparalleled period of nearly forty years; and he advances this as an irrefragable proof of the truth of his argument, that all great progress depends upon the growth of the intelligence. But where has he discovered his fact? If he had referred, indeed, to wars exclusively within the limits of Britain, he might have extended this period to a far wider duration; but if we are to pass beyond these limits, yet still speak only of ourselves, what are we to say of our many wars in India, Burmah, China, Afghanistan, Persia, South Africa, Canada, and elsewhere, all within the last forty years, and all prior to the important contest in the Crimea? Or, if we are to look abroad to the remainder of civilized Europe, what country within the same period has been wholly free from contention, and how many of the greatest capitals have escaped being deluged with blood? If Paris, Berlin, Brussels, Vienna, Venice, Rome, Dresden, and other cities have not received any new lights with regard to the moral evils of war, neither, certainly, have they discovered, in the growth of their intelligence, any remarkable amount of those checks to it which Mr. Buckle has so singularly imagined. But on this point we have our own ready comment: that knowledge, as knowledge, is nothing; and that it is knowledge in reciprocal relation to the pure moral principle, and in its broadest practical application, that in such matters alone avails. Even Mr. Buckle admits, as we have seen, that moral principles affect nearly the whole of the actions of the individual, though not in the least, as he subjoins, those of mankind in the aggregate. But the aggregate is nothing more than an association of individuals, at whom, synthetically or analytically, we must begin at first or arrive at last. In history, if the abstract have its curiosity and interest, it is the concrete which has its value and importance. What strange philosophy, then, is this, which attempts the impossible feat of stripping man of his identity, and losing him in the mass? We know the very great value of averages to science, as science; but, even in this respect, we cannot admit that Mr. Buckle has always selected his averages of the best quality, applied to them the necessary corrections, or limited their use to its proper scope. There are myriads of examples, beyond medicine as within medicine, where the general rule must remain subordinate to the individual instance. The very idea of establishing an average infers that of an inequality; and it becomes often of far more importance to ascertain a maximum and a minimum range, than to determine the intermediate expression of either, by constraining the facts into this kind of bed of Procrustes.

Thus, if Mr. Buckle had examined more closely one of his selected sources of illustration, the statistics of suicide, he would have found, confessedly imperfect as these are in Paris, Berlin, St. Petersburg, and elsewhere, not a fixed proportion, denoting what he terms a law, but a rapidly ascending ratio, during a little more than the last half-century; and even in London, the discrepancies between the various successive years are greater than suits his leading hypothesis of a compulsory sequence of events, a dogma in other respects, we need hardly add, as old as the Stoical philosophy. But Mr. Buckle manifestly not rarely confounds a law with the fact which would be the product of that law, if it were really established. A law of nature is the constant and regular order through which a cause operates in producing a given category of effects, but the effects are not the law. That certain qualities in marshy districts produce ague is to the physician a law: but the individual marsh is only a condition, which may, or may not, be persistent; and the individual ague is the attendant fact, which resolves itself accordingly. And so with the moral marsh, which may be drained, the moral miasmata, which may be dispelled, and the moral pestilence dissipated; yet here, or in the conversely possible event of the individual man being shielded from its operation, the general law would remain unimpeached; for the fact, we repeat, is not the law. There is a disregard of this vital distinction in most of our author's illustrations from statistics, on which he founds so zealously. The proportion of crimes, as of suicides, is no fixed or general law, but a resulting fact from certain states of morals, under which, on a narrow view, the semblance of a law is constituted. It changes, however, with changing conditions, and the facts accordingly are modified or disappear.

We shall have had little success, while thus examining and countervailing the views of Mr. Buckle, if we have given no indications of what are our own. We have attributed to him, and we think justly, a proneness to a too decided isolation of causes, which can have no independent action; we have shown that he is not exempt from the charge of an omission of certain facts, and a misapprehension or misapplication of others, which suffice to break up certain of his leading categories, and which prove their incompetency to embrace all that he demands from them; and we have, besides, demonstrated the unreasonableness of any attempt to separate the idea of the practical application of morals from the exercise of the intelligence, thus, refusing, by implication, to concede to the latter an autocracy of influence with reference to the progress of civilization. We do not deny, because it would be ridiculous to deny, the partial, and usually even very important, agency of climate and soil on the condition of mankind; but we do not admit these to be absolute at any time, just as we cannot admit them, or any other special influences, to be alike persistent through all times, a conclusion which would force us to claim the glories of Nineveh for the hovels of Nebbi Yunus, or to discover, in the Athens of Otho, the intellectual and æsthetic splendours of that of Pericles. So far from ignoring the action of moral causes upon the growth of culture, we see in them rather its main and indispensable condition; because morality infers the laws which regulate the just conduct of human life in the individual and in his relations, and the conduct of human life, so understood, is to us the very essence of civilization. Thus judging, we cannot, moreover, with Mr. Buckle, disjoin the idea of civilization from religion, because nowhere can morals be wholly separated from religion, and in Christianity we encounter them in their widest and purest expression. Nor can we view it apart from Government; because law is, or should be, only morals systematized and enforced: or from literature, because the lowest aspect of this renders it the record, and the highest the minister, of the progress of mankind. We claim for man a free agency, and do not admit that he is the slave of events, further than that he is not endowed with omnipotency: but he can choose at least the means of adapting himself to events, and of selecting his course with reference to them; and that he is possessed of this power is proved not only by his own consciousness, but by the fact that scarcely any two individuals, though placed in the same circumstances, will be found to pursue an identical line of conduct. Thus, as truth is never single, but exists only in its relations, it is to a mixed career of influences that man becomes subjected; and cause and concause, secondary cause and reactive cause, in manifold but not wholly inextricable complexity, make up, with the spirit of intelligence more or less directing through all, the sum of existence in the individual and in society. And as the past, with its instructions, becomes our mirror, throwing its light forward into the darkness of the future, we stretch beyond time to realize more and more closely the conception of our greater and ultimate relations to eternity; a culminating idea, at once so magnificent and so exalting, that his could only be a degraded spirit, and hardened in degradation, who, for the sake of its responsibilities, would forego its grandeur.

In thus concisely summing up our own views, while recording the expression of our partial dissent from those of Mr. Buckle, we remain not the less willing to renew that homage of respect for the general talent and learning of his treatise which we have already justly and cordially offered. We should fail, indeed, in duty to ourselves, if we did not again express the deep gratification we have received from its perusal. An author who discusses, ardently and eloquently, questions of supreme interest to humanity, who frequently carries us with him thoughtfully into far remote tracts of disquisition, or guides us aptly through richly diversified fields of research, and who thus wakens up our resources, giving a stimulus to the mind which is at once animating and healthful, has laid us unquestionably under no common obligation. With all his occasional faults, no one has ever been more successful in adapting the graces of a fervid declamation to the formal phraseology of the abstract sciences, or has given to dialectics a truer inspiration and charm.

We are glad, meanwhile, on every consideration, that Mr. Buckle has seen fit to issue even the introductory part of his work in fragmentary portions. The comments each of these will receive in succession, cannot fail wholly in their influence on what is to follow; and the author will gain from them, at least, a nearer view of the difficulties and objections he must strive to overcome, before he can expect a tolerably wide acquiescence in his doctrines. Should he be enabled to complete his vast design, and to commit it finally to posterity, even though the finished labour should still lie under the exceptional blemishes we have pointed out, thinking men, on the approach of that future which Mr. Buckle honours, and when he himself has become a part of that past which it is his too prevailing spirit to contemn, will doubtless own of him admiringly, that in those days there were giants in the land.

Review II.

On the Organs of Vision: their Anatomy and Physiology. By Thomas Nunneley, F.R.C.S.E., Lecturer on Surgery in the Leeds School of Medicine, &c. Eight Plates and 179 Woodcuts.—*London*, 1858. 8vo. pp. xxiv., 373.

Mr. Nunneley divides his work into seven chapters, embracing,—1, sensation, and the senses generally; 2, ideas derived from vision; 3, light, and its laws applicable to vision; 4, structure of the human eye and its appendages; 5, comparative anatomy of the organ of vision; 6, eyes of extinct animals; 7, physiology of vision. The idea of the work is good—namely, to provide an introduction for medical students to the study of the eye; but the execution of it is by no means equal to the subject, involving as it does many defects, a few of the more prominent of which we consider it indispensable briefly to specify. Mr. Nunneley gives abundant tokens in many cases of assiduous research, the results of which in establishing any new or important facts we would gladly indicate. Oftener, however, his conclusions are of a negative sort; he fails in detecting what has been seen and described by his predecessors, and candidly admits that this is the case.

Objects of Sense.—To several of the notions thrown out in the first chapter we are disposed to object. Thus, Mr. Nunneley tells us (p. 2) that "we can imagine modifications and extensions in the powers of our present senses, as, for instance, in that of light, of having the faculty of seeing through opaque bodies." Now, as light is the object of perception to the sense of sight, and as light cannot pass through opaque bodies, it is impossible to conceive such a modification of this sense as Mr. Nunneley supposes.

"In seeing," says our author, "it is not the tangible and large distant landscape, but the very different and small picture thrown upon the retina itself, that is the immediate and only object of perception." (p. 7.) The fact is, however, that the picture thrown upon the retina is not the object of perception. The direct impression made by the concentrated rays of light upon that nervous membrane is the immediate and only object of perception in the act of vision. The nature of that vital impression is unknown, but the picture is a mere optical, mechanical, and unavoidable coincidence, which takes no part in the production of sensation.

Method.—To enter, as Mr. Nunneley has done in his second and third chapters, on the metaphysics of vision and on optics, involving the ideas derived from the sense of sight, the construction and use of optical instruments, and the methods by which optical aberrations may be obviated, with discussions about the retina, the image on the retina, the motions of the eye, the conditions of the pupil, the effect of the crystalline in bringing rays to a focus, the changes of focal length in the eye, the means by which we judge of distance, magnitude, &c., before explaining the structure of the eye, is, in our opinion, putting the cart before the horse, and necessitates a considerable amount of repetition in the seventh chapter. The tediousness of such repetition Mr. Nunneley himself acknowledges (p. 64), but does not seem to be aware that it might have been obviated by following a better method.

Vagueness of Popular Science.—Mr. Nunneley announces (p. 35) that he purposes "briefly to illustrate" in chapter third "the construction and action of the more common optical instruments." He adds: "No mathematical formula, nor any abstruse calculations will be introduced." To such popular explanations there can be no objection; only they must be comprehensive and correct. This is not the case with Mr. Nunneley's statement (p. 44), that "all images formed by concave mirrors are inverted, and generally positive." Any one who looks at his face in a shallow concave mirror, knows that he sees his image not inverted, but erect. Such a phrase on such a subject as "generally positive," is altogether inadmissible. It must be stated when the image is positive or real, and when it is virtual or imaginary, and the meaning of such terms as *positive* and *virtual* must be explained before they be used, which Mr. Nunneley has not done. With respect to concave mirrors, a few additional words would have saved a glaring error, and avoided all vagueness. Images formed by a concave mirror appear before it, are positive, diminished, and inverted, except when the object is placed nearer to the mirror than its principal focus, in which case the image is virtual, magnified, erect, and appears behind the reflecting surface.

Another example of a similar sort occurs in the following statement:—"In consequence of this bending of the rays as they pass through a convex lens, it is an universal rule that an image so formed is in an inverted position relatively to the object." (p. 58.) Any one who looks through a reading-glass or a pair of convex spectacles, knows that the letters of the book he reads do not appear to him inverted. Hence it is plain that Mr. Nunneley's rule is not universal, but requires to be corrected. It is only when the rays, bent as they pass through the convex lens, converge to actual foci, that the image is inverted. If they diverge from a virtual focus, the object and the image subtending equal angles at the centre of the lens, as is the case in the instance of the reading-glass, convex spectacles, or a simple microscope, the image is erect.

Use of Terms.—"From this property of light [refrangibility] arise," says Mr. Nunneley, "two most important laws, which are of so general application (*cæteris paribus*) as to constitute axioms." (p. 47.) Here the term *axioms* is misapplied. An axiom is a self-evident theorem, the truth of which the mind admits as soon as the words in which it is expressed are understood; but the facts or laws referred to by Mr. Nunneley require to be demonstrated by experiment, and are therefore of a totally different nature from axioms.

To some of our remarks, Mr. Nunneley may perhaps be disposed to say, "Oh, this treatise is not meant to be strictly and scientifically accurate." Why, we would ask, should it not? Would it not be better to take a little additional pains, and make it accurate so far as it went, than to leave the student in a maze of unexplained terms or an actual flood of errors? We object particularly to the using of technical terms, the meaning of which is not generally known, and which is left unexplained. For example, Mr. Nunneley states correctly, that "it was formerly supposed that the angles of incidence and refraction always bear the same proportion to each other. This, however, is not the case, but it has been ascertained that the sines of these angles always bear an exact proportion to each other." (p. 50.) He gives no definition of what a sine is—no explanation of the fact that the sines of angles have not the same proportion to one another as the angles themselves. "Principal focus," "conjugate foci," and various other terms, are introduced by Mr. Nunneley without any previous explanation.

"It is unnecessary," says Mr. Nunneley, "to enter upon the rules for finding the precise degree of refraction which each of these forms of lens produces upon the different rays of light." (p. 53.) We should say it is necessary; only if it is meant, however, that the subject should be understood. No one can be said to understand any subject in physics unless he knows the rules belonging to that subject, and knows them mathematically. He may learn, like a parrot, to repeat a number of words or sentences about the matter in hand, such as the law of the sines or the general effects of the different forms of refracting media, but he does not know it as a matter of science.

Diameters of the Eye—Place of the Punctum Cæcum.—In his fourth chapter, after a description of its appendages, the author proceeds to the globe of the eye, remarking that "the human eye is described by some anatomists as being a true sphere, but by the greater number as being a spheroid, the diameter of which, from before to behind, is somewhat greater than in any other direction." (p. 127.)

> "I am much inclined to think," he goes on to say, "whatever departure there is from a spherical figure is in the contrary direction to that which it has been so confidently and commonly stated to be, and that the opinion of the antero-posterior diameter being larger than the transverse has rather been formed from looking at a section where the projection of the smaller curve of the cornea, set in, as it were, to the somewhat flattened termination of the sclerotic, gives rise to the appearance of greater projection, than to the actual measurement of the two diameters." (p. 128.)

He then enumerates many mammals, birds, reptiles, and fishes, in which the antero-posterior diameter is shorter than the transverse, without noticing, however, the bat, in which the reverse is strikingly the case,* so that while the habits of this mammal resemble those of a nocturnal bird of prey, so does the form of its eyes. He has taken much pains in measuring a great number of eyes of men, women, children, and inferior animals. The tables of measurements which he subjoins are of importance, as showing also the form and size of the cornea, and in pointing out the place where the optic nerve enters the sclerotic, and its distance from the axis of the eye.

Mr. Nunneley shows that in man, and a great number of other animals, not only does the optic nerve enter to the inner side of the axis, as has generally been noticed by anatomists, but that its entrance is, both in the lower animals and in man, below the level of the axis. That this is the case in the lower animals has, we think, been generally observed; and all the ophthalmoscopists, in their representations of the interior of the human eye, have shown the papilla conica as not only to the nasal side of the macula lutea, but below its level. That the situation of the entrance of the optic nerve does not appear very strikingly below the level of the axis, in the human eye removed from the orbit, is shown by the fact that Mariotte had set it down as entering higher than the axis. He says:—

> "J'avois d'ailleurs souvent observé par l'anatomie tant des hommes que des animaux, que jamais le nerf optique ne répond justement au milieu du fonds de l'œil, c'est à dire, à l'endroit où se fait la peinture des objets qu'on regarde directement; et que dans l'homme il est un peu plus haut et à coté, tirant vers le nez."†

Accordingly he placed the piece of paper the image of which he meant should fall on the punctum cæcum, below the level of his eye. Dr. D. Griffin, in his experiments on the punctum cæcum, found a tendency in the eye, in moving outwards, to move also a little upwards; from the circumstance that the image, or candle, was more perfectly hidden then, than when the axis of the eye was directed to a point in the same horizontal line with it. "This," says he, "seemed to indicate that the punctum cæcum was situated a little higher than the extremity of the visual axis."‡ Now, it really indicated quite the reverse; the eye tended to move upwards, that the image might fall below the axis, on the end of the optic nerve. Instead of 1° 11′ being the probable elevation, as Dr. Griffin states it to be, of the centre of the optic nerves above the plane passing through the visual axis of both eyes, there can be no doubt that this is the depression of the same point below that plane. Accordingly Mr. Nunneley, on measuring in the human eye the distance from the centre of the optic nerve to the upper margin of the cornea, found it greater than the distance from the same point to the lower margin, in the proportion of 1·9 inch to 1·7 inch, and in some cases still more. His measurements show that a difference in this respect exists in some individuals between the two eyes; and, what is still more remarkable, that in certain rare instances some of the measurements from the optic nerve to the cornea are the reverse of the normal. Thus, in one case, the measurement from the optic nerve to the inner margin of the cornea was $1\frac{1}{13}$ths inch, while from the same point to the outer margin was only $1\frac{6}{13}$ths inch. This was the case in the right eye only, the measurements of the left being the usual ones. The person to whom these eyes belonged had been noticed when a patient in the infirmary, to have a peculiar manner of holding a book or newspaper to one side, and very near his face. The probability is, he read only with the left eye, the punctum cæcum of his right being in, or nearly in, the axis of vision. In the examination of cases of congenital imperfection of one eye, the possibility of such an occurrence should be kept in mind. In two fœtuses, the one at full time and the other at five months, Mr. Nunneley found the measurement from the optic nerve over the eye to the cornea less than that under the eye to the cornea. This was the case in both eyes of each fœtus.

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* "Axis bulbi diametro horizontali longior; cornea insigniter convexa, hemisphærium anterius fere totum occupat, ita ut scleroticam magnitudine fere æquet."—D. W. Soemmerring, De Oculorum Hominis Animaliumque Sectione Horizontale, p. 26. Goettingæ, 1818.

† Nouvelle Découverte touchant la Vue: Œuvres de Mariotte, p. 496. Leide, 1717.

‡ Contributions to the Physiology of Vision; Medical Times and Gazette, vol. xxii. p. 293. London, 1838.

Proportions of Sclerotic and Cornea.—In the two following statements, Mr. Nunneley has fallen into error. "The sclerotic forms about five-sixths of the external wall of the globe." (p. 150.) "The cornea forms about one-sixth part of exterior tunic of the eye." (p. 154.) We know it is stated, and perhaps it is stated correctly, that in a horizontal section of the eyeball, five-sixths of the circumference of the section are formed by the sclerotic, and the remaining sixth by the cornea. Now, of all such sections, the areæ of the spherical surfaces are to one another as the lengths of the included portions of the diameter. The proportion, then, of the surface or external wall of the globe which the sclerotic bears to the cornea, will be as one hundred and eighty-six to fourteen nearly; or, omitting fractions, as thirteen to one. The cornea forms, not about one-sixth, as Mr. Nunneley states it to do, but about one-thirteenth part of the exterior tunic of the eye.

Corneal Tubes.—If a puncture, as was discovered by Mr. Bowman, be made near the edge of the cornea of an ox, or other large mammal, and the mouth of a mercurial injecting tube be inserted, the metal runs along in parallel and delicate lines for a short distance, to diverge at different angles into other similar channels, crossing the former at different depths in the cornea. Whether this system of canals is concerned in endowing the cornea with its necessary transparency, or in promoting the nutrition of its non-vascular tissues, has not been determined; but in exhibiting such a structure, in the manner described, we have never found any difficulty. Mr. Nunneley, however, has failed to observe any such special arrangement of tubes, and does not believe (p. 160) in its existence.

Anterior Elastic Lamina.—This layer of the cornea, first described by Mr. Bowman, and regarded by him as the basement membrane of the conjunctival epithelium of the cornea, and the counterpart of the membrane of Descemet, Mr. Nunneley has looked for (p. 161) most carefully, without having satisfied himself of its existence. Certainly, the so-called anterior elastic lamina cannot be viewed as the counterpart of the membrane of Descemet. There is on the posterior surface of the proper substance of the cornea a condensation of tissue, similar to that on the anterior; but neither the one nor the other ought to be looked on as a distinct lamina. The smooth dense surface exposed by removing the epithelium anteriorly, or the membrane of Descemet posteriorly, can no more be regarded as distinct from the proper substance of the cornea, than can the outer and inner tables of the skull be, properly speaking, regarded as distinct from the intervening diploe.

Pigment.—In describing the pigment of the choroid and iris, Mr. Nunneley entirely confounds the cells with the granules. He acknowledges that, under the microscope, hexagonally-shaped bodies are seen forming the membrane of the pigment; while on the outer surface of the choroid, and incorporated with its substance, bodies approaching to a stellate shape are discovered; but that these are true cells he does not admit (p. 170). On teazing out the stellate bodies, he finds, of course, a great number of granules, but he insists on calling them cells. He describes (p. 173) the well-known molecular motion of the granules, bodies which are seen in myriads when the pigment is broken up under the microscope, but he insists on their being true cells filled with fluid.

The generally received doctrine is, that pigment, both in vegetables and animals, is contained in the interior of cells, and that it may exist either in a fluid or in a granular state; that the granular state, while extremely rare in vegetables, is common in animals; that the cells containing the granular pigment, have walls of structureless membrane; that they vary both in size and figure, the most common form being hexagonal, with a diameter of about $\frac{1}{1000}$th inch, while in other instances they are larger and of a different form, being elongated and branched, so as to present more or less of a fusiform, bifurcated, or even stellate appearance; that the colour of the cells depends on the quantity of granules which they contain, bodies measuring on an average $\frac{1}{20000}$th of an inch in diameter; that in newly developed cells, such as those of the membrane of the pigment in the fœtus, their nucleus may be seen as a white spot in their centre, while in older specimens it is seldom plainly seen, having either disappeared or being hid from view by the great abundance of the granules; and that one of the most distinct examples of pigment-cells is found in the hexagonal bodies of the choroid epithelium, and of granules in the contents of these bodies.

It shows a degree of perverseness on the part of Mr. Nunneley, to insist on calling the granules cells, and to deny the true character of the hexagonal and stellate bodies of the pigment. He states his belief, that the granules are identical with the pigment-cells of the rete mucosum, and the colouring matter in melanosis; but every one knows that such is not the case, and that the rete mucosum, or last-deposited layer of cuticle, owes its colour to the presence of cells, containing granules of pigment, and that melanosis also consists principally of cells filled with black granules.

Muscæ volitantes.—Our author follows up his description of the choroid pigment by stating his belief (p. 174) that its arrangement affords a satisfactory explanation of what are termed *muscæ volitantes;* to the appearance of which, as seen by the eye of the patient, he thinks a portion of teazed-out choroid bears a perfect resemblance, and therefore that the cause of this affection resides in the nodulated masses of pigment, with their connecting and stellate fibres. Attributing the stellate arrangement to the aggregation and attachment of the pigment to the vessels of the choroid, he ascribes muscæ which change their form, to congestion or contraction of the capillaries, or to the deposition or absorption of fibrin, while those that are constant he refers to some organized deposit or permanent capillary alteration.

With respect to *fixed muscæ*, without pretending to decide that they never arise from morbid changes in the choroid, we believe they generally depend on lesions or deposits in the substance of the retina; but so far as the more common kind of *muscæ*, namely, the *floating* ones, is concerned, a satisfactory answer to our author's hypothesis is afforded by the fact, so simply demonstrated by Sir David Brewster, that the corpuscles or filaments which cause the sensation are situated anterior to the retina. Sir David showed, that by looking through a pin-hole at two bright sources of light, such as two lighted candles, we obtain by their two divergent beams, double images of all objects situated within the eye and in front of the retina. By this means, the floating muscæ, as we know by frequent repetition of the experiment, are all seen double, depending as they do on corpuscles or filaments anterior to the retina, while any object in or behind the retina, any musca arising from defective sensibility of the retina, or from pressure on its convex surface, could not produce a double image, but would appear single. By means of the same experiment, the size of the corpuscles and filaments, and their distance in front of the retina may also be determined.

Nourishment of the Lens—Pupillary Membrane.—Mr. Nunneley does not formally treat of the development of the organ of vision. The notions he has accidentally emitted on the subject, and especially those on the nourishment of the lens in the fœtus, and the nature of the pupillary membrane, are in several respects incorrect, as the following extracts will show:—

> "In fœtal life there is no pupil, the aperture is closed by the *membrana pupillaris* upon which vessels pass from the iris, forming a plexus not unlike in distribution those upon the posterior and anterior capsules of the lens." (p. 195.)
>
> "The use of the membrane has not been very satisfactorily pointed out; probably it is essential to the due development of the iris in a true plane across the aqueous chambers, and

serves to keep the base or foundation upon which the muscular and elastic structure are [is] deposited in a correct form. So long as it exists, the aqueous chamber is of course divided into two. As to the mode of its development there is some difference of opinion, some regarding it as a single membrane, derived altogether from the anterior surface of the iris; but probably the idea of Cloquet is correct; viz., that it is double, the anterior layer being a continuation of a membrane which lines the cornea, and is reflected on the anterior surface of the iris, while a similar membrane lines the posterior chamber and covers the posterior surface of the iris, the two laminæ being at the pupil in apposition with each other." (p. 196.)

"In the adult, neither vessels nor nerves can be traced in the lens or its capsule; they are therefore regarded as extra-vascular; but during fœtal life, up to the period of birth, and even some little time afterwards, both contain vessels." (p. 259.)

It is plain, that Mr. Nunneley supposes the vessels of the pupillary membrane to be distinct from those covering the anterior capsule of the lens; the pupillary membrane to be destined for the development of the iris; two chambers to exist, each filled with aqueous humour, previously to the breaking up of the pupillary membrane; the pupillary membrane to be a double one, attached to the pupillary edge of the iris; and during fœtal life, up to the period of birth, and even some little time afterwards, both lens and capsule to contain vessels; all which suppositions are erroneous. Neither the lens nor its capsule is ever vascular; the pupillary membrane is attached to the front of the iris, leaving the pupillary edge free; the membrane is not double; there is no posterior chamber so long as the pupillary membrane is entire; the pupillary membrane is merely a portion of the anterior half of the vascular sac which surrounds and nourishes the crystalline capsule and lens during the first seven or eight months of fœtal life.

In the fœtus, the central artery of the retina, entering the eyeball through the optic nerve, divides into two sets of branches; one, which is persistent, to the retina, and another, which is temporary, to the vitreous body, and to the vascular sac which encloses the lens and its proper capsule. The trunk of the latter set, on entering the hyaloid canal, divides into the central artery of the vitreous body and its circumferential branches. The central artery advances straight to the middle of the hyaloid fossa, and spreads out in radii on the posterior half of the vascular sac. Having reached the edge of the crystalline body, these branches anastomose with the arteries of the zonula ciliaris, these in their turn being connected with the proper vessels of the retina, with the circumferential vessels of the hyaloid, and perhaps also with the ciliary arteries. The radiating branches now turn round the edge of the crystalline body, to be distributed to the anterior half of the vascular sac, which for a time lines the walls of the small aqueous cell, while the comparatively large crystalline is almost in contact with the cornea. The iris being undeveloped, the aqueous cell is not yet divided into an anterior and posterior chamber. When, however, the iris does sprout out, it soon comes into contact with the vascular sac, and adheres to it in such a way that the anterior half of the vascular sac fills the pupil, constituting what is known by the name of the *pupillary membrane.* The portion of the vascular sac which extends from the pupil to join the posterior half of the sac at the edge of the crystalline, was described by William Hunter,* and is called the *capsulo-pupillary membrane.* The iris having joined the vascular sac in the manner now explained, sends vessels into the pupillary membrane to anastomose with its original vessels. These irido-pupillary vessels are larger than those coming from the posterior half of the vascular sac, and anastomose together in arches, turned towards the centre of the pupillary membrane. Continuing to grow, the pupillary or small ring of the iris shoots forth beyond the point where the first formed part, or ciliary ring, joined the vascular sac; and this in such a way that the circular line of junction comes to be on the anterior surface of the iris, where the ciliary joins the pupillary ring; the edge of the pupil remaining free and unattached.

All this apparatus of vessels for the development of the crystalline is destined to shrink and be removed, as soon as that object is attained. None of it exists at birth. The arteries of the hyaloid, the central artery of the vitreous humour, and those of the two halves of the vascular sac, all disappear. The portion of the vascular sac called the capsulo-pupillary membrane disappears before the pupillary membrane; according to Cloquet, the arches formed by the irido-pupillary vessels retreat into the iris to form its little arterial circle; by the end of the eighth month, in general, the whole process of removal is complete, and the dioptric media of the eye left with scarce a shred to disturb the passage of light on to the retina of the child at birth. The temporary structures have been swept away, and the nutrition of the vitreous body and the crystalline is left to those sources which are to supply it through the rest of life. If we wish to see the vessels of the vitreous and crystalline humours, we must inject from the aorta a six or seven months' fœtus, and in dissecting the eye, we must bear in mind that sometimes the posterior half of the vascular sac becomes detached from the anterior, the latter being left adhering to the iris, filling the pupil, and answering the common description of the pupillary membrane; while in other cases the lens comes away entirely covered with vessels, the vascular sac having separated in an entire state, and no pupillary membrane, such as is commonly described, being found.

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* Medical Commentaries, part 1, second edition, p. 68, note. London, 1777.

Retina.—Mr. Nunneley occupies thirty-five pages with the retina, which he justly describes as "one of the most complex, perhaps the most complex, structure to be found in the body." (p. 197.) These thirty-five pages are the most laboured of Mr. Nunneley's book, and exhibit on the whole an interesting account of the anatomy of the retina, although it would appear that in attempting to follow up the discoveries which have been made in the structure of some of its parts, he has often been disappointed. The chief cause of this want of success is unquestionably the extreme difficulty of the investigation, owing to the minuteness of the organizations and the rapidity with which they become changed after death. Another cause we suspect to be that our author's microscopical examinations of the retina have been made chiefly with artificial light. "The work," he says in his preface, "has been written amidst the constant interruption and distraction of private practice, which has occupied the whole of the day, leaving only the night for scientific investigation." (p. v.) Now, we believe it to be much more difficult to make out the elements of such extremely minute textures as are combined in the retina, and their relations to one another, with artificial than with good daylight. Leisure, too, and the uninterrupted attention which it permits, as well as some share of that mechanical address in the preparing and disposing of objects for the microscope, which practice alone can give, are necessary for the successful examination of such structures as the retina. "Whoever," says Schleiden, "would observe successfully, must observe much, and with strenuous attention, by which he will soon learn that seeing is a difficult art."

The remarks by Mr. Nunneley in his note, p. 199, on the effects of different reagents on the different textures of the retina, are well worthy of the attention of those commencing the microscopical study of this part of the eye. "I believe," says he, "the only way to examine the retina unchanged is to do so *immediately* after death, and without the addition of *any substance whatever*."

We shall add a few other hints, which perhaps may prove useful to beginners.

1. Having tied down the eye to a disk of lead about 1½ inch in diameter and 1 inch thick, by means of a thread passed through the cornea and through a diametrical canal in the lead, a considerable portion of the sclerotic and choroid is to be dissected away, without removing if possible the pigment membrane, and a thin piece of vitreous humour is to be cut out with the corresponding piece of retina and pigment membrane. The piece is to be placed, pigment uppermost, on a glass slide, and examined. At some point the pigment membrane will probably give way, and become slightly detached, so that the ends of the rods will be seen like a very minute tessellated pavement. All that can be seen in this way is to be examined without covering the object with a thin glass.

2. The same object, or another prepared in the same way, is to be covered with

a thin glass, and examined. As the examination is continued, more of the pigment membrane will probably become detached, so that a more complete view of the stratum bacillosum will be obtained. Some of the rods will be seen overthrown by the pressure of the glass, some perhaps detached altogether. A little pressure over the thin glass with the point of a needle will probably promote these changes.

3. To see the transition of the stratum bacillosum into Jacob's membrane, a drop of water is to be allowed to fall upon the object before covering it with the thin glass, and the observer must continue to watch some time for the change, adding a little more water when necessary, by putting a drop at the edge of the glass, and letting it run under it. Sometimes the transition is well seen without covering the object at all.

4. By pressure and watching, the different layers will come to be seen. The stratum bacillosum in a short time readily becomes detached, and we see the stratum underneath. Then by tearing the piece of retina, and regulating the focus of the microscope, the structures may be discerned in their stratification.

5. By folding a piece of retina and examining the edge of the fold, we get sight of something like a vertical section.

6. After having thus studied a piece of the retina from without inwards, we must next take a piece and examine it from within outwards.

7. Watching, pressure in various degrees, and a little tearing, are the simplest means by which one can get a notion of the structure of the retina.

8. In dissecting the retina, the simple microscope is of very great use in preparing the object to be examined under the compound microscope.

Our author describes the retina as consisting of the following layers, enumerating them from the convex to the concave surface—viz., 1, columnar or bacillar layer, rods, or Jacob's membrane; 2, bulbous or conoidal bodies; 3, granular layers; 4, nucleated vesicular layer; 5, vascular layer; 6, fibrous layer; 7, hyaloid cellular layer. Those who are in the slightest degree acquainted with the anatomy of the retina will see that this enumeration of its structures is very likely to mislead. So also is Mr. Nunneley's vertical section (plate 7, fig. 1), where he represents a vascular layer as placed exteriorly to the fibrous or nervous layer, whereas the vessels do not properly constitute a layer at all, but occupy several of the layers while they chiefly lie close to the concave surface of the retina.

Mr. Nunneley acknowledges, as he proceeds, that the cones or bulbs are placed amongst the rods, so that they are hardly to be considered as a distinct layer, He notices the discrepancies in the descriptions of the cones or bulbs by different microscopists, such as Hannover, Bowman, and Kölliker; and adds that he has searched most carefully for these bodies in the eyes of many animals, but cannot say he has satisfied himself of the existence of any bodies, such as have been described, in the perfectly fresh eyes of any creature except fish.

> "There is no difficulty whatever," says he, "when regarding the undisturbed external surface of the retina of either reptiles or mammalia, of recognising the forms figured by Hannover and Bowman, which they consider the cones or bulbs not in focus; but I have always failed in detecting the appearances represented by them in profile, and I am more inclined to think the bodies seen at a deeper level, and out of focus, when the outer ends of the rods are in focus, not as cones or bulbs, but as the outer portions of the granular layer to be presently described, and upon which the ends of the rods rest." (p. 208.)

Under the title "Granular layers," Mr. Nunneley comprises the external granular layer, the intermediate striated layer, and the internal granular layer. The intermediate striated layer is by some microscopists, such as Vintschgau, not noticed at all; but generally, as by Kölliker and Blessig, it is represented as formed by the radial fibres of H. Müller, running perpendicularly from the rods to the membrana limitans, and intimately connected with the two granular layers. The difficulty of making out the actual arrangement is evidenced by Mr. Nunneley's statement.

> "If a section," says he, "of the dried retina of man, the sheep, or the ox, be examined with water or dilute spirit, there is no difficulty in perceiving this irregular line of very minute granular matter and indistinct fibres, with flattened globules, arranged horizontally in the length of the retina, as described by Bowman; and not vertically in the direction of the thickness, as figured by Kölliker; but it is by no means so easy to detect this separation into two layers in the perfectly fresh retina. I have sometimes fancied it was to be seen in the bullock and sheep, but I have so often not been able to find it, that, knowing how greatly every portion of the retina is changed by all fluids, and how little dependence is to be placed in appearances there found, I feel doubtful if there really be two layers." (p. 218.)

Similar difficulties await Mr. Nunneley when he proceeds to examine the nucleated vesicular layer.

> "It is very difficult," he says, "to see these brain-cells *in situ* in the fresh eye, but floating about they are readily seen; and in a successful examination they may be seen forming a layer in which the fibres of the optic nerve are imbedded and expanded." (p. 214.)

As for the caudate ganglionic cells, detected by Bowman and others, in this layer—

> "I have searched most carefully," says our author, "over and over again, for these long caudate cells in the eyes of man, many mammalia, various birds, reptiles, and fish, and particularly in the almost living eye of the turtle, and must confess, like Hannover, having failed to find them in the perfectly recent eyes. When reagents are employed, when the retina has been dried and moistened with water, or the retina examined is not from an animal just dead, not the same difficulty exists; large, irregular, more or less caudiform cells are then abundant enough. I am therefore, unwilling as I am not to see what such competent observers speak unhesitatingly of, constrained to doubt if cells such as figured and described, with many long caudate processes, continuous with the nerve-fibres, do really exist in the living eye." (p. 214.)

Mr. Nunneley's fibrous layer consists of the expansion of the optic nerve.

> "The nerve-fibres," says he, "expand in every direction, forming a complete layer upon the outer surface of the hyaloid membrane." (p. 216.)
>
> "The fibres," he adds, "lie to the inner side of the granular layer, which separates them from the rods, and they are, as it were, imbedded in the vesicular layer, which, without care, always renders the fibres indistinct." (Ib.)

The hyaloid cells which are found forming a layer between the retina and the vitreous humour, Mr. Nunneley inclines to consider as more intimately connected with the retina than with the hyaloid membrane.

Although the author enumerates a vascular layer, he tells us that

> "At first the larger branches [of the central artery] are on the inner side of the fibrous layer, but as they pass forwards they gradually penetrate this, some of the branches running parallel with the nerve fibres for a considerable distance; but the smaller branches and capillaries, which form beautiful loops with each other, appear to be exclusively distributed in the vesicular and granular layers, on the outer surface of the fibrous; none, so far as can be detected, passing into the bacillar layer." (p. 219.)

The very thin membrana limitans, described as structureless, hyaline, and elastic, which bounds the retina on its concave surface, is not acknowledged by our author.

As to the foramen centrale of Soemmerring (which it might be better to call a macula, as it is certainly not a hole), the author propounds a theory which we consider as altogether unfounded:

> "On the whole," says he, "I incline to regard this much-debated spot as a vestigiform remains of the spot where a large bloodvessel has passed through the retina in the development of the eye, and carried with it some of the choroidal colouring matter." (p. 227.)

No such bloodvessel has been seen in the eye of the fœtus. Perhaps, bethinking himself of this objection, Mr. Nunneley hazards another conjecture as to the central spot, but no sooner does he venture to express it than he adds its refutation. "It may be," says he, "an undeveloped analogue of the marsupium in birds, but if so, why it should not be generally distributed it would be difficult to say." (p. 227.)

It is generally acknowledged that the central spot and its yellow border exist in the eyes of the quadrumana; they were found in the eyes of this class of animals by Home, Blumenbach, Cuvier, and D. W. Soemmerring; and Mr. Bowman describes* their appearance in one specimen which he minutely examined; but Mr. Nunneley is unsuccessful in his researches on this point, as on many others: "I have dissected the eyes of two species of baboons and three monkeys without finding it." (p. 220.)

Mr. Nunneley makes so slight reference to the fibres, first distinctly described by H. Müller, which pass in the direction of the radius of the eye, through the layers of the retina, between the rods and the membrana limitans, and which constitute what is termed the radial system of fibres, in contradistinction to the horizontal system formed by the fibres of the optic nerve, that we presume they have not come under his observation. Of the nature of the radial fibres, the most opposite views have been adopted; for while Kölliker speaks of the rods, cones, and radial fibres as true nervous elements directly engaged in the functions of visual sensation, Remak regards the radial fibres as a structure merely of the connective kind, for binding the different elements of the retina together. H. Müller did not at first incline to attribute a nervous function to the radial fibres, although afterwards he adopted Kölliker's views on this point. Hannover excludes the layer of cones and rods from the nervous tissues of the retina. Brücke regards it as a catoptric reflecting apparatus. Almost as much diversity, in fact, exists in the views entertained by microscopical physiologists, as to the functions of the tissues forming the retina, as is to be found in the various anatomical descriptions of these tissues.

Eye of the Cuttle-fish.—Passing on to Chapter V., which treats of the Comparative Anatomy of the Eye, we find Mr. Nunneley, at page 278, introducing a woodcut of the eye of the cuttle-fish (*Sepia officinalis*), taken from John Hunter's figure† (as usual, without acknowledgment), and in explaining it, mooting the old difficulty first started by Cuvier, how the animal can see, considering that between the retina and the vitreous humour a "dark choroid coat is interposed, which being placed anterior to the nervous expansion, thus cuts off all the rays of light." To remove this difficulty, Mr. Nunneley quotes the statement, which is perfectly correct so far as it goes, of Prof. Owen:

> "That there is a nervous layer anterior to the choroid, in the same situation the retina occupies in quadrupeds, and that although it may be difficult to discover the connexion between the anterior and posterior layers, yet he has no doubt the anterior is composed of the fibres of the optic nerve, and that it constitutes a true pre-pigmental retina." (p. 279.)

The connexion which Professor Owen had not discovered, had, previously to the Professor's article 'Cephalopoda,' in the 'Cyclopædia of Anatomy and Physiology,' been made known by Mr. Wharton Jones, and illustrated by an engraving in the 'Philosophical Magazine' for January, 1836. The reader will there find the structure of the retina, both of the sepia and the octopus, fully explained. The optic fibrils, which penetrate singly the thin cartilaginous lamina, which corresponds to a sclerotic, expand into a layer of a light reddish-brown tinge, constituting the first layer of the retina. The second layer of the retina is also of a reddish-brown colour. Betwixt the two layers there intervenes a pretty thick and dark layer of pigment, through apertures in which the nervous substance penetrates from the first layer to the second. Examined with the microscope, the second layer is observed to be composed of short fibres perpendicular to its surfaces; and these fibres, towards the inner surface, end in a delicate, pulpy, nervous substance, also tinged of a reddish-brown colour, and presenting a corrugated or papillary appearance.

This view of the matter has been followed up by H. Müller, who describes the innermost layer of the retina in the cephalopoda, as consisting of elongated, slender, transparent cylinders, similar in many respects to the rods of the vertebrata, and like them densely crowded together, and disposed in the direction of the radius of the eye. The layer of pigment he describes as penetrated by fusiform filamentary prolongations of each cylinder, by which means a connexion is effected with the outer layers of the retina, the outermost of which is the horizontal expansion of the fibres of the optic nerve. He regards the arrangement of the elements, then, as one pretty nearly the opposite of that which exists in the vertebrata.

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* Lectures on the Parts concerned in the Operations on the Eye, p. 91. London, 1849.

† Descriptive and Illustrated Catalogue of the Physiological Series, &c., vol. iii. plate 42, fig. 2. London, 1836.

The conclusion which H. Müller draws from these facts is, that it is hardly conceivable that the light should act directly upon the fibres of the optic nerve, which lie behind the pigment, but that the perception must, in the first instance, proceed from the radially-disposed elements which alone are opposed to the light.

Choroid Gland.—Figure 127, page 282, is said to show the posterior part of the sclerotic, cut open and reflected, to expose the choroid gland surrounding the optic nerve. It will be impossible for any one to form the slightest conception of the form of the choroid gland from this woodcut. Mr. Nunneley states that the so-called gland is situated between the layers of the choroid, is peculiar to osseous fishes, of a red colour, of a horse-shoe shape, and made up of convoluted bloodvessels. He describes the choroid of fishes as double—the outer layer of a shining metallic lustre, and the inner dense and black.

Usually, the choroid in fishes is described as dividing itself into four layers; the outer, or lamina argentea, such as Mr. Nunneley describes it; a second, the tunica vasculosa Halleri, in which is the so-called gland; a third, the proper choroid, or tunica Ruyschiana; and a fourth, a pigment layer. The silvery membrane which lines the sclerotic, does not appear to us to belong to the choroid. When we remove it, along with the sclerotic, to which it adheres, we expose a thin fascia closely adherent to the outer surface of the choroid gland and vascular choroid. Removing the vascular choroid and gland, we expose the dense pigment-layer, differing from the membrane of the pigment of mammals only in being more consistent, in consequence of the prolongation of the cells; and under it we meet with a soft grey layer, which exhibits under the microscope the small rods and large bigeminal cones of Jacob's membrane. In our opinion, the tunica vasculosa is the proper choroid, and must include the tunica Ruyschiana or capillary network.

That the choroid gland is made up of convoluted bloodvessels, as is stated by our author, is true; but the addition of a few lines would have put the reader in possession of the fact, presumed by Albers, but first, through injection and the microscope, demonstrated by Mr. Wharton Jones, that the structure in question is a rete mirabile, or rather the centre of a series of retia mirabilia. The artery which supplies the whole of the gland and vascular choroid, having entered the back of the eye, divides into two branches, which run along the inner or concave edge of the gland, giving off twigs, which by subdivision and anastomosis form a network. The outer or convex edge of the gland is also surrounded by a network of vessels large enough to be visible to the naked eye. On examining the gland itself with the microscope, it is seen to consist in an innumerable quantity of minute straight vessels, which passing across, establish a connexion between the inner and outer networks. The large vessels of the inner network divide into the minute straight vessels which form the body of the gland, and these minute vessels coalesce in their turn to form the large vessels of the outer network; thus exhibiting the well-known character of a rete mirabile. Many of the vessels, however, derived from the outer network, as they proceed forward in the tunica vasculosa, subdivide into small branches, which again unite, thus forming secondary and more simple retia mirabilia. Lastly, the arteries of the tunica vasculosa terminate in a star-like manner, and the radiating twigs of these stars inosculate together.*

The sixth Chapter, on the Eyes of Extinct Animals, is short and amusing. The seventh, on the Physiology of Vision, occupies seventy-three pages, and discusses the questions of optical aberrations, single vision with two eyes, erect vision with an inverted image, complementary colours, ocular spectra, and the like. Seeing the length to which this article has grown, we shall confine our remaining remarks to one or two topics only.

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* Wharton Jones on the Choroid Gland; Medical Gazette, vol. xxi. pp. 650, 864. London, 1838.

Optical Aberrations—Use of the word Parallax—Monocular and Binocular Parallax.—It is well-known that there are three optical aberrations or sources of indistinctness, which must be obviated in the eye, to render vision such as it is—viz., that from sphericity of the dioptric media, that from unequal refrangibility of the differently coloured rays of light, and that from distance of the object viewed. The first is called spherical, and the second chromatic aberration, and by a newly-coined word the third is known as distantial aberration. Though *distantial* is scarcely an English word, yet it is founded on sufficient analogy, and indicates clearly enough the meaning to be expressed. Mr. Nunneley, however, not contented with it, calls the indistinctness arising from distance, the *aberration of parallax*—a phrase which by many will not be readily apprehended.

By parallax Mr. Nunneley no doubt means the sensible *deviation* from parallelism of rays emanating from a given point, and falling on a surface of given magnitude, such as that of the lens as exposed through the pupil. If a convex lens is placed at the distance of its principal focus from the recipient surface, it will give distinct images of objects at such a distance that the rays falling from them on the lens are sensibly parallel; but if the object is brought nearer, so that the rays from a point in the object, falling on the lens, become sensibly divergent, there is a deviation from parallelism, or in other words a *parallax*, which will prevent the convergence of the rays to one point, and consequently exhibit on the recipient surface what is termed a *circle of dissipation* or *aberration;* but this can be made to disappear by moving the lens farther from the recipient surface. The application of this to the eye is very obvious; but we see no advantage in substituting the phrase aberration of parallax for distantial aberration. It will require additional explanation before it be generally understood; this, however, Mr. Nunneley has not thought fit to offer. This *monocular parallax* will require, moreover, to be distinguished from the *ocular parallax* of Sir David Brewster,* which he regards as the measure of the alleged deviation of the *visible* from the *true* direction of objects, and is therefore a totally different matter. Such are the disadvantages of not adhering to the phraseology in common use; by new applications of scientific terms we may make ourselves unintelligible.

From Mr. Wheatstone's paper on the stereoscope, Mr. Nunneley quotes the statement, that when an object is viewed at so great a distance that the optic axes of both eyes are parallel, the perspective projections of it, seen by each eye separately, are similar, and the appearance to the two eyes precisely the same as when the object is seen by one eye only; but that this similarity no longer exists when the object is placed so near the eyes that to view it the optic axes must converge, that under these conditions a different perspective projection of it is seen by each eye, and that these perspectives are more dissimilar as the convergence of the optic axes becomes greater. In this case the angle formed by lines drawn from the centre of each eye to the object constitutes what has been called by the successors of Wheatstone the *binocular parallax* of the object.

> "This angle," says Mr. Nunneley, "supposing the object to be twelve feet distant, and the space between the eyes to be two and a half inches, would be exactly one degree, and at twenty-four feet half a degree, so that it is evident it is only objects near to the eye in which this dissimilarity of images exists, for in the more distant the binocular parallax is too small to produce any effect; in those persons in whom the eyes are widely separated the binocular parallax will be greater, and consequently the appearance of relief proportionally greater, and in those in whom the eyes are near together it will in a corresponding degree be less." (p. 363.)

In illustration of this subject, Mr. Nunneley introduces (Fig. 169) two perspective projections of a cube—the one that seen by the right, and the other that seen by the left eye; but if he will actually look at a cube in the way directed, he will see that the appearances it will present, first to the one eye and then to the other, are the reverse of what he has delineated.

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* Transactions of the Royal Society of Edinburgh, vol. xv. p. 350.

Accommodation to Distance.—Mr. Nunneley discusses at considerable length the subject of accommodation to distance, enumerating various observations which prove the fact; and considering the more prominent hypotheses which have been formed to explain the mode in which it is effected. He omits, however, the proof of the actual occurrence of a change in the eye, according as it is adjusted to different distances, which is afforded by means of the ophthalmoscope, the distinctness of the image of the luminous object on the retina of the observed eye being seen to vary as the eye is adjusted for near or distant vision.

It is strange, too, that our author takes no notice of the highly interesting application of the catoptrical test, as it is termed, to the determination of the method in which the adjustment to distance is actually effected. This subject has been handled in his usual lucid style by Dr. Allen Thomson, in a paper in the 'Glasgow Medical Journal' for April, 1857, from which, had he had no access to the original memoirs on the subject by the Dutch and German physiologists who first made the application, our author might have derived an accurate notion of the interesting discovery. Even so short an abstract as the following, which we take from Dr. Thomson's paper, would have supplied a striking want in Mr. Nunneley's account of the focal adjustment of the eye.

We select a person under middle age, possessing the perfect power of adjustment, and having placed him in a darkened room in such a position that his head may be kept steady, a small artificial light is held to the side and in front of one of his eyes, at a distance of six inches, and we then examine, at an equal angle from the other side, the images formed by the several reflecting surfaces. These images may be seen with the naked eye, or with a convex lens held in the hand; but Dr. Thomson employs for this purpose a compound microscope, with the erector applied, and a power of twenty diameters. This arrangement allows the eye to be viewed in a steady fixed direction, and the observer to be placed at a convenient distance from the subject of experiment.

The eye not under observation is now to be covered, so as to prevent much shifting of the opposite one, which the person under observation is then alternately to direct to a near and to a distant object, conveniently placed in a direct line before him.

If the pupil be of sufficient size (and if not so it may be moderately dilated by atropine, without destroying the accommodating power), the three reflected images are seen—viz., one erect, distinct, and large, to the side next the light, proceeding from the cornea; a second, also erect, of a much duller appearance, in which sometimes we scarcely recognise the form of the flame which gives the light, situated near to the middle of the pupil; and a third, much smaller but brighter than the last mentioned, inverted, a virtual image situated between the two former ones, and appearing towards the opposite margin of the pupil from the corneal image. Of these last two images, the former is a reflection from the anterior, the latter from the posterior, surface of the crystalline.

When, by the arrangement of the light, &c., the eye has been brought into such a position that the deep erect image, or that from the anterior surface of the crystalline, is nearer the margin than the middle of the pupil, and pretty close to the inverted image, the eye being at the time adjusted for distant vision, it will be found, that whenever the adjustment is made for vision of a near object, the pupil contracts and advances, and a marked change occurs in the position and appearance of the deep erect image; while the image reflected from the cornea undergoes no change, and the inverted image is only slightly altered. The deep erect image has become suddenly smaller and more distinct, and has shifted its place nearer to the corneal image, or towards the centre of the pupil. The inverted image, or that formed by the posterior surface of the crystalline, has not perceptibly changed in appearance, but has receded very slightly to a greater distance from the corneal image.

The first notice of these interesting facts we owe to Max Langenbeck, of Göttingen, and to Cramer, of Gröningen. The further investigation of them has been carried out by Donders, of Utrecht, and Helmholtz, of Berlin.

The position, size, and distances of the images have been accurately determined by instruments employed by Helmholtz, particularly his ophthalmometer. The following is the result of his calculations, from the observation of the change in the size of the deep erect image in two persons. The measurements are in millimetres, each of which is a little less than $\frac{1}{25}$th of an inch.

In one person the radius of curvature of the anterior surface of the lens for distant vision was 11·9 millimetres, for near vision 8·6; and the advance of the pupil during the change, ·36. In the second instance, the same respective measurements were 8·8, 5·9, and ·44. The diminution in the radius of curvature of the posterior surface of the lens amounted to ·5; but the position of the surface was probably not altered.

As to the mechanism by which the lens is made to change its form, and thus to shorten or lengthen its focal distance, according as near or distant objects are regarded; this is a question on which we shall not enter, leaving our readers to consult the paper whence we have taken the above account of the application of the catoptrical test to the subject of accommodation. Of course the relations and action of the ciliary muscle, and of the other structures at the basis of the iris, and surrounding the lens, are directly involved in this question, and still lie open, we think, to new research and discovery.

Mr. Nunneley's style is very unequal, some parts of his work being written with considerable care, while others exhibit marks of haste, and an indifference to elegance, if not even to perspicuity.

Mr. Nunneley's work is illustrated by eight plates, three of which are lithographic, and the other five engraved. Of the lithographic plates, one is coloured. The figures in these plates are well selected, well drawn, and the execution good. One hundred and seventy-nine woodcuts and diagrams are interspersed in the text, most of them taken from works on the same subject, and familiar, like the faces of old acquaintances, to those who have studied the eye and optics. The figures are not at all improved in being copied. No acknowledgment is made of the sources whence they have been taken. Mr. Nunneley makes a sort of apology for this in his preface, as much as to say that the illustrations, and even the ideas, of former authors, have become the common property of the world, and therefore do not require to be referred to their original sources. We cannot subscribe to this sort of doctrine; and, in particular, we like to see such acknowledgments of the pictorial labours of our predecessors, as *From Soemmerring*, *From Müller*, *After Hannover*, *Altered from Bowman*, and the like. The common property of the world, certainly, these labours are;—to be studied and admired for ages to come. Many of them, such as these of Zinn and the two Soemmerrings, must have cost their authors much time, pains, and money; and when we copy them into our manuals and abridgments, the least we can do is to copy them accurately and neatly, and to acknowledge whence we have borrowed them.

The work before us abounds with orthographical errors; as, catoptic for catoptric, inferiorius for inferius, eribosa for cribrosa, poup for poulp, perinibrachiate for perennibranchiate, and many others. Proper names fare no better; for we have Berkely for Berkeley, Reed for Reid, De Charles for Des Chales, Poterfield for Porterfield, Decemet for Descemet, Horne for Home, Chopet for Chossat, Huck for Hueck, Morgani for Morgagni, &c.

The paper and printing are unexceptionable, but we do think the printers are partly to blame for the numerous trangressions which occur against the rules of punctuation.

REVIEW III.

Horæ Subsecivæ. Locke and Sydenham, with other occasional Papers. By JOHN BROWN, M.D., Fellow and Librarian of the Royal College of Physicians, Edinburgh. —*Edinburgh*, 1858. pp. 478.

THE sacred precincts of the Royal College of Physicians of Edinburgh would appear to shut out the wrangling and contention which make themselves so painfully heard beyond its walls, and jar loudly upon our ears, though in body, and we trust in spirit, far removed from it. Pass the threshold of that edifice, and the strife ceases; there you may learn, if the outer world has not taught it, how the science of medicine is not dissociated from the Graces; how the hours gleaned from the labour and trouble of active life may be rendered a means of throwing a halo and a charm over our every-day existence; how harmony and spiritual beauty may be traced in the very weariness of our work, in the restless and unceasing toil which is our lot. It is well for the man who can find such *horæ subsecivæ* as have fallen to the share of Dr. John Brown; and the next best thing to filling out a spare hour in the most profitable manner yourself, is to accept the guidance of another who can show you how to do it. Indeed, whether or not you possess the amulet by which you may ward off dull-heartedness or heaviness of spirits, you will equally well bestow the time requisite for the perusal of the *Horæ Subsecivæ*, for you will find in it how to appreciate the great ones of the past, you will see how much of poetry can be extracted from even the records of a hospital, you will be shown how there is good in all things, if man but knows the process by which it may be extracted. We have no knowledge of the author, but if we may paraphrase *l'empire c'est moi*, into the "book is the writer," and argue the character of the man from the style of his book, we should pass encomiums upon the qualities of his head and heart, which are altogether foreign to the duties of a critic. We merely allude to this mode of interpretation in order to convey the impressions we have received from the *Horæ Subsecivæ*. Dr. John Brown presents us with a genial book—a book full of thought, and of that kindly appreciation of the virtues and intellectual characteristics of others, which elevates and warms the heart; while treating earnestly of serious and profound things, he thinks with Horace,

"Dulce est desipere in loco,"

and appears to act upon the principle laid down by Autolycus:

"A merry heart goes all the day,
Your sad tires in a mile-a!"

"Let me tell my young doctor friends," he says, in a preface which is as good as any of the set pieces, the overture which conveys the leading ideas to be elaborated in the opera,—

> "Let me tell my young doctor friends, that a cheerful face, and step, and neckcloth, and buttonhole, and an occasional hearty and kindly joke, a power of executing and setting a-going a good laugh, are stock in our trade not to be despised. The merry heart does good, like medicine. Your pompous men and your selfish men don't laugh much or care for laughter: it discomposes the fixed grandeur of the one, and has little room in the heart of the other, who is literally self-contained."

In illustration of these views he adds an anecdote, the essential truth of which we can vouch for, because exactly the same thing occurred in our own connection. It is to this effect:

> "A comely young wife, the 'cynosure' of her circle, was in bed, apparently dying from swelling and inflammation of the throat, an inaccessible abscess stopping the way; she could

> swallow nothing; everything had been tried. Her friends were standing round her bed in misery and helplessness. '*Try her wi' a compliment*,' said her husband, in a not uncomic despair. She had genuine humour as well as he; and as physiologists know, there is a sort of mental tickling which is beyond and above control, being under the reflex system, and instinctive as well as sighing, she laughed with her whole body and soul, and burst the abscess and was well."

Those who with us enter into the humour of this *mot de métier*, will thank us for another quotation of a similar kind; alluding to Sir Adam Ferguson, "whose very face was a breach of solemnity," the author states that he saw him not long before his death at a very advanced age, when he knew himself to be dying.

> "There was no levity in his manner or thoughtlessness about his state; he was kind and shrewd as ever; but how he flashed out with utter merriment when he got hold of a joke, or rather when it got hold of him, and shook him, not an inch of his body was free of its power—it possessed him, not he it. The first attack was on showing me a calotype of himself by the late Adamson, in the corner of which he had written with a hand trembling with age and fun, 'Adam's-sun, *fecit*.' It came back upon him, and tore him without mercy."

Sickness is often used synonymously with sadness, and the atmosphere of the sick chamber is necessarily not the same as the invigorating balmy breeze that passes over the mountain heather; but as we have learnt not to close hermetically the rooms of our patients, and know that all disinfectants and deodorizers yield in power to the eremacaustic effects of constantly renewed common air (formerly common to all but those who required it most), so too we may hope that the time is past when the medical man is characterized by "overwhelming brows," and his presence regarded as a signal for funereal solemnity.

Among the things which Dr. Brown discusses, one of the very few upon which we are disposed to differ from him is the view he promulgates on man-midwifery. He puts it prominently forward, and his arguments have a strong *primâ facie* semblance of justice; but we think that however good in theory, in practice the system advocated by the author would fail. He argues that because in the great majority of births no adventitious aid is necessary, it would suffice for women to assist in all ordinary cases of parturition, the interference of medical men being reserved for those extraordinary cases in which the experience or dexterity of the midwife failed. There can be no doubt that medical men experience a great loss of "time, health, sleep, and temper," in attending upon obstetric cases; but in what way would they acquire that tact and discrimination, that ready aptitude to deal with emergencies which distinguishes the bulk of general practitioners in this country, if they were only called in to assist the midwife once in every two or three hundred cases? Parturition may be a very simple process in itself, but the obstetrician who is not perfectly familiar with its ordinary course will not be likely to appreciate correctly any irregularity that may occur; he may over-estimate or undervalue its importance, and upon the conclusion arrived at may depend the life of the patient. Doubtless in all ordinary cases a good midwife would suffice; but would midwives, however good, inspire the woman in her hour of travail and pain with the confidence and hope that the presence of a medical man carries with it? Moreover, it is to be remembered that the medical man necessarily passes from the house of the poor to the house of the wealthy, and receives remuneration from the one which must indemnify him for gratuitous, or nearly gratuitous attendance upon the other, so that poor and rich commonly enjoy equally careful and conscientious attendance; but this would not be case with midwives; the lady would not consent to employ the midwife known to have come from the pauper's hut, and thus the latter would be debarred from earning such an income as would indemnify her for the anxieties and fatigues of her calling.

We have no statistics at our disposal to determine the point absolutely. Neither the Registrar-General nor the leading obstetrical works afford the necessary material, and bearing as it does upon the welfare of humanity and upon the "emancipation" of the female sex, no less than upon the obstetrician, the question well deserves to be ventilated by the new Obstetrical Society which has lately been formed in London, and to whose members we specially commend its consideration. The readest method of arriving at a conclusion would probably consist in comparing the mortality during labour occurring in those parts of the Continent where midwives are mainly employed, with the mortality of parturient women in the towns of England where midwives are comparatively unknown. Much as we should wish to lend our aid to increase the means by which females may earn a livelihood, we fear that in the present instance it could only be done at an increase of the hazard to human life. If this view can be proved to be erroneous, we willingly concede all that Dr. Brown asks for. We cannot, however, avoid observing that our inquiries have led us to believe that the mortality occurring in women during parturition is larger on the Continent than it is in England. We were struck with the large number of deaths happening in this way that came under our notice when connected with an insurance company which transacted an extensive business on the Continent of Europe. Whether our inference was correct or not, we are unable to decide authoritatively, but we certainly were disposed to attribute it to the much more frequent employment of midwives abroad than with us. In short, we now have a good supply of first-rate practitioners throughout the land, who are willing to aid their countrywomen in the hour of peril, and we have to create the class of midwives to whom Dr. Brown would entrust them. The question is one of extreme importance, and sundry voices have been raised of late—not, certainly, on the same plea as Dr. Brown—in favour of female attendants upon the lying-in woman, and it is not unlikely that public opinion may be influenced by such reiterations. We have therefore thought it necessary to dwell somewhat upon the matter.

The first and most important essay in the book is a parallel between Locke and Sydenham, and the two sciences of which these distinguished men were representatives. Both lived during the most momentous period of British history, and impressed the seal of their greatness upon the times, while the influence they exerted is felt even to the present generation. What has been said of Sydenham and medical science, applies equally to Locke and to the state of science at large: "Quum tempore quo Sydenhamius medicinam colendi consilium ceperat vana in scholis hypothesis magis quam sobrium naturæ studium regnaret et magni alicujus viri auctoritati plus ponderis tribueretur quam maxime perspicuo Naturæ jussui; acutum Sydenhamii ingenium recte perspexit, hac via parum vel nihil adeo ad ægrotorum salutem contribui;" and again: "spretis omnibus quæ tunc temporis vigebant speculationibus theoreticis, hoc unum curabat, ut sine præconceptis opinionibus Naturam attento animo observaret, et quid ea tum de morborum natura, tum de illorum curandorum ratione doceret, diligenter notaret, suamque agendi methodum, ad eam quæ constanter observaverat, componeret."*

It is but consistent with what we know of the constitution of the human mind to expect that men who, like Sydenham, refused "jurare in verba magistri," should have espoused the cause of freedom when the conflict with arbitrary power arose. Accordingly, we find that in politics both acted consistently with the habitual independence of their minds, and espoused the parliamentary side. Locke certainly appears to have acted with more caution and reserve than Sydenham, yet it availed him nothing. Macaulay tells us that when he refrained from expressing opinions on politics, spies were set about him, and though he never dissembled, treachery could do nothing; and Government, unable to find a just ground to punish him, resolved to vent its spite upon the friend of Shaftesbury, and ordered him to be summarily driven from his college. Accordingly the Dean and Canons of Christ Church ejected him, and thus deprived him of his home and his bread. But Sydenham and Locke have a further claim to be coupled together, in the fact that they were personal friends; and as our author has it, "they were of essential use to each other in their respective departments." On this mutual influence of medicine and metaphysics we entirely adopt Dr. Brown's views; the former, alone, sinks into rude empiricism, the latter becomes vague transcendentalism, without such reciprocal aid.

"The observational and only genuine study of mind," observes our author, "not the mere reading of metaphysical books and knowing the endless theories of mind, but the true study of its phenomena, has always seemed to us (speaking *quâ medici*) one of the most important, as it certainly is the most studiously neglected, of the accessory disciplines of the student of medicine.

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* Vita Thomæ Sydenham, conscripta a Carolo G. Kühn, p. xi. Opera omnia, ed. G. A. Greenhill, M.D.

"Hartley, Mackintosh, and Brown were physicians, and we know that medicine was a favourite subject with Socrates, Aristotle, Bacon, Descartes, Berkeley, and Sir William Hamilton. We wish our young doctors kept more of the company of these and such like men, and knew a little more of the laws of thought, the nature and rules of evidence, the general procedure of their own minds in the search after, the proof and the application of, what is true, than we fear they generally do. They might do so without knowing less of their auscultation, histology, and other good things, and with knowing them to better purpose."

Undoubtedly their stethoscope and their microscope would stand them in better stead than they now do, and instead of such studies impairing the value of these aids to knowledge, they would enhance and extend their power by enabling the student to concentrate and direct his forces. Trusting as we do in the wisdom of the general Council of Medical Education, we look forward with hope to the time when the author, and, with him, those who are anxious for the advancement of the profession, will cease to

"wonder how many of the century of graduates sent forth every year, armed with microscope, stethoscope, uroscope, pleximeter, &c., and omniscient of *râles* and *rhonchi*, sibilous and sonorous; crepitations moist and dry; *bruits de râle, de scie, et de soufflet;* blood-plasmata, cytoblast, and nucleated cells, and great in the infinitely little,—how many of these eager and accomplished youths could 'unsphere the spirit of Plato,' or are able to read with moderate relish and understanding one of the Tusculan Disputations, or have so much as even heard of Butler's 'Three Sermons on Human Nature,' Berkeley's 'Minute Philosopher,' or of a posthumous 'Essay on the Conduct of the Understanding,' of which Mr. Hallam says, 'I cannot think any parent or instructor justified in neglecting to put this little treatise in the hands of a boy about the time that the reasoning faculties become developed.'"

In the sketch of Locke's life which follows, Dr. Brown shows that he began his career as a medical man, and that we have therefore a distinct claim to consider him as one of ourselves. It was to medicine that he was indebted for the bonds that knit him to the first Lord Shaftesbury, inasmuch as in the absence of his lordship's regular attendant during a visit to Oxford, he was called in to advise concerning the treatment of an abscess brought on by a fall from his horse. Locke received the credit of saving his patient's life, and appears to have subsequently resided with him as his family physician. It is not stated whether his lordship's previous attendant, Dr. Thomas, who seems thus to have been supplanted, took the occurrence in good part or not.

We must hasten to other topics, yet we cannot refrain from calling the following extract, which we submit to the special consideration of our readers, as the quintessence of the whole essay:

"Sydenham, the prince of practical physicians, whose character is as beautiful and as genuinely English as his name, did for his art what Locke did for the philosophy of mind; he made it, in the main, observational; he made knowledge a means, not an end. It would not be easy to over estimate our obligations as a nation to these two men, in regard to all that is involved in the promotion of health of body and soundness of mind. They were among the first in their respective regions to show their faith in the inductive method, by their works. They both professed to be more of guides than critics, and were the interpreters and servants of Nature, not her diviners and tormenters. They pointed out a way, and themselves walked in it; they taught a method and used it, rather than announced a system or a discovery; they collected and arranged their *visa* before settling their *cogitata*—a mean-spirited proceeding, doubtless, in the eyes of the prevailing dealers in hypotheses, being in reality the exact reverse of their philosophy." (p. 24.)

We have given above an extract intended specially for the guidance of students and their instructors; here is one for the practitioners of the day, which we think not less applicable to some of our over-scientific friends:

"One grand object we have in now bringing forward the works and character of Syden ham, is to enforce the primary necessity, especially in our day, of attending to medicine as the art of healing, not less than as the science of diseases and drugs. We want at present more of the first than of the second. Our age is becoming every day more purely scientific, and is occupied far more with arranging subjects, and giving names and remembering them, that with understanding and managing objects. There is often more knowledge of words than of things."

The second paper is entitled, "St. Paul's Thorn in the Flesh; what was it?" And it is a learned inquiry by the author's cousin, which argues, very conclusively, we think, though somewhat lengthily, that St. Paul used no metaphor in employing these terms, but spoke of a physical infirmity of the eyes. The writer's opinion is that the Apostle, in the various passages which may be brought to bear on the question, alludes to the diseased state of his eyes, accompanied by great short-sightedness.

The third paper is what our friends on the other side of the British Channel would term an *èloge* of Dr. Andrew Combe. If we were to judge of the influence which he exerted upon the medical mind of this country by the gratitude we ourselves owe him, we should perhaps attribute to him more than his due; and still, when we read Dr. Brown's estimate of him, we cannot but think that he has been to others what he has been to ourselves—the clearest expositor of the laws of health, and wisest guide to the practical performance of our professional duties, whom we could name among modern writers. Singularly clear in his statement of physiological facts, and of the arguments to be deduced from them; biased in no way that could lead him from the straight path which he persued; eschewing hypothesis because it could only mystify his teaching, he was, more than any medical author whom we know, the interpreter of Nature. His works are so remarkably free from the cant or jargon of all technicalities, that they are models of professional writing; and though suited for the educated general reader, they will certainly ever deserve to be specially appreciated by medical men. We trust that the younger generations of doctors have learned to feel that respect for Dr. Andrew Combe which he deserves at the hands of the entire profession; for we think, with Dr. Brown, that his 'Principles of Physiology' are still the best exposition and application of the laws of health. We equally subscribe in full to the remarks made by our author on his 'Physiology of Digestion,' and his treatise on the 'Management of Infancy.'

> "His 'Digestion' is perhaps the most original of the three. It is not so much taken up as such treatises, however excellent, generally are, with what to eat, and what not to eat, as with how to eat anything and avoid nothing, how so to regulate the great ruling powers of the body as to make the stomach do its duty upon whatever that is edible is admitted to it. His book on the 'Management of Infancy' is to us the most delightful of all his works; it has the simplicity and mild strength, the richness and vital nutriment of 'the sincere milk,' that first and best cooked food of man. This *lactea ubertas* pervades the whole little volume; and we know of none of Dr. Combe's books in which the references to a superintending Providence, to a Divine Father, to a present Deity, to be loved, honoured, and obeyed, are so natural, so impressive, so numerous, and so child-like."

Apropos of an anecdote about Opie, the painter, related at the commencement of the next essay, Dr. Brown discourses wisely on the *je ne sais quoi* necessary to make knowledge self-productive in the individual to whom it is imparted; our readers who, we believe we may say it without flattery, already possess that νοῦς, that "justness of mind" which is essential to constitute the sound practitioner, will see the gist of the whole essay when we have given them his anecdote. It is this:—"'Pray, Mr. Opie, may I ask what you mix your colours with?' said a brisk dilettante student to the great painter. 'With *brains*, sir,' was the gruff reply." What these brains are, and how the painter's observation about mixing his colours applies to the physician, we leave our readers to divine, or, perhaps, still better, to search out in the book itself. Let us follow our author for a short time in his *Excursus Ethicus*, which succeeds, and which with the various papers on medical ethics by Dr. Alison, Dr. Gairdner, and other well-known writers, that we have from time to time recommended to the notice of our readers, we should like to see widely diffused among the instructors as well as the learners of our profession. The author's observations are confessedly miscellaneous, and of an occasional kind, but "a rambling excursion" now and then is more invigorating and refreshing than the formal promenade. The former often brings you to unexpected and startling scenes; there is a charm in the surprise and in the prospect of adventure, which the latter cannot afford. We can only take a glimpse here and there at the regions through which the excursionist conducts us.

> "Were we for our own and our readers' satisfaction and entertainment, or for some higher and better end, about to go through a course of reading on the foundation of general morals, in order to deduce from them a code of professional ethics, to set ourselves to discover the root, and to ascend up from it to the timber, the leaves, the fruit and the flowers—we would not confine ourselves to a stinted browsing in the ample and ancient field; we would, in right of our construction, be omnivorous, trusting to a stout mastication, a strong digestion, an eclectic and rigorous chylopoietic staff of appropriators and scavengers, to our making something of everything. We would not despise good old Plutarch's morals, or anybody else's, because we know chemistry and many other things better than he did; nor would we be ashamed to confess that our best morality and our deepest philosophy of the nature and origin of human duty, of moral good and evil, was summed up in the golden rule of childhood—'Love thy neighbour as thyself.'"

After planning an ideal journey through the realms of mind, which it would be well for us all to take, a warning is put in, which it equally behoves the traveller to act upon:—

> "But, after all our travel, we would be little the better or the wiser, if we ourselves did not inwardly digest and appropriate, as 'upon soul and conscience,' all our knowledge. We would much better not have left home. For it is true, that not the light from heaven, not the riches from the earth, not the secrets of nature, not the minds of men or of ourselves, can do us anything but evil, if ourselves, our inward and outer senses, are not kept constantly exercised, so as to discern for ourselves what is good and evil in us and for us. We must carry the lights of our own consciousness and conscience into all our researches, or we will in all likelihood lose our pains."

We are glad to find that Dr. Brown is no advocate for that vague cosmopolitanism which is one of the extremes of modern philanthropy.

To a nation, as to an individual, what is more important than the influence of home —home ties, home feelings, home attachments, home control, and home advice? who that has been abroad, has not deplored that emancipation from the salutary check of social opinion which so often leads the Briton into physical and mental vagaries which he would not have dreamt of on his own soil? We cannot too strongly denounce the system which, for the sake of acquiring a correct pronunciation of a foreign language, detaches the youth from the sacred influence which surrounds him in his native country, and which helps to impart that strength and vigour which he must acquire before he can stand alone, and resist the buffetings and temptations of a strange world. Much as we may appreciate the advantage of travel and foreign intercourse, and desire to see an interchange of all that is good between different nations, it is above all things necessary "to carry with us the lights of our own consciousness and conscience," in order that the dazzling effect of novelty may not overpower our judgment. A man must indeed be "used up," who says:

"Cœlum non animum mutant, qui trans mare currunt."

The change is more likely to be too great than too small, unless, especially in a moral and intellectual point of view, the influence of a home atmosphere is an abiding one. Therefore, physically and mentally, beware how and under what circumstances the untutored child or youth is allowed to roam among the mountains and valleys of a foreign country or of foreign literature.

"An's Vaterland, an's Theure schliess Dich an,
Dies halte fest mit Deinem ganzen Herzen,
Dort in der fremden Welt stehst Du allein."

Whether at home or abroad, let our researches and our work be carried on with the remembrance

> "That there is something else than this earth—that there is more than meets the eye and ear—that seeing is not believing; and that it is pleasant, refreshing, and wholesome, after the hurry, and heat, and din of the day, its flaring lights and its eager work, to cool the eye and the mind, and rest them on the silent and clear darkness of night, 'sowed with stars thick as a field.' Let us keep everything worth keeping, and add, not substitute; do not let us *lose ourselves* in seeking for our basic radical or our primary cell; let us remember that the analytic spirit of the age may kill as well as instruct, and may do harm as well as good; that while it quickens the pulse, strengthens the eye and the arm, and adds cunning to the fingers, it may, if carried to excess, confuse the vision, stupify and madden the brain, and instead of directing, derange and destroy."

In the very able exposition of the advances made during the present century in military hygiène, which follows the *Excursus Ethicus*, Dr. Brown dwells eloquently upon the great services rendered to the cause by Dr. Henry Marshall, whose 'Military Miscellany' should, according to Lord Hardinge, "be in the hands of every army-surgeon and in every orderly-room in the service." Willingly would we draw upon this valuable essay for some of the interesting and cheering information it contains, but the inexorable Fates which control a reviewer force on our march. This brings us to a *morçeau* that we regard as the gem of the volume, a genial, warm-hearted bit of medical experience which, for truthfulness of colouring and drawing, for elegance of conception and propriety of execution, may take rank with the best literature of the day. As medical men, we would especially thank Dr. Brown for the manly and hearty sympathy which he expresses for the poor and the suffering, for the example he here holds forth to the student never to forget in the scientific interest the claims of humanity. There is nothing in the 'Diary of a Physician' that can be compared to "Rab and his friends." It is a simple tale. It records an event such as daily occurs in the walls of a hospital; its subject is the same as that related by Warren in the 'Diary' under the title of Cancer; but there is a reality about it, a healthy vigour in the sympathy and sentiment which run through it, which give it a peculiar stamp. The opening is so characteristic that we cannot refrain from giving it as a *bonne bouche* which must, we think, make our reader's mouth water to enjoy a taste of what follows:—

> "Four and thirty years ago, Bob Ainslie and I were coming up Infirmary-street, from the High School, our heads together and our arms intertwisted, as only lovers and boys know how or why.
>
> "When we got to the top of the street and turned north, we espied a crowd at the Tron church. 'A dog-fight!' shouted Bob, and was off; and so was I; both of us all but praying that it might not be over before we got up! And is not this boy nature? and human nature too? and don't we all wish a house on fire not to be out before we see it? Dogs like fighting; old Isaac says they 'delight' in it, and for the best of all reasons; and boys are not cruel because they like to see the fight. They see three of the cardinal virtues of dog or man—courage, endurance, and skill—in intense action. This is very different from a love of making dogs fight, and enjoying, and aggravating, and making gain by their pluck. A boy—be he ever so fond himself of fighting—if he be a good boy, hates and despises all this, but he would have run off with Bob and me fast enough; it is a natural and not a wicked interest, that all boys and men have in witnessing intense energy in action.
>
> "Does any curious and finely ignorant woman wish to know how Bob's eye at a glance announced a dog-fight to his brain? He did not, he could not see the dogs fighting; it was a flash of an inference, a rapid induction."

We wonder whether Tom Brown—*the* Tom Brown whose school days are the delight of old boys and young—is a near relative of our author. The family-likeness can scarce be denied, and we wish we might prove it by quoting the whole of the ensuing *κυνομαχία*. However, we have often thought, while enjoying the pith which a reviewer had abstracted from a book, that an injustice was being done to the author, inasmuch as many readers would be satisfied with having thus got at the quintessence of the book without the trouble of extracting it for themselves. We will be open to no such imputation. We should therefore say to all who have any faith in our judgment upon such matters, Go and buy the book, were we not afraid of seeing so commonplace a piece of advice quoted upon our authority in the ordinary vehicles of booksellers' advertisements: "Doctor John Brown's 'Horæ Subsecivæ.' Go and buy the book.—'British and Foreign Medico-Chirurgical Review.'" To prevent the possibility of misunderstanding, we therefore emphatically disclaim and deny having given the advice that would be conveyed in such an announcement.

"Rab and his Friends" does not conclude the book, which contains several other articles of value; but for more reasons than one we can say no more about it than this, that should it be our good fortune to meet Dr. John Brown in the course of our pilgrimage, we should wish to ask him to allow us the pleasure of giving him a hearty shake of the hand, and of thanking him for enabling us to add one more name to the roll of worthy names who are an honour to our common profession and our common country.

REVIEW IV.

1. *Beitrage zur Heilkunde.* Von Dr. BÖCKER.—*Crefeld,* 1849. Band II. p. 324.
Contributions to Medicine. By Dr. BÖCKER.
2. *Untersuchungen über die Harn und Harnstoffmengen.* Von H. BEIGEL.—('Aus Nova Acta, &c.,' besonders, abgedruckt.)—*Wien,* 1856. pp. 49.
Researches on the Quantities of Urine and of Urea. By Dr. H. BEIGEL.
3. *Beitrage zur Pharmakodynamik des Brechweinsteins.* Von Dr. THEODOR ACKERMANN. ('Zeitschrift für Rationelle Medicin,' von Dr. J. Henle und C. v. Pfeufer. Dritte Reihe, Band ii. p. 241.) 1858.
Contributions to the Pharmaceutical Action of Tartar Emetic. By Dr. ACKERMANN.

IT was to be expected that the easy analytical methods of determining the amounts of the chief urinary excretions, to which we have alluded in our Review 'On the influence of Baths,' would soon be applied to elucidate the action of the principal medicines. This is, in fact, now being done on an extensive scale, and we propose to select for review on the present occasion the late researches which have been made in this direction into the effects and the mode of action of TARTAR EMETIC.

Some years ago, investigations* into this subject were made by the indefatigable Dr. Böcker, of Bonn, an observer whose name will be very familiar to our readers in connexion with observations on the influence of tea, coffee, wine, beer, &c.† Dr. Böcker's inquiries into these and similar subjects have been received in Germany with some scepticism, chiefly, we presume, because they seemed almost too elaborate and too extensive to have been performed by one man. But we must say that we can see no good reason for doubting their general correctness, and as they have been verified in several instances, even by persons who were not disposed in the first instance to attach much credit to them, we cannot pass them over without notice.

Dr. Böcker's experiments were made on himself. He submitted to the martyrdom of taking for nine successive days no less than two grains and a half daily of tartar emetic. Naturally he lost for the time his usually good appetite, and was able to take very little food. Probably from this cause, and not from any lessening of metamorphosis or retention of excretion, the elimination of solids by the kidneys was considerably lessened,‡ although the water was slightly (immaterially) increased. The experi-

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* We pass over the experiment recorded by Mayenofer (Heller's Archiv, 1846), as it is quite useless. The urea, from a per-centage determination, is said to be increased.

† See vol. xiv.

‡ The solids of the urine were lessened by 7·160 grammes, or 110½ grains in each 24 hours.

" urea	"	"	4·874	"	75	"
" uric acid	"	"	0·002	"	0·8	"
" fireproof salts	"	"	0·086	"	1	"
" volatile salts and extractives			2·307	"	35	"

The most curious result here is the very slight diminution of the fireproof salts—the sulphates, phosphates, chlorides.

ment, in fact, was, on account of the partial deprivation of food, so complicated as render Dr. Böcker's self-devotion really of little use.

He noticed, however, one fact—viz., that with this lessening of urinary excretion ther was simultaneously an increase in the quantity of carbonic acid expired from the lungs. Some exception may, perhaps, be taken to Böcker's experiments on this point; but the statement is interesting in connexion with more exact experiments presently to be detailed.

Beigel, the next observer whose experiments we have to notice, was not so self-sacrificing as Dr. Böcker, and used as the subjects of his researches four unlucky patients, to whom he gave for four days two grains of tartar emetic daily in divided doses, and whom he thus kept in almost constant nausea. The amount of food taken by these unhappy victims to science is not stated, but it is certain they must have been anything but good trencher-men, and we are not surprised to hear that the urea (the only ingredient determined by Beigel) was greatly and invariably lessened (by about five grammes = seventy-seven grains daily). The water was sometimes increased, sometimes lessened.

Beigel (to whose researches we attach on the whole little importance, so incomplete are many of them, and so carelessly are the results calculated) believes that this lessening of urea was due to a true "antiphlogistic action" of the tartar emetic, and not to any interference with the reception and digestion of food. But we cannot allow that there is satisfactory evidence for this opinion. As long as we can find an adequate cause for a result (and this is indubitably given in this instance by the insufficient supply of food, which made the urine like the urine of inanition), it is surely not correct to assign another cause for the diminution of urea. Clearly the onus of proof still rests with those who affirm the antiphlogistic action of the tartar emetic.

No observations were made by Beigel on the excretions of the skin, the lungs, or the intestines.

A much more complete investigation has been made by Dr. Ackermann, and we shall not apologize for analysing his elaborate essay at some length.

Dr. Ackermann attempted to determine the effect of tartar emetic on the pulse, the temperature of the body, the respiration, and the excretions. In examining the urine he was able only to examine the quantities of the urea, uric acid, the chloride of sodium, the free acidity, and the pigment (after Vogel), and therefore he has left much for future observers to do. Still his results are highly interesting.

Before giving an account of his experiments, some detail of his method is necessary.

He selected three healthy young men, between twenty-one and twenty-three years of age, and made three sets of experiments on each. Between any two experiments on the same person, at least six days were allowed to intervene. On the day before the experiment the person experimented on was kept on regulated diet; on the day itself nothing was taken except a morning cup of coffee. He was weighed after the bowels and bladder had been emptied, and then went to bed and remained tranquilly in it for nine hours. Every five minutes the pulse and respirations were counted, and the temperatures of the mouth and of the hands were determined. Dr. Ackermann details at length the means he adopted for avoiding fallacies in determining this latter point. After the person had remained in bed an hour a certain quantity of tartar emetic was given, usually four centigrammes, in warm infusion of orange; and this dose, doubled or halved according to its slight or severe effect, was repeated.

One hour after the tartar emetic had been taken the bladder was emptied, and from that time to the end of the experiment the urine was considered to represent the composition it would have under the influence of the medicine. The amount of "insensible perspiration" (skin and lungs) was known of course by deducting the weight of the urine and fæces (if any) passed during the period, from the total loss of weight of the body as determined at the end of the experiment. By this method it will be seen that the influences of food and of exercise were avoided, as the system was fasting, and the body was tranquil; and that the skin was kept as far as possible at the same temperature, and under the same conditions throughout.

We now proceed to give the results.

1. *The Pulse.*—For a variable time, according to the individual and the dose (length of time being inversely as the dose), no effect was produced in any way on the system by the tartar emetic. Then commenced an acceleration of the pulse; and, either at the same time, or usually a few minutes later, the feeling of nausea was perceived. The quickening of the pulse increased with the degree of nausea, rose rapidly just before, and reached its maximum with, the vomiting; then declined slightly, and remained stationary for some time, and subsequently underwent changes to which we shall presently allude. This quickening of the pulse was so intimately connected with the nausea, that it might be taken as a measure of this; it even varied in amount as the nausea increased or decreased temporarily, before the sickness fairly come on. As with the nausea, so with the pulse, its rapidity and quickness of rising increased as the dose of tartar emetic was increased. The mean maximum of the rise (of all three men) was forty-two beats per minute. Never on any one occasion during the period of nausea, did the pulse, even after vomiting, sink to the normal standard.[*]

The pulse was not only quicker, it was also weaker—i.e., there was a diminution in the largeness of the wave of blood, as felt by the finger; when this diminution reached its highest point, a diminution in the strength and extent of the heart's impulse could be perceived. The frequency and strength of the pulse were therefore in an inverse ratio.

After the cessation of the nausea the pulse fell somewhat, yet still remained frequent for some time; then it increased again in frequency, and then finally declined. This last or second rise was never equal to that of the primary rising, it was earlier and greater in proportion to the dose. The largeness of the pulse was greater than in the primary rise,[†] and the heart's action increased in amount.

Fall in the number of beats of the pulse below the healthy frequency, as described by so many observers, has never been seen by Ackermann on any single occasion in the first eight hours after a small or medium dose of tartar emetic.

The weakness of the pulse—i.e., diminution of lateral pressure, has been proved by Lenz by hæmadynamometric researches.[‡]

The increased *frequency* of the pulse is referred by Ackermann to a weakened or paralytic condition of the vagus. Whether the *weakening* of the pulse is to be attributed to a like cause is not so clear.

The effect of section of the vagus (of course besides increasing the frequency of the heart's action) is, according to Volkmann, Nasse, Lenz, and Brown-Séquard, to lessen eventually the pressure of the blood on the arterial walls, although it at first causes an increase. The experiments of Ludwig, Jacobson, and Fornelin, however, lead to a different result, for after section of the vagi there was a permanent increase in the force of the heart. While experiments are thus contradictory, the lessened size of the pulse due to tartar emetic cannot be safely referred to paralysis of the vagus. It may possibly be partly due, Ackermann surmises, to arterial cramp.*

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[*] These results accord with some formerly made by the same author; they are in accordance also with the statement of Paris, who speaks of the pulse as "feeble, *quick*, and irregular," during nausea, and of Neumann. Dr. Ackermann does not deny, though he does not expressly affirm, that, as stated by a great number of authors, the quickened pulse of pyrexia may be lessened in frequency during the nausea of tartar emetic, and not increased, as the healthy pulse would appear to be under the same circumstances.

[†] Ackermann gives the following scheme to explain his views (which are of course not novel), on the size of the pulse. The quality of the pulse—i.e., the amount of lateral tension—is the result of two factors: the pressure under which the blood flows in the vessels, and the resistance which the arterial walls exert on the distending blood. An increase in distending *pressure* is produced by the increased force of the heart, or by an increased resistance in other parts of the vascular system. The amount of *resistance* depends on the degree of elasticity and contracting power of the vessels. The following scheme shows the varieties:

1. Pressure remaining unaltered, with—
a. Unchanged resistance = pulse is unaltered.
b. Increased " = pulse small and hard.
c. Diminished " = pulse large and soft.
2. Pressure increased, with—
a. Unchanged resistance = pulse large and hard.
b. Increased " = pulse unaltered.
c. Diminished " = pulse large and hard.
3. Pressure diminished, with—
a. Unchanged resistance = pulse small and soft.
b. Increased " = pulse small and soft.
c. Diminished " = pulse is unaltered.

[‡] Experimenta de ratione inter pulsus frequentiam, sanguinis pressionem lateralem, et sanguinis fluentis celeritatem obtinente.—Diss. Inaug., Dorpat, 1858. Quoted by Ackermann.

Whatever may be the cause of the lessened force of the heart, its consequences are plain enough; a certain amount of venous and capillary hyperæmia must occur, as in these vessels the resistance to the flow of blood is from obvious circumstances greater than in the arteries, and this hyperæmia occurs most in the parts remotest from the heart, as the hands and feet. A number of phenomena result directly from this congestion or hyperæmia; the temperature of the skin of the hands lessens; a bluish red (cyanotic) congestion of them and of the face is observed; and a cold sweat appears on several parts of the skin. Of these results the most interesting are in

2. *The Temperature.*—Some minutes (five to thirty) after the primary quickening of the pulse at the commencement of the period of nausea, the temperature of the hands was clearly found to diminish; it had a relation to the rapidity of the pulse (and to its weakness?), and was greater also in proportion to the dose of the tartar emetic. It was in proportion to the cyanosis of the hands and face. The coldness of the hands gave the patient a sensation of general cold.†

The cyanotic appearance of the hands and face disappeared, and the temperature of the hand usually rose (even above the normal height), after the nausea and vomiting were over, and about the time when the pulse, after remaining at the same point for some time, commenced to have what Ackermann calls its secondary rising; and when the impulse of the heart and the size of the pulse were alike increased. Although the temperature of the extremities was thus at first lowered, the *temperature of the mouth was not so;* on the contrary, it was either at the normal height, or had unimportant variations, till the period of the secondary rising of the pulse, when it became much increased (about 1° Fah. on an average). The fact that the temperature did not fall during the nausea period, when the heart's action was quick and weak, is not considered by Ackermann to militate against his hypothesis, that the fall of temperature in the hand depended on the cyanosis, for he observed that this venous and capillary congestion would of course be much less near, than at a great distance from, the heart.

3. *The Respirations.*—The number of respirations was increased, and followed the alterations in the pulse, i.e., the respirations at first were quickened, lessened after the nausea was over to the normal amount, and then rose somewhat again with the secondary rising of the pulse.

4. *The Excretions and Secretions.*—Simultaneously with the nausea there occurred an increase in the quantity of saliva, which reached its maximum shortly before the vomiting. It came principally from the parotid, and gave rise to frequent deglutition. As we know from Ludwig's experiments that this may have been from irritation of the nerves of the gland (either direct or reflected), it may be presumed that tartar emetic acts on the salivary branches by reflected irritation from either the glosso-pharyngeal nerve, the gustatory branches of the fifth, or the vagus—probably the latter. Or, as Kölliker has lately argued that increase of saliva from the action of Urari depends on nervous and vascular paralysis, it may be supposed that the increased salivary flux caused by tartar emetic may be due rather to this cause than to irritation, and the other depressing and paralytic effects of the drug give probability to this supposition.

The effect on the urine and insensible perspiration was the following. On the whole, tartar emetic augmented very much the total excretions, and this in proportion to the dose. The "insensible perspiration" (skin and lungs) was increased by half in some cases, in others doubled, or even more than doubled. This depended chiefly, no doubt, on the clammy sweats of the nausea period, and the warm perspiration after vomiting; but probably partly also an increase of pulmonary exhalation; and here we may recall to mind the experiments of Böcker on the excretion of carbonic acid. It would be very important to examine these two excretions separately.

The quantity of vomited matters depended on the largeness of the dose. In some cases there was purging. The amount of the water of the urine was lessened, and more so in proportion to the dose and to the amount of the other excretions. The quantity of urea was, however, increased, on an average, by one-eighth, or in some experiments even by one-fourth; the greater the dose the greater (almost constantly) was the increase.

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* It is curious enough to find how often modern research makes use of such terms, and refers to such phenomena, as cramp and spasm of the vessels—phrases so familiar to us in the writings of fifty to one hundred years ago.

† Just as Thierfelder has shown that in typhoid and typhus fevers the patient feels hot when the hands and face are hot; it is the state of these parts and not the real heat of the blood which causes the feelings of heat and coldness in febrile cases.

The amount of chloride of sodium lessened especially in the cases in which, besides vomiting, there was diarrhœa. The pigment (determined by Vogel's method) and the uric acid increased generally; the amount of free acidity was variable.

Some of these results are easily explicable; the diminutions of urinary water and of chloride of sodium are accounted for partly by the fasting, but chiefly from the passage of these substances in increased quantity from the stomach, bowels (in some cases), and skin. That the skin plays a part here is evident from the fact, that even when there was no vomiting or purging, the amount of urinary water lessened. But the decided and invariable increase in the urea, and the usual increase in the pigment and uric acid, are much less easily explained.

As the urea was thus increased in a fasting system (when it would normally have lessened), and when the skin was acting freely, the increase was not owing to supply of food or checked perspiration. Was it due to increased metamorphosis, or simply increased elimination? Ackermann refers it to the former, and seems to believe that tissue-change became extremely active in what we may term the period of reaction, i.e., in the period after the nausea and vomiting, when there was the so-called secondary rising of the pulse, and when also the cyanosis of the hand disappeared, and the temperature both of hands and mouth augmented. In order to determine this question a series of hourly analyses, at the commencement, and at the height of, and after the nausea period, are necessary; and these Ackermann has not made. In his table, the urine of the whole tartar-emetic period is put together, and is compared with a corresponding period without medicine. It is certainly more likely that the ureal increase is due rather to augmented metamorphosis, especially during the latter period of the action, than to merely increased elimination; but further experiments must definitely settle these points.

On the whole, the most difficult phenomenon to explain in the whole inquiry is this said increase of the urea; and it is to be remembered that (though we do not attach a great importance to them) Böcker's results are entirely opposed in this particular to those of Ackermann.

One of two hypotheses must be adopted, if Ackermann's statement be received: first, either the urea was increased during the whole period of the action of the tartar emetic; or, second, it was increased, as Ackermann appears to maintain, only during that period of returning force of the pulse and heightened temperature which accompanied the complete cessation of the nausea. In the first case, it seems highly unlikely that, with a weakened heart and a general capillary and venous congestion, proceeding from weakened circulation, there should have been such great increase of tissue-metamorphosis. In the second case it seems equally unlikely that the tissue-metamorphosis should have been so great as to raise the urea to so great an amount.

We have looked anxiously at Ackermann's experiments to detect some fallacy, but not only do we perceive none, but it would appear that not only was the increase constant, but it augmented (with one exception in seven experiments) with the dose of tartar emetic.* Still, without being able to allege any good grounds for our doubt, we must confess we still do doubt on this point, and shall be glad to have Ackermann's observation confirmed or disproved by others.

To sum up Ackermann's results:—

1. Tartar emetic quickens the action of the heart, possibly by partially paralysing the vagus.

2. It weakens, at first, the force of the heart from an uncertain cause (paralysis of vagus?); subsequently, when its effects are passing off, the heart's action augments in force.

3. It produces, at first, general capillary and venous hyperæmia, on account of

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* We could have wished, however, that Ackermann had stated his experiments on the urine in greater detail.

the weakened heart; and afterwards, general relaxation of the vessels, when the effects are passing off, and when the heart's action increases.

4. It causes increased action of the skin during the hyperæmia (cold and clammy sweats), and during the after period of increased action (warm sweating).
5. It lowers during the first period the temperature of extreme parts, and subsequently causes a moderate rise of temperature over the normal amount.
6. It increases the number of respirations, and probably augments the pulmonary exhalations.
7. It augments the stomach and intestinal secretions, producing vomiting and sometimes purging.
8. It augments the flow of parotid secretion, whether from irritation (direct or reflected), or paralysis of the nerves, is uncertain.
9. It lessens the urinary water and the chloride of sodium, on account of the increased discharges from the stomach, intestines, skin, and lungs.
10. It augments the urea and uric acid, from heightened metamorphosis (?).
11. On the whole, in spite of the lessening of urinary water, it augments the total excretions, even when there is no vomiting or purging, and this is produced especially by its great action on the skin.

We could have wished that the observations by Ackermann had been carried still further, and that the effects of doses of tartar emetic too small to produce nausea, had been determined, but this must be left for future observers. In the meantime, we believe that Dr. Ackermann has made a valuable addition to our knowledge of the action of this important drug.

Such is the effect of tartar emetic on the *healthy body;* what are its effects in *disease?* We need scarcely observe, that the influence of a remedy on the body in disease is not necessarily of the same kind as that on the body in health, though of course it is very probable that the effects will be found to be analogous, if not identical. Independent observations must, however, be made in each separate disease, and a great field is thus thrown open for exact therapeutical inquiry. At present we are not in a position to do more than indicate what seem to be the chief agencies which tartar emetic exerts in the diseases in which it is usually given.

As remarked by Ackermann, tartar emetic, by reducing the force of the heart and lowering the temperature and increasing elimination, is entitled to the reputation it has acquired in febrile and inflammatory states. Still it is not a febrifuge in the sense in which quinine or arsenic is. No amount of it will cut short a fit of ague, and it must be termed, not a radical but a symptomatic febrifuge.

In bronchitis and pneumonia it acts in the same way by its action on the heart, the circulation, and the emunctories. But it has here a special action, as it appears to aid expectoration, which it does not only by the mechanical act of vomiting, but also by producing a sort of liquefaction or diminution of consistence of the exudations in the bronchial tubes and air cells. This last and very important effect seems to be tolerably certain, and it is conjectured by Ackermann to occur by an increased transudation of blood serum through the pulmonary capillaries, in an analogous way to that in which the increased sweating of the skin is brought about. In hæmoptysis, the good effect of tartar emetic must be attributed to its depressing effect on the force of the heart, and on the diminution of the lateral pressure on the vessels.

In asthma, its good effect is produced by an increase in the expectoration, but as it is also beneficial for the time when it does not do this, and when, in fact, there is little expectoration to be got rid of, Ackermann conjectures that it may act by producing in the vagus not only a degree of paralysis, but also a diminution of its power of conducting sensation, so that the changes in the pulmonary organs, which by reflex action cause the asthma, produce a lessened influence on the nervous centres.

As an addendum to these observations of Ackermann's, we may mention some experiments lately made by Hoppe,* on the effects of tartar emetic applied locally to the eyes of rabbits. When one-eighth of a grain of tartar emetic was placed in the eye of a rabbit, it caused at first some sensitive irritation, and a certain moderate amount of vascular irritation and hyperæmia. A second quantity of one-eighth of a grain (making a quarter in all), caused contraction of vessels and exudation. Two days afterwards the exudation disappeared, the contraction of the vessels still continued. On the third day the contraction began to diminish, and was succeeded by enlargement of the vessels, which, after lasting for some time, lessened and disappeared. Then, without any re-application of the tartar emetic, fresh contraction came on, and was followed again by dilatation, and these alternations appear to have occurred several times, so that the eye was not entirely normal even on the five hundred and eighty-ninth day after the experiment. Hoppe considers that the action of tartar emetic is especially exerted directly on the vessels, and he somewhat dogmatically pronounces all the indirect reflex, paralytic, "inhibitory" theories of inflammation to be incorrect. The most remarkable part about his experiment, is the immense length of time the effects of so small a quantity of tartar emetic lasted. At the end of more than a year and a half, the vessels of the eye were still unsound from the local action of half a grain of tartar emetic!

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* Die Gefässwirkung des Tartarus stibiatus und des Sulphur stibiatum aurantiacum, untersucht am Auge des Kaninchen, von Prof. Dr. J. Hoppe; Zeitschrift der k. k. Gesellschaft der Aerzte zu Wein, Nos. 45, 46. 1858.

Surely some other causes must have co-operated in producing such a duration of symptoms which are usually supposed to be comparatively transient.

Review V.

Principes de Méchanique Animale, ou Étude de la Locomotion chez l'Homme et les Animaux Vertébrés. Par Felix Giraud-Teulon, Docteur en Médecine de la Faculté de Paris, ancien élève de l'École Polytechnique; ouvrage accompagné de 65 figures intercalées dans le text.—*Paris*, 1858. pp. 483.

Principles of Animal Mechanics, or Studies on the Locomotive Organs of Man and Vertebrate Animals. By Dr. Felix Giraud-Teulon. With Illustrations.

Mechanik der Menschlichen Gehwerkzeuge. Eine Anatomisch-Physiologische Untersuchung. Von der Brüdern Wilhelm Weber, Professor in Göttingen, un Eduard Weber, Prosector in Leipsic. Nebst einem Hefte mit 17 Tafeln Anatomischer Abbildunger.—*Göttingen*, 1836. pp. 426.

The Mechanism of the Organs of Locomotion of Man: an Anatomico-Physiological Inquiry. By the Brothers William Weber and Edward Weber.

The work of Dr. Felix Giraud-Teulon is one of considerable merit; but we think that it might have been compressed with advantage, and that the author scarcely does justice to the amount of intelligence generally diffused at the present day.

The first chapter is devoted to the subject of muscular action—that is to say, of the two varieties of contractility of which Bichat wrote, the one voluntary, the other tonic or involuntary. The latter is rightly described as a force which, like voluntary contractility, performs a definite *rôle* in the maintenance of equilibrium in the animal mechanism; it presides over the equilibrium of repose in the same way as the former governs the state of movement. Let us analyze Section I.

The author is not quite content with the observation of Bichat, who, speaking of muscular action, says:

> "Whatever may be the spots of attachment which serve as the fixed point or the moveable point, they (the muscles) always act in the inverse sense of the supposed direction proceeding from the first point. After this general rule, it is sufficient to see a muscle in the dead subject to pronounce its use."

Admitting that this principle is true in a great number of circumstances, Dr. Teulon goes on to say that

> "All movements are far from being so simple as we imagine. If at every instant they are subject to the most ordinary law of the lever, at every instant the conditions of their law may vary; and they do vary, in truth, in consequence of the simultaneous action of some other muscle, of which nothing would tell us *à priori* that we ought to take account. Thus the deltoid has for its evident effect to raise the arm; and nevertheless it has been known only for *a very short time*, that it is unable, without assistance, to bring the limb beyond the horizontal. The combination of the serratus magnus is necessary to bring about in its whole extent the complete movement which from all antiquity has been referred to it." (p. 7.)

Now this error, as well as many others of a similar kind, has long been recognised in the anatomical world; it originated in conclusions deduced from shandy dissection and faulty experiment. It was taught at the schools and repeated at the examining boards long after it had been banished from every scientific work. But for many years the *precise* attachment of muscles has been made a special matter of investigation, and the conclusions drawn from such dissections, though often differing from preconceived suppositions, have yet borne out the generally received law expressed by Bichat. We go farther even than this; we recognise powerful muscles, such as the deltoid, trapezius, and serratus magnus, by which the anterior or upper extremity and the trunk are firmly united in a variety of positions, as well as a subjacent set of muscles, which act as fine-adjusters, such as the suprà and infrà spinatus, the teres minor and subscapularis. Let Dr. Felix Giraud-Teulon witness the small-sword practice of some of his martial countrymen. He will see the upper extremity firm in its relation to the trunk, owing to the continued action of the larger muscles, while the rapid rotation of the limb is such as to leave the hand free to direct the sword's point in any direction; and yet both sets of muscles are acting in concert, though physiolocally distinct. We may ascertain the action of a muscle by learning upon the dead subject its exact points of attachment, but in drawing conclusions as to its action during life, we must use common sense, and remember that were muscles to act singly, movements would be sharp, sudden, angular, and inelegant, such as they are not in nature.

Of the voluntary contractility of muscles it is unnecessary to speak here. Of the tonic or involuntary contractility, Dr. Giraud-Teulon observes that Borelli, having a false idea of this tonic force, imagined that, in order to produce its useful effect, the muscles had need of being solicited by a certain resistance on the part of their antagonists. He rested this opinion on the false interpretation of a true but badly comprehended fact (vide p. 12). But, says the author, it is inexact to advance that one group of muscles finds its power only in the resistance of an antagonistic group; and Barthez, a century and a half later, was equally in error when he attributed to muscular tissue the very extraordinary property which he designated the fixed situation of muscles. This idea was suggested in consequence of the confusion which he made between the situation of the levers and that of the forces in the state of equilibrium. All that can be said is that the osseous levers are constantly solicited, in the direction of their movements and around their *points d'appui*, by contrary forces equal during repose, but differentially unequal during movement. If it were otherwise, movements, instead of being uniform, would become uniformly accelerated; and the passage from repose to action could not operate without shocks and serious errors of the working machine.

The author describes with spirit the "muscular sense," as conceived by the late Sir Charles Bell. He tells us that M. Duchenne, of Boulogne, has lately designated it "muscular conscience." We do not see that the latter term is the more expressive.

Dr. J. Guerin affirms that he has made the discovery of a contractile property of the tendons of certain, and perhaps of all the muscles. The author warns us to be circumspect in receiving this view without further confirmation, in which advice he is doubtless correct. The muscular tissue, during its contraction, gains in thickness what it loses in length; it does not experience any condensation. This opinion was affirmed by Borelli in opposition to Stenon, and was demonstrated by the researches of MM. Erman and Gerber.

We pass over Section II., which is devoted to a disquisition upon "the mechanical and geometrical errors of Borelli," and proceed to Section III., On the Movements of the Limbs. The levers are generally of the third kind (interpuissant), and that which the moveable extremity loses in power, it gains in rapidity; but when cases arise in which Nature wishes to produce power rather than rapidity, then there is a lever of either the first or the second order.

Chapter II. is devoted to the consideration of the erect posture (*la station*). The levers destined to transmit to the ground the weight of the trunk in quadrupeds cannot be compared to vertical columns. They communicate successively the charges which they support, under notable inclinations of levers, one upon the other. The muscular effort which strives against weight, is entirely furnished by the extensors, successively, of each articulation, &c. By the aid of a diagram, the author endeavours to demonstrate precisely the mode of transmission of that action of gravity from the point of suspension of the trunk on the member to the ground; and he points out how the base of support in quadrupeds, embracing a vast quadrilateral space by the four members, has nothing to lose or to gain by the relative size of such members. But the case is different when the animal is supported by the posterior members alone, such as is the case in man, and in birds.

A bird is constructed to support itself alternately in the air and on the ground, so that its centre of gravity has to obey two different conditions. In the first case it must be situated under the interglenoid axis; in the second, above the intercondyloid axis. The necessities of flight require that the trunk of the bird should not be vertical; the smallest surface is to be presented to the air. But then the weight of the body is thrown so much in front that the base of support must come similarly forward to meet the projections of the line of gravity in the ordinary act of standing.

In man, a vertical line passing through the centre of gravity of the whole system (head and trunk united), starting above the foramen magnum, cuts the four superior cervical vertebræ, the four first lumbar, falls upon the centre of the last, and finishes by meeting the axis of horizontal suspension of the trunk on the femoral articular heads. The real equilibrium of the trunk is very near a geometrical equilibrium. But this state of geometrical equilibrium is very unstable, and the trunk describes a succession of constant oscillations, being pulled alternately backwards and forwards, as regards the mere position, now by the flexors, now by the extensor muscles. It is a condition of constant balance.

Thus far we have quoted opinions concerning which there are no important differences; but, in Section 5 (p. 84), "On the Passage from Unstable Equilibrium to the State of Fixed Equilibrium," some startling novelties are introduced by the author, to which we must call attention. Dismissing the opinion of Borelli, who attached importance to the elastic properties of the intervertebral cartilages, and that of the Webers upon the influence of the ligaments, Dr. Giraud-Teulon says:

> "In truth, to account for the possibility of steadily maintaining the upright attitude, we must have recourse to some other explanation. According to the present state of our knowledge, the equilibrium in the upright posture depends upon the sole motion of the contraction of the extensor muscles; and when this has lasted too long (that which our experience teaches us quickly enough), this attitude cannot be conceived, otherwise than as a variable one, alternating at every moment between flexion and extension. It is therefore an equilibrium particularly unstable. Is it not known, by our daily experience, that active muscular contraction cannot be sustained in a continued manner without determining, and that very quickly, an intolerable amount of fatigue? Try to support with the hand any object at a distance from the body; few persons can continue the act longer than ten minutes. How much time, exercise, and custom is required to maintain for the same period, or even less, the position of simple guard in fencing, a position depending manifestly on the active contraction of the triceps below, and of the deltoid above, without going further into detail. And yet we can remain twenty-minutes, or half an hour, without any previous exercise, in the immovable upright posture, and that without notable oscillations." (pp. 85–86).

We presume Dr. Giraud-Teulon concludes that the extensor muscles, and particularly those of the spine, would become tired of acting continuously ten minutes, like the deltoid or the triceps. But we would beg the author to remember that the term erector spinæ is given, not to one, but to a group of muscles, composed of many thousands of muscular and tendinous slips, each of which can act independent of the rest, to afford relief to those parts of the extensor which are tired, to correct any faulty position, and to maintain the proper equilibrium. We deny that any human being can stand many minutes without "notable oscillations." Dr. Giraud-Teulon may feel them in his own person. The most complete immobility of the human body is in the state of rigid extension by the fixed voluntary contraction of the extensor muscles. Then, as the Webers first proved, by the peculiar arrangement of ligaments, the hip and the knee become fixed and motionless, the spine held together as one bone, but the muscular spasm once over (and it lasts, as Dr. Giraud-Teulon rightly observes, but a few minutes), the equilibrium of the body is maintained by the endless change of combinations of the various muscles, among which the many thousand extensores spinæ are the most remarkable. It is with surprise that we read of a discovery by M. J. Guerin of "tedinous contractility," that tendons unconnected with muscle, visibly swell under the control of the will; that they are endowed with a contractility peculiar to themselves, and subject to the will or the instinct. "May not the same be affirmed," says the author, "with regard to ligaments and the fibrous tissues which invest the articulations? May not the upright position of the spine be connected with tendinous contractility?" (p. 86). Undoubtedly, we reply, if the supposition can be proved. But we see no necessity for the existence of such a power; indeed, the contrary; for the maintenance of any fixed positions in the animal body must depend on the action of an extensor muscle pulling upon and keeping to the utmost stretch some inelastic band. When the position is permanent, as in the spine, a multitude of muscles act in harmony and succession in effecting the same object. Some tendons and ligaments are more elastic than others, and in these instances the amount of elasticity is in inverse ratio to the perfect stability of the part.

The author quotes at length the experiments and conclusions of M. Duchenne, of Boulogne, on the functions of the muscles of the foot, and we really cannot see the occasion for the extract. The muscular nomenclature seems to be more involved than ever, while the inferences scarcely possess a claim to either novelty or increased accuracy. What will the anatomist say to this sentence?—

> "Let us pass to the *flexor*-abductor muscle,—i.e., the *extensor*-longus digitorum. (p. 95.) The principal flexor is an adductor: the tibialis anticus. (p. 94.) The triceps suralis (or gastrocnemius and soleus) is called the *extensor*-adductor, and acts of course with the subjacent set of muscles, which are the *flexor*-longus pollicis, &c."

The movements termed flexion and extension of the foot are misnamed. By reversing our ideas of these two positions, we acquire the natural grouping of the sets of muscles in front and behind the leg. We do not agree with Dr. Duchenne that all the extensor and flexor muscles produce at the same time abduction or adduction. The great muscles of the calf of the leg simply point the toes downwards, although inversion of the foot soon follows, in consequence of the part taken secondarily by the deeper layer of muscles, among which the tibialis posticus, not named in the extract before us, plays the most important part. Sufficient attention is not paid to the movements in the tarsal and metatarsal articulations, by means of which the sole of the foot is enabled to grasp the uneven surface of the ground, while the astragalus retains its upright position, and receives through the tibia the weight of the body. But let us pass on to Section 4, On the Equilibrium of the Pelvis.

The annular conformation of the pelvis is required for a special function in the economy—namely, parturition; but in a mechanical point of view it is a winch suspended on its pivots or supports (*un treuil suspendu sur ses tourillons*), and the forces which are applied to it make it a lever of the first order, whatever may be their direction.

The sacrum has hitherto been regarded as a wedge, the summit being inferior and impacted between the ossa ilii. But this view is incorrect. The sacrum is a wedge, the summit of which is superior, suspended between the two ossa ilii by the very strong sacro-sciatic ligaments. Owing to its obliquity, the pressure which it receives from the weight of the body makes it slide, not as a wedge which presses downwards, but as a wedge which frees itself from its surrounding connexions. This view was first promulgated, according to the author, by MM. Hubert and Valerius of Louvain, and it can be readily understood by those who view the pelvis in its real position in the upright posture, that which was determined by the Brothers Weber. The rule is easily remembered by which such position is ascertained. The notch in the acetabulum, to which the ligamentum teres is attached, should be the most dependent spot of the articular ring; the signification of the different bony eminences and ligaments becomes much more comprehensible when this important segment of the skeleton is so held and considered.

When a person falls upon his feet, the ligamentum teres is rendered tense, the head of the femur is slightly separated from the acetabulum; the effort sustained by the pelvis, and which consists in a traction on its ligaments, produces a kind of see-saw movement about the sacro-iliac synchondrosis. The effect of the fall is supported by the round ligaments on one side and the interosseous sacro-iliac ligaments on the other. There is no shock communicated to the frame.

The author informs us (p. 118) that weight is the true flexing power of the head, the flexor muscles, properly so called, being too feeble to act against the extensors. The sterno-mastoid muscles, commonly considered as flexors of the head on the neck, are in truth the extensors; they flex the neck on itself, but they extend the head on the neck (p. 118). But we must remember the flexing movement of the cervical vertebræ, one upon the other, is inconsiderable, in consequence of a depending tongue of bone which leaves the anterior inferior border of each vertebral centrum. The atlas represents a double wedge between the head and the axis (drawing, p. 124), held in position by the anterior and posterior arches; and when from a sudden fall upon the ground the pressure experienced at the point of junction cannot be expanded in the lateral direction, and the surfaces of contact of the condyles with the atlas can be led to form in the antero-posterior direction, an angle more or less marked with the horizon, the intensity of the vertical pressure communicated from the head to the atlas diminishes in proportion to the co-sine of that angle.

Section 9 is devoted to the base of support of the human frame.

> "An attempt has been made to determine with mathematical precision the figure which the base of support should assume in order to offer the maximum of surface. But the calculation has proved vain, and the question cannot receive a mathematical solution. But it has been decided by experience in the position of wrestlers, of fencers, &c." (p. 136.)

We next come to the subject of motion (Section 10): "Movements executed without change of place." After showing how the centre of gravity is affected by every accidental movement, as of the arm, in elevating that member; he proceeds to special considerations on flexion, extension, lateral inclination, and rotation upon its axis, as affecting the trunk. We must refer the reader to the somewhat diffuse but well-written pages upon these points, as they would lead us far beyond the limits of this review.

The remarks upon the lateral or swaying movements of the trunk (p. 157) will be interesting to those who have not previously considered the subject.

> "They do not take place, except in the lower part of the dorsal region and the superior third of the cervical region; they are nearly null at the summit of the dorsal curve. But they are much pronounced at the base of this region (the union of the eleventh and twelfth dorsal vertebræ), as if the superior part of the trunk moved there around a special hinge. They are of considerable extent in the upper half of the cervical region, although perceptible enough in the inferior." (p. 157.)

We do not quote these remarks as containing what is new, but what is interesting; indeed, a yet more perfect appreciation of the movements of the spinal column may be acquired by an attentive study of the direction of the articulating facettes and of surrounding ligaments. We believe that the lateral inclination of the trunk is a movement extending through the whole of the dorsal region.

Chapter III. (Section 1): *On Walking.* The difference between walking and running consists in this: that in the former, however rapid it may be, the body never quits the ground. Running consists in a combination of steps and springs, the body for a time losing contact with the ground. This remark is familiar to all. Dr. Giraud-Teulon divides the act of walking into two stages.

> "In the first, the body and shoulder are inclined to one side, elevating the opposite side of the pelvis as well by spinal and lateral muscles of the trunk, as, according to Gerdy, by the impulsion of the limb, which is about to throw itself in advance; this limb becomes moveable as a pendulum with a solid stem or shank (*un pendule à tige, solide*), moved by a force applied near to its point of suspension. In the second, the will projects this member in front by the action of the flexor muscles of the thigh on the pelvis. *At the same moment* the body is inclined forwards in a manner more or less pronounced (which depends on the proposed length of the step); the member which is posterior and serves as the fixed support, extends itself, opening the angle of the foot on the leg. By this movement, the centre of gravity is pushed in front and laterally towards the suspended member, on which it comes to repose at the moment when this limb arrives in contact with the ground. At the conclusion of this first step, which is the commencement of the step following, the line of propension of the centre of gravity is carried on to the heel of the anterior limb. Then, inasmuch as at this moment the posterior limb is extended and still touches the ground, one sees that just for a very short space of time the two limbs touch the ground simultaneously, the posterior limb forming the hypothenuse of a rectangular triangle, of which the two other sides are the length of the step and the height of the centre of gravity from the ground. Such is the first step. The second step, which commences the series of all the other steps, and which is an exact and complete image of that which precedes it, finds at the moment it commences the centre of gravity already installed upon the limb thrown anterior." (p. 187.)

This description, of which we have only given a part, must be interesting, as explaining the nature and succession of movements which are performed from infancy till old age, but we confess to an inclination to smile at the minuteness of detail, and instinctively thought of the elaborate and graphic account given by Charles Dickens of the attitudes into which the cricketers threw themselves in the celebrated match between All-Muggleton and Dingley Dell. The act of stepping backwards is performed by a series of operations analogous to the preceding, except that it is in the reversed sense, the length of the step with equal efforts being less.

Section 2 gives a long account of the oscillations of the body, &c., at each step, and contains some remarks of no great interest to the reader, but of a controversial character, as regards the discoveries of MM. Weber.

The act of ascending is of course more laborious than walking upon level ground, because at each step the weight of the body has to be raised to a given height; while that of descending, though less laborious, is one demanding a considerable amount of labour, for it has to transform into uniform and regular quickness, the uniformly accelerated quickness which the body, abandoned to itself, receives from its own weight. There are no oscillations of the centre of gravity in this act, and the body, instead of inclining forwards, is thrown backwards.

Chapter IV., On Jumping, Chapter V., On Running, are both well written, and will repay the trouble of perusal.

The remaining six chapters relate respectively to progression in quadrupeds, especially the horse; the mechanism of swimming in fishes; the mechanism of swimming in mammifera; flight; creeping; and climbing. For information on these points we must refer the reader to the work itself, inasmuch as we must now proceed to review the work of MM. Weber, of which it is a remarkable fact, that the contents have been gradually and for many years incorporated into anatomical and physiological works of all languages, without sufficient public acknowledgments to the authors.

We have put this work last in review, although twenty-three years have elapsed since its publication, that the reader may contrast the language and general ideas with those of the preceding work, in which it is so frequently criticised. Part I. treats of general considerations upon the directions which the human body assumes in the acts of walking and running; the limb, which hangs suspended from the pelvis under these circumstances is very moveable, and swings backwards and forwards as a pendulum. But the lower extremities are supports which, by means of the joints admit of considerable elongation and shortening. The leg, when it supports the body in the act of walking in an oblique direction, carries not only a part of its burden as a firm prop, but likewise its whole burden, by the power through which it becomes elongated. It follows, therefore, that in walking, the weight of the body is supported not only by the unyielding structure of the bones, but partly also by muscular power (p. 24).

We find throughout the work of MM. Weber, explanations of the different movements, which, by their terse and graphic language, bear the stamp of original research, and offer a pleasing contrast to the controversial captiousness of many modern works. Speaking of the act of walking, they say:—"At every step which we make in walking, two periods can be distinguished: one, the longer of the two, when the body rests on the ground by one leg; a second, and shorter one, when it rests on both legs." (p. 39.) From page 42 to page 55, we have a succession of sections explaining the differences between slow and rapid progression; they show how there are two modes of progression natural to man, one, the progression of dignity (*den gravitätischen schritt*), the other, the progression of speed (*den Eil schritt*). He enters at length into the questions of vertical oscillation. (p. 53.)

But we would draw especial attention to Part II., 'Anatomical Investigations on the Locomotive Organs.' In order to ascertain the natural curves of the vertebral column, that part was incased in gyspum, in a fresh subject, from which the viscera had been removed, and the whole mass was then sawn through. Table 1, page 92, gives the measurements of the vertebræ and of the intervertebral cartilages, and we there see that vertically the bodies of the second and of the third lumbar vertebræ measure more than the rest, the numbers being in lines, as contrasted with the 1st, 4th, and 5th, 28·15 to 26·30. The intervertebral substance of the 5th lumbar vertebræ is the deepest. The height of the vertebræ and of their intervertebral cartilages in the neck, chest, and loins, is as follows:—

	Height of vertebræ.		Height of intervertebral cartilage.		Total.
Neck	+· 1·3	...	+· 7·8	...	+· 9·1
Chest	—13·3	...	— 9·2	...	—22·5
Loins	+ 6·7	...	+21·1	...	+27·8

Now, by these tables it is seen that the curves of the spinal column in the neck and loins depend chiefly on the form and thickness of the intervertebral cartilages, inasmuch as the vertebral bodies are mostly of equal thickness. The curves in the dorsal region depend chiefly on the wedge-shape of the vertebral bodies, and only in slight degree on the intervening fibrous substances. We know that the ribs require a firm column, in which they may rise and fall in respiration, and the lungs require a certain cavity in which they may expand and contract. But, under circumstances of disease, the dorsal vertebra may become curved either to the right or to the left, and produce corresponding disturbance to the function of either the spinal cord or the thoracic viscera.

In the upright posture the head is balanced on the trunk. The angle of inclination of the pelvis is the same in man as Nägele found it in women—namely, 16°51 for the inferior outlet, and 65° for the superior. In the skeleton this can be easily ascertained by drawing a line vertically through the upper and lower extremity of the vertebral column and the notch in the acetabulum, which latter spot is the most depending part of the pelvis in the upright posture of man. The hip-joint is formed by the round head of the femur and the acetabulum. The former is not held *in situ* by the rim of the articular cavity. In order to do so, the articular excavation must surround the head of the femur in one direction more than 180° of its rounded surface, by which the opening would be much narrowed. The ligamentum teres lies in a depression; the cartilaginous rim clings closely to the joint, which is moistened by the synovial secretion. The capsular membrane, with its thick anterior and upper ligament, and its posterior orbicular zone; the vertical direction of the ligamentum teres; the establishment of the equilibrium of the lower extremity in the hip-joint by means of atmospheric pressure; these are points which have long been received by the better anatomists, and have been taught in the dissecting-rooms; although we think that in many published works the full amount of justice has scarcely been done to the Webers. But Dr. Giraud-Teulon boldly denies some parts of these generally-accepted statements.

"The theoretical opinion of MM. Weber supposes that atmospheric pressure acting through the soft parts (from without inwards), applies them against the bones as a bell upon the plate of an air-pump. This view does not take into account the fundamental fact, that this pressure does not communicate itself to the surface of the skeleton but by a succession of pressures equalising one another, by means of the fluids which fill all vacuities and circulate through our tissues. In such a manner that the osseous layer which finds itself pressed in a certain direction, experiences an equal and opposite pressure from the fluid layer which is adjacent. These physiologists reason as if, in their eyes, the pressure in the osseous substance, at the bottom of the cotyloid cavity and under the synovial membrane, was different from that experienced by the same bone in the interior of the pelvis at some millimetres from the same point. We have no need to protest against this doctrine, it strikes one of itself. It is not contestible that the tension of the fluids under the synovial membrane and at the bottom of the cotyloid cavity is otherwise than identical to the pressure on the other side of the osseous layer; for the capillaries, very numerous, spread equally over the two sides and communicate one with another in a thousand ways."*

We must leave the reader to judge how far this argument supports M. Giraud-Teulon's objection, confessing at the same time our belief in the views of the Webers. We do not see the force or applicability of the above remarks; and we are confident that not one medical man or even *savant* in a thousand would participate in the lively Frenchman's animation at the following clinching proof of the Webers' inaccuracy, which we give in his own words, remarking that we have omitted no context which would have facilitated the matter. He says:

"Here are some remarkable points:—'Inasmuch as the distance of the two points *m* (centre of movement of the body), *m'* the concentrated mass of the leg is of constant size, it results therefrom that if we call *l* the length of the rigid line (*la ligne regide*), *r* the relation of distance of *m m'* to *l* will be constant.' 'Then,' he adds, examining the limb in two successive positions, 'one cannot fail to recognise that the length *l* varies, and thus varies also the relation of its length to *m m'*; *r* then cannot figure as anything constant in the calculation. It can serve at most to determine the constant part of an integral for a definite situation. But let us proceed—that is only a detail. Here is a serious fact.

"'2nd. As the quickness of the point *m* is always horizontal and uniform (*c*=),

$$\frac{d\,x}{d\,u}=c, \text{ and consequently } \frac{d^2\,x}{d\,u^2}=o.$$

"'3rd. As the point *m* always moves horizontally, *y* is constant=*h*.'" (*Op. cit.* p. 231.)

Let us follow out these important simplifications:

"MM. Webers have established the equation of a machine endowed with uniform movement. But whoever will cast a glance upon the mechanism of locomotion in walking, from which the oscillations of the centre of gravity represent themselves at each step, will retain the conviction that this mechanism, of which the active principle is intermittent, cannot belong to any other than the class of periodically uniform movements; that which is essentially different."

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* Principes de Méchanique Animale, p. 227.

Committing this specimen of close reasoning to its due appreciation from the reader, we proceed to the Webers' account of the anatomy of the knee-joint. It is shown how alone in the flexed position of the limb the movement of pronation and supination can take place. In comparing the elbow with the knee-joint, notice is directed to the rolling movement in the latter. The mean degree of flexion and extension at the knee, taken from experiments on four bodies, amounted to 165°. Upon the living subject the same experiments gave a mean result of 144° 8, or 20° less than in the case of the dead body. But then, in the latter, the muscles, which offer some mechanical obstruction to extreme flexion, had been removed. The measurement of the second movement at the knee—namely, pronation and supination, amounted to 39°. The greatest amount of pronation and supination took place when the knee was stretched gradually from 45° to 90°.

We owe to the Webers the proper appreciation of the different ligaments of this articulation. The lateral ligaments are chiefly in action in the extended position of the limb; the crucial in the flexed position. The former hinder the leg being extended beyond the straight line; they prevent pronation and supination when the whole limb is extended; but they become relaxed in flexion. The crucial ligaments are partly extended in flexion as in extension. They compel the condyles of the femur in flexion and extension to *roll* on the articular surface of the tibia, and prevent the parts becoming separated in the continued position of the limb in either way.

The use of the semilunar cartilages appears to be to divide the pressure on the bones, and to break the shock of a sudden fall. But MM. Weber might have added that they are intimately associated with the second movement of the knee, that of pronation and supination; and that they serve as cushions between the bones, the articular surfaces of which cannot, by reason of the peculiar mechanism of the joint, accurately fit. We have seen similar fibro-cartilages in the elbow-joint of birds of strong powers of flight, in whom there is no movement of the radius on the ulna; the two bones of the fore-arm move together, as the tibia and fibula in the leg of man.

The ankle-joint is more complicated than the knee. The astragalus forms two joints between the leg and the foot, as the atlas between the head and the neck. In the first, flexion and extension are performed to 78°; in the second abduction and adduction to 42°. By both together, a rolling movement of 20°. The description of the ligaments by which the different bones are held together, and of the arches of the foot, is such as is now generally incorporated in the usual anatomical works, although the conciseness of language in the book now under review might be, in many instances, as advantageously copied as the material which it contains.

From experiments on the dead subjects, MM. Weber ascertained the muscles concerned in the pronation and supination of the leg. The former movement is effected by the sartorius, semitendinosus, gracilis, and popliteus; the latter by the biceps. He omits the co-operation of the tensor vaginæ femoris with the biceps, a muscle which, as Dr. Palasciano, of Naples, proved, has been greatly misunderstood. Indeed it has no effect in tightening the fascia, the strong tendinous band which leaves its lower border being a strap or stay, which acts from the tibia to the pelvis.

For the abduction and adduction of the foot, nature has given not only a special joint, but likewise special muscles. The tibialis posticus adducts; the peronei abduct the foot; the two are antagonistic. But the extension of the foot in abduction cannot be effected by the peroneus brevis *alone* without great expense of force; nor can the foot be moved readily in the direction of the conjoint action of tibialis posticus and peroneus brevis. The action of the peroneus longus is essential to easy progression. The statement is confirmed by the experience of patients suffering from talipes valgus.

Of the anatomical observations on joints by the two Webers, it is impossible to speak in terms of too high commendation. Their researches filled a gap which was felt by anatomists at the time when their work appeared; and nothing of moment has been added since then to the subject by investigations emanating from other quarters.

Part III. comprises the physiological observations on walking and running. The basis of the theory on this subject will demand measurements, and they must be conducted on a horizontal path, protected from the influence of the wind; both time and distance must be measured. In progression the body is not vertical, but is inclined forward; and the amount of the inclination increases with speed. (p. 230.) The angle formed by the inclination of the body in turning from before backwards is 14°·00. In slow progression it is 4°·9; in quick progression, 13°·8–18°·6. In running, 34°·1–39°·3. The vertical oscillations of the body under these circumstances equal thirty-two millimetres.

The length of a step depends upon the length of the lower extremities; children make shorter steps than a grown person. But we do not extend the legs to the utmost in walking; the greatest length attained in ordinary progression, according to the experiments of the authors, being half the amount to which the limbs are capable of being extended. (p. 239.) In the act of walking, the extended limbs, anterior and posterior, form with the ground a right-angled triangle; the raising of the posterior foot from the ground is effected by a bending of the knee, while the foot and the toes remain extended. (p. 247.)

From the fact of the sole of the foot being raised at each step in walking, it follows that the length of the step is increased by the length of the foot, independently of the angle formed by the two legs.

The pendulum-like movements of the lower extremity are discussed at page 249, and MM. Weber lay down this important law, which Dr. Giraud-Teulon seems so much to doubt, that the greatest rapidity which a man in walking can attain without exhaustion of muscular power, depends on the length of the legs, and the rapidity with which they can swing propelled by their proper weight. The influence of atmospheric pressure is felt in all these movements, the length of a step in the quick walk being one-half the duration of the pendulum-swing of the lower extremity. In the quickest walking, when we touch the ground with the ball of the foot, and not with the heel, this interval of time is somewhat less.

"In running," say MM. Weber, "as contrasted with walking, the body is not continuously supported, but it is raised periodically from the ground, and sweeps forward for a short time quite free in the air." (p. 278.) The elevation of the heel and bending of the knee take place the same as in walking, but the vertical oscillations are less.

MM. Weber next enter upon the Theory of Walking (page 305). The forces which exert influence on that movement are:

A. The extending power.

B. The power of weight—i.e., the weight of the body.

C. The resistance, which the body experiences in walking. The mechanism of walking, &c., consists in a constant change between the swinging and the supporting limb.

The extending power of the lower limbs is just so great and no greater than is necessary to support the middle points of the body always in one and the same horizontal plane. The direction of the extending power always passes through the middle point of the body and the foot-point (*Fusspunkt*) of the supporting limb. The anterior limb rests vertically on the ground at the moment when the posterior leaves it.

The calculations and measurements instituted by the Webers, which are to a great extent dependent one upon the other, occupy a considerable portion of this work. They are scarcely fitted, valuable as they may prove to be, for the general reader, and constitute the reason why the work now before us has been popularized in its anatomical parts and neglected in the rest. It is undoubtedly a most valuable contribution to anatomical and physiological literature, and will long maintain its place unequalled as a book of reference to the special subject on which it treats.

REVIEW VI.

1. *Recherches sur les Vêtements Militaires comme Moyens de Protection contre la Chaleur et le Froid.* Par le Dr. COULIER. ('Journal de la Physiologie,' Jan. 1858.)

An Inquiry into the Dress of the Soldier, as a Means of Protection against Heat and Cold. By Dr. COULIER.

2. *On the Temperature of Man within the Tropics.* By JOHN DAVY, M.D., F.R.S., &c. ('Philosophical Trans.' for 1845–50.)
3. *United States Army Regulations*, 1857. pp. 457.

ON a former occasion we brought under consideration the food of the people; now we propose to offer some remarks on the clothing of the people, believing that dress has not received the attention it deserves, and convinced that properly regulated it contributes materially to the comfort and health of our race, and thus conduces, as asserted by Lord Bacon, to the prolongation of life. In treating the subject, we shall be guided by such lights as science and experience afford, irrespective of fashion, or of that fantastic taste which in its strange workings seems to be directed by caprice rather than by reason, *mutabile et inconstans*, without rule and without law.

Man, as regards the surface of his body, a very few parts excepted, differs from all other animals breathing by lungs and having warm blood, in possessing no natural protection from inclemencies of weather, neither from great heat or great cold, from the tempestuous blast or the pelting shower;—naked he comes into the world, left to his own intellect to supply those requirements which Nature has secured to other animals not so highly gifted with reason, and this with a wonderful and very instructive adaptation, according to the exact quality of climate, or kind of element, whether water, land, or air, they may inhabit. Thus, in the icy waters of the Arctic seas, the aquatic mammalia, the walrus, the whale, the seal, are as it were insulated from the frigid element in which they swim by thick strata of oily matter. The land animals, such as the bear, the fox, the musk ox, are protected from the intensely cold atmosphere they are exposed to by an abundant covering of hair; the birds, the few that spend the winter within or near the same circle, or the many that resort to it during the genial season, are admirably guarded against the cold air by a dress of feathers of an elaborate kind, constructed and arranged so that whilst they retain with as little loss as possible a high temperature, they add but little to the weight, and increase greatly, as regards specific gravity, the buoyancy of their muscular bodies. These are admirable examples of clothing where animal life is endangered by the intensity of external cold. If we change the scene to the tropics, or the bordering regions as remarkable for high temperature, and the powerful influence of the sun, we have examples of a different kind hardly less noteworthy. In the native animals we no longer find the warm furs or the copious shaggy hair of the northern creatures; instead, we see animals with thick skins and sparse hair, such as the pachydermata, or with delicate, perspirable skins and short hair, such as the simiæ, not to mention other species, all in their natural covering equally adapted to the exact climate in which they are found.

How different is the condition of man—how singularly contrasted! He, designed evidently for all climates, and actually inhabiting the most diverse, bears in his outward appearance but slight marks either of the physical impress of climate on him, or of any special qualification for braving with impunity its varied agency. Thus, bringing us back to his nakedness, and how he of all animals is the one, the only one, designed by his Creator to provide himself with clothing, left, as we have said, to the guidance of reason to adapt his dress, according to varying circumstances, to his wants.

Let us glance at the manner in which, apart from fashion, this his reason has been exercised, commencing with extremes, which are often most instructive. The Esquimaux and the Hindoo are good examples, the climate inhabited by each being too tyrannical to allow of the play of fancy in habiliments so as to render dress in its qualities subordinate to usefulness. As in the instance of other animals, so in that of man; the pressing call in the Arctic regions is the preservation of animal warmth; in the tropical and bordering regions the moderation of animal heat, and protection from the sun's rays and the hot winds. The Esquimaux, we find, uses no flowing robes like the Hindoo, and the Hindoo as carefully abstains from close covering garments, and Nature provides each with materials best suited to his wants,—the former, with the skins and warm furs of his own animals; the latter, with light and cool tissues fabricated by textile art from cotton and silk, the produce of his own plains.

If we now extend our view to other countries in which the climate is almost equally imperative, such as that of the Arabian desert, such as that of our own shores, in the one we find the Bedouin in his loose, flowing bernouse, his head well protected by the many folds of his turban from the intense rays of the sun, and his loins well girded and guarded by the many rolls of his sash, equally from the trying vicissitudes of day and night, from the hot blast, and from the chill breeze; on the other, our own coast, we see the hardy fisherman and pilot clad in the short dreadnought jacket, a defence against wind and spray, his legs encased in waterproof, well-oiled boots, and his head helmeted in leather, overlapping behind, protecting the neck from wet and cold. The miner affords another example of man clothing himself according to reason under circumstances affording free scope for its exercise. Descending to a great depth in our mines of copper, and tin, and coal, the temperature increasing with the depth, he has to put forth all his strength in a confined atmosphere, humid and hot, rivalling the sirocco in heat and moisture; he selects for his dress one that is light, moderately loose, and formed entirely of wool, which, however wet it may become from the sweat of his own body, and from the roof-drippings, will prevent his being chilled.

To these examples of rational clothing it would be easy to add instances of the contrary; they are chiefly to be met with in temperate climates, these not exacting ones, like the preceding, and amongst people variously occupied and of vastly different conditions as regards their place in the social scale, and more or less subject to the disturbing influence of that tyrant of all tyrants, fashion It is amongst people such as these that dress in its complications becomes a serious study. Giving our attention to the subject under this point of view, we shall consider not what is in use, but what seems to be wanted, so as to render dress most conducive to health, or in other words, best adapted to the warding off of disease.

Physiologically, as regards man's organic structure, what are the requirements? Are not the following some of the principal? That the feet should be kept cool, not cold (we think this and the following compatible with the well-known Boerhavian aphorism), the head warm, not hot; the neck, if at all, moderately and not closely or tightly covered; the abdomen supported by an elastic girding; the chest free, loosely covered, allowed to have its natural play of action in performing the function of respiration. The manner in which the head in both sexes is provided with hair, flowing, when allowed, over the neck, seems to point to the conclusion which we have come to; and the peculiar nakedness of the feet favours, we think, the inference we have made respecting them. That the head will bear much warmth, and may need to be kept warm, seems to be shown not only by its own high temperature, but also by the healthy action of the brain of those who as a part of their professional costume are under the necessity of wearing wigs; and the adoption of the turban amongst Eastern people, in countries where the sun's rays are intense, and this with marked advantage, may be adduced in confirmation. That coolness of feet is wholesome, we think is proved by the general good health of the peasantry, whether Scotch or Irish, who go barefooted, and also by the feeling experienced whenever the feet are unduly hot, amounting almost to torment. As in the finest machines, in highest order, when in action, there is least friction, most ease of motion, so in life, when the functions are best performed, are in their healthiest state, there is least sensation: and to speak generally, is not that mode of dress the best which preserves the body in the happy neutral condition, a *via media* between heat and cold? We make the remark, moreover, with special application to the extremes, the head and feet, those opposite parts in their requirements as well as position, and to qualify the preceding observation, liable to be questioned as paradoxical, respecting the temperature, as most approved of these parts, adding, that by coolness and warmth, as we use the terms, we would imply merely such a degree of each as is hardly perceptible,—such a degree as the sensuous faculty is hardly conscious of, no more so than the respiratory organs or the heart when their action is most healthy. As to the propriety of leaving the chest unshackled the reason is most obvious, and the affording of support to the loins and lower belly is hardly less matter of reason, remembering the nature of the contents, the yielding quality of the parietes, and the weakness of certain parts of them, parts in which, under muscular exertion, the danger is ever impending of rupture. A favourite position of the arms is the folding them over the abdomen, a position this, in itself, affording warmth and support; and amongst a people supplied with little clothing, the attitude, judging from our own experience, in Eastern countries is most common, especially in the cool of the early morning.

These premises granted, disapproval follows of all tight-lacing, confining the chest, so often practised by those of the female sex, more regardful of an imaginary beauty of form than of health, without which there is no satisfactory beauty; and also of that excessive attention to the feet to keep them dry by the use of clogs, water-tight and heating, and confining the natural moisture of perspiration, when taking exercise deserving of the name.

Other circumstances require consideration, and of these the most important perhaps, in connexion with dress, are the different periods of life, the different seasons, or whether taking exercise or repose.

Physiologically viewed, as regards animal heat, there is a difference observable comparing infancy with old age, and these two periods of feeble life with that of mature age, one of established strength and power of endurance. Both extremes, it would appear, are most easily chilled, or in other words lose their normal temperature soonest when exposed to cooling influences, unprotected, and are liable to suffer in consequence. A distinguished physiologist sums up the results of his researches on this point, in the conclusion, "that the power of producing heat in warm-blooded animals is at its minimum at birth, and increases successively until adult age."* Admitting this, how much is to be deprecated the too frequent practice of clothing children scantily, and especially the leaving exposed the upper and lower parts of the trunk. How contrary to reason is it that those parts which in the firm adult are covered and protected, in the child should be left defenceless! How often, how very often, may fatal diseases have been owing to such want of judgment, the parts alluded to being in early life very susceptible of diseased action—an action having its issue in some of the most formidable complaints of childhood. The so called, and most improperly called, hardening system, is full of danger, and mothers cannot be too strongly cautioned against it. How well very young children bear warmth is shown by their flourishing health, even within the tropics, provided the locality be free from malaria. Even the farmer, the breeder of stock, is become sensible of the evils of the system, so improperly named, and of the advantages of the opposite procedure in the instance of young animals. The approximation of old age to infancy in susceptibility of cold, and the need of warm clothing, are so well known as not to require proof. The warm bed, the fire-side, the padded vests, are the requirements of the old, and are most willingly, as it were a right, given to and adopted by them. Even with advancing age, it should be kept in mind that susceptibility of cold, or risk of suffering from a lowering of temperature, is an established fact. This is demonstrated by the mortality tables of the Registrar-General. We quote the words of Dr. Farr; he says:

> "The power of cold on life varies according to definite laws; the general result being, that the danger, after thirty, of dying of cold is doubled every nine years of age; for out of the same numbers living, to one death by cold at the age of thirty, there are two at thirty-nine, four at forty-eight, eight at fifty-seven, sixteen at sixty-six, thirty-two at seventy-five, and sixty-four at eighty-four; a series which represents the relative mortality by cold at these respective ages, during five weeks, amongst two millions and a half of people."

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* Dr. W. F. Edwards on the Influence of the Physical Agents on Life.

Of the demands on dress connected with change of season, little need be said, the propriety of some alteration is so obvious to the senses. If there be any difficulty in regulating the quantity and quality of clothing, according to atmospheric temperature, it is most experienced in spring and autumn, in which seasons, in our climate, a hot day is not unfrequently followed by a cold night, and changes of a trying kind of many degrees of the thermometer, are not uncommon between sunrise and sunset. Prudence then dictates, especially in the instances of the young and delicate, the avoidance of any sudden and great alteration of dress, and the adoption of a graduated one, inclining rather to excess than deficiency, the one risking less than the other. In the East, where the natives are so attentive to clothing for warmth-sake, duly valuing its importance, it is even amusing to see how many changes of dress are sometimes made by the sensitive Oriental at these seasons in the course of a few hours, and at all seasons when taking, or resting from, active exercise.

As to exercise and repose, in relation to clothing, much may be said, for the common notions on the subject are not such as science, we think, authorizes. How general is the practice, in preparing to take exercise, to put on an additional garment, and that sometimes a heavy one, and on returning, after being warmed or heated by the exercise, to throw off that garment. The contrary, in most cases, would be more judicious, as is practised in the East, inasmuch as muscular exertion (we speak, of course, of active exercise) accelerates the heart's action, accelerates respiration, and increases the temperature of the body. We have found it, tested by a thermometer placed under the tongue, to rise rather more than one degree after brisk walking or riding, to such an amount as to occasion perspiration; and this with a quickening of pulse of from twelve to forty beats in the minute, and of respirations of from one to seven in the same space of time. For detailed experiments illustrating the effect of exercise on the temperature and on the heart's action, we would refer to two of the papers given in the heading of this article.[*] In the same papers will be found results, proving also that with rest the temperature falls, and generally the lowering is the greatest, the higher the temperature had previously been, indicating, we think, clearly the propriety of rather adding to our garments than the diminishing of their number on taking repose. And here we would offer a word of caution in connexion with the difference of effect of active and passive exercise, and concerning the clothing suitable. As we advocate a lighter dress in the one instance, so we would a heavier, a warmer in the other, inasmuch as in the act of taking what we call passive exercise, such as that obtained in slow walking or in slow riding, or in a carriage drive,[†] the tendency is to a lowering of the temperature, and, at the same time, of the heart's action and the lungs. We may refer to the paper just quoted, for experimental proof of this statement.

Before advancing further, it may be well to consider briefly the qualities of the more important ingredients of textile fabrics used in dress—such as silk, wool, cotton, flax. Some idea of their importance in the way of clothing may be formed from the amount of each imported; which, in 1847, was as follows:—cotton, 869,335,024 lbs.; flax, 188,848,592 lbs.; raw silk, 12,077,931 lbs.; wool, 127,390,875 lbs.

What are the properties of each of these materials, irrespective of appearance, which should induce us to prefer one to the other? Answering this question in the most general manner, we have little hesitation in expressing our opinion that the cardinal property is low conducting power, or that quality which best preserves animal heat in a cold climate, and best protects against external heat in a hot climate; and which is tolerably tested by the sense of touch, the worst conductors feeling least cold when a warm hand is applied to them; the best conductors feeling hottest when of a temperature exceeding blood heat, or in excess of that of the part brought in contact with them. Hair, it would appear, is one of the worst conductors, and those tissues or textures in which hair most abounds, mechanically intermixed and adhering, are, *cæteris paribus*, in the same proportion remarkable for their low-conducting power. We cannot adduce better instances for comparison than the materials we have mentioned, those entering most into the composition of our ordinary clothing; especially if considered in relation to their mechanical condition of coarseness and compactness, or fineness and looseness of texture. From the researches of Count Rumford, to whom we are most indebted for information on this subject, it would appear that equal quantities by weight of the materials we have been treating of lose the same amount of heat (60°) in the following number of seconds:

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[*] On the Temperature of Man in England and within the Tropics: Philosophical Transactions, 1845–1850.

[†] Of the effect of exercise in railway travelling we cannot speak so decisively as above, but, we are diposed to believe that in most instances it is the contrary of sedative, and more so in second and third-class carriages than in first class; in the latter the motion being less felt. We believe, from the few observations we have made, that the pulse is commonly accelerated, and this even in carriages of the first class.

Raw silk in	1284
Fine ravellings of taffety	1169
Sewing silk cut into lengths	917
Sewing silk wound round the bulb of a thermometer	904
Sheep's wool	1118
Woollen thread wound round the bulb of a thermometer	934
Cotton wool	1046
Cotton thread wound round the bulb of a thermometer	852
Fine lint	1032
Linen thread wound round the bulb of a thermometer	878
Linen cloth wrapped round the bulb of a thermometer	783

To these results we add the following, from the same author:*—Hare's fur, 1305; eider down, 1305.

The combination of lightness of tissue and fineness of fibre with warmth or bad conducting power, as shown by the preceding results, is surely a beautiful provision of Nature, and deserving of being kept in mind, whether in contriving dress with the intent to preserve warmth or to exclude heat, or it may be, even to prevent wetting by the penetration of rain,—adhering hair repelling, or to speak perhaps more correctly, supporting the drops of water, as we witness in the leaves of many plants possessed of microscopical papillæ, and in the down of birds and the webs of spiders, possessed of exquisite fineness of fibre.†

Other qualities there are deserving of attention—such as hygrometrical power, or power of absorbing water from moist air; radiation; degree of inflammability; fitness for washing; passing over others of minor importance.

Respecting the first mentioned property, we shall give the results of Count Rumford's experiments, made on most of the substances already named, premising that each was weighed after having been dried twenty-four hours in a hot room (No. 1 in the following table); again, after an exposure of forty-eight hours in a cold uninhabited room (No. 2); again, after an exposure of seventy-two hours in a damp cellar (No. 3).

		No. 1. Parts.	No. 2. Parts.	No. 3. Parts.
Sheep's wool		1000	1084	1168
Beaver's fur		1000	1072	1125
Eider down		1000	1067	1112
Silk	Raw, single thread	1000	1057	1107
	Ravellings of white taffety	1000	1054	1103
Linen	Fine lint	1000	1046	1102
	Ravellings of fine linen	1000	1044	1082
Cotton wool		1000	1043	1089
Silver wire, very fine gilt, the ravellings of gold lace		1000	1000	1000‡

The last result with the metal drawn out in ravellings is remarkably contrasted with the results preceding it; showing that the metal (and it is true of all metals) has not the property, which the other substances possess in different degrees, of attracting moisture from the atmosphere. Count Rumford deduces from the higher degree of attraction exhibited by wool, an argument in favour of the use of flannel, which for health's sake he advocates strongly, and on the ground of its promoting, by its attractive force, insensible perspiration, and by a ready evaporation from its flocculent surface keeping the skin dry and warm. The apparent dampness of linen to the touch, and its coldness when the body is perspiring, he refers to the greater ease with which it parts with its hygrometrical water; we would rather attribute it to its superior conducting power, to its inferior elasticity and smoother surface, one in which air is less entangled. The circumstance that flannel, in the act of absorbing hygrometrical water from the skin, evolves heat in a higher degree than cotton or linen, may in part be concerned in the explanation. This property has been well insisted on by Dr. Coulier, in his able remarks on the clothing of the French army. He is of opinion, and we adopt it as rational and in accordance with our own results, that the warmth thus evolved may even contribute to neutralize the cooling effect produced by evaporation; the warming effect in contact with the skin, the cooling effect external. This view accords with the sensation of warmth imparted by flannel, and with its being a positive effect rather than a mere negative one.

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* Philosophical Transactions for 1792.

† See Proceedings of the Royal Society, vol. viii. p. 520, for some ingenious remarks on this property, by Dr. Buist.

‡ Philosophical Transactions for 1787.

On the other properties mentioned we must be brief. That the quality of radiation is not an indifferent one in relation to dress cannot be questioned. As a rule, it is well known that the worst conductors of heat are the best radiators. Were it otherwise the uses of both classes would be diminished: the polished silver teapot would deservedly fall into discredit, and the flannel dress might even become oppressively heating. Those who have not made the trial would probably hardly believe that a polished metallic vessel, filled with hot water, will be accelerated in cooling by giving it a covering of flannel. This radiating power of flannel, in addition to its bad conducting quality and power of absorbing hygrometrical moisture, with evolution of heat, renders it, we may remark, admirably fitted as an article of clothing, not paradoxically, as at the same time warming and cooling, but as a moderator and regulator of temperature.

The different degrees of inflammability of the common materials of dress are easily shown by one or two simple experiments—we allude to silk, wool, flax, and cotton. If a slip of each, in a woven state, is placed on a support of platinum foil and held over the flame of a candle, the silk and wool will become charred without inflaming, whilst the cotton and linen will take fire and consume with flame; but of the two latter, the cotton more readily and rapidly than the linen. Further, if slips of each be wound round a copper wire of one-fiftieth of an inch in diameter (we particularize because it is an experiment) and used as a taper, the cotton, brought to the lighted candle, will inflame readily, and held perpendicularly will burn to the bottom, leaving only the trace of a white ash; the linen will do the same, but slower, leaving a similar ash; but not so the woollen and silk—these hardly break into flame; the flame, when it occurs, lasts only for a moment, and leaves a coal, which burns with difficulty and soon goes out. The application of such results as these to dress, especially to women's dress, entailing so much danger of person from fire, are so obvious as to require no comment. It would be well were the subject brought forward and illustrated by experiments before popular audiences. The demonstration through the eye would probably be more impressive than any words, even if uttered by the most persuasive of orators.

As to fitness of the materials for washing, it may be sufficient to remark, that in point of degree there is but little difference between them, with the exception of wool, which from the structure of its fibre, not smooth like that of silk, cotton, and flax, but having minute processes or offshoots, is subject to entanglement or felting, giving rise to a shrinking of superficies with increase of thickness; but which fortunately can, to a considerable extent, be prevented by peculiar modes of washing and drying well known to the skilful laundry-maid. Here we would offer a passing remark on a laundry practice much to be deprecated—the use and abuse of starch, sacrificing thereby a great amount of valuable food, and at the same time by its hardening, stiffening effect when applied to articles of dress, rendering them colder and less agreeable to the feel. Even our towels and pocket handkerchiefs cannot escape the addition, as may be proved by testing them with iodine.

Another property, the last we shall advert to, as influencing the materials of dress is colour. Its influence is most strikingly witnessed in the heating effects of the sun's rays. From the experiments which have been made, all of them of a simple and conclusive kind, it would appear that, *cæteris paribus*, dark-coloured bodies become soonest and most heated on exposure to the sun, varying with the degree of intensity of colour, the extremes of the scale being black and white. It also appears to be proved that when the sun's rays are absorbed by a dark surface, the heat evolved ceases to be radiant in a great measure, and loses, consequently, its peculiar powers, one of which is that of exciting inflammation, as witnessed in sun-burn. These are facts applicable to dress, especially in a hot climate. They tend to show that, for protection's sake, the two colours should be used at the same time, white in the outer garments exposed to the sun's rays, black in the inner clothing to prevent these rays from acting injuriously on the skin. In the African, with a black skin, there is a strong taste for white clothing: in the instance of the Arab horse, of purest breed, the hair is white, the skin black; and, universally, as regards the effects of the sun's rays, whilst they conduce to the bleaching of the hair, they equally contribute to the darkening of the skin. This is well witnessed in the cotter's child left to his own enjoyment in fine weather, *sub dio*, bare-headed and bare-footed, in the darkening to nut-brown of his complexion, and the lightening, often to whiteness, of his hair. We remember how we once gained a little credit with a friend, peculiarly sensitive of the sun's rays, and suffering from them, by suggesting an umbrella constructed on the above principle. He had the courage to spread his protector with its contrast of colours—white outside, black inside—and he assured us with the most satisfactory result as to his comfort. Confirmation is readily obtained experimentally. We shall give an example: Four vials, of the same form and size, were charged with a prepared mixture consisting of weak mucilage and a little nitrate of silver and exposed to the sun's rays; one (No. 1) was left uncovered; one (No. 2) was covered with white silk; one (No. 3) with black silk; one (No. 4) with white silk over black silk; and one (No. 5) with tinfoil. Examined after three hours, the fluid in No. 1 had become almost black, its temperature 75°; No. 2, dark brown, temperature 68°; No. 3, only just perceptibly coloured, temperature 75°; No. 4, just perceptibly coloured, temperature 69°; No. 5, just perceptibly coloured (the foil had some minute holes, allowing the passage of some rays), its temperature was 71°. The air at the time was 61°; water in a vial, without the addition of mucilage and nitrate of silver, was 64°.

Amongst the conditions we have passed over as influencing a choice of materials of dress, are durability and market price; important as these are, they hardly here require comment—the one being fluctuating, and the other depending much on the quality of the fabricated tissue, whether made of coarse or fine thread; and neither of them *per se* having any bearing on the question of health.

We have given some examples of rational clothing in the instances of certain people and classes left to their own free will and judgment. We shall now advert to an example of an opposite kind, an instance in which neither the free will nor the judgment of the individuals concerned are anywise consulted. We allude to the dress of our troops, their uniform—a term very appropriate indeed! and that both for commendation and censure; the former, inasmuch as a certain uniformity of dress is necessary for men acting together; the latter, inasmuch as the dress is too little varied according to the exigencies of circumstances, especially of climate. How irrational is it that regiments serving at home and in the West Indies should be obliged to wear the same description of clothing—the same cloth coat, the same cloth trowsers, of the same shape in both countries, equally tight, and the former equally buttoned up. The dress contrived in the one country and tolerable there, in the other becomes almost intolerable. The consequences are serious: the efficiency of the soldier is diminished and his health endangered. On the march, within the tropics, and even on ordinary parade, he suffers unduly from heat. In quarters, so soon as released from duty, he too often suddenly throws off his warm dress, and exposes his surface, bathed in perspiration, to the wind in quest of coolness, and thereby not unfrequently contracts a mortal disease.

How much is this to be regretted! How great is the desideratum that the subject should have careful attention—the attention of men who, by their science and experience, would be competent to do it justice, dealing with it on general principles, and taking into account all the circumstances which should have consideration. We had hoped that the dress of the troops would have been fully inquired into by the Commission on the Sanitary State of the Army; but we have been disappointed. Referring to the voluminous Blue Book, consisting of the report, and the evidence on which it is founded, we find that the inquiry in this matter has been scanty in the extreme, and restricted to a very few particulars, such as the shirt, whether it should be of flannel or cotton; the stock, what should be its material; the head-covering, which of those at present in use should have the preference.

As we have recommended careful inquiry, and knowing how difficult the subject is in its wide bearings, we shall not presume, nor will our limits permit us, to discuss it in detail. We shall do little more than propose a few queries and offer a few suggestions, and these propounded for troops in the field engaged in actual warfare, for which, his special service, the soldier should always be prepared, keeping in mind, and if possible impressing on his mind, that noble saying of Lord Bacon, "That the offices [the duties] of life are preferable to life"—a saying applicable to dress as well as to other things, whether diet or exercise, conducive in training to make the complete soldier.

1st, Let us speak of the material: Ought it not to be entirely woollen—coat, trowsers, shirt, even to the lining of the former, if they have any lining, as may be proper in a cold climate? and as regards climate, adapting the warmth of the material and the degree of looseness of dress (always to be compatible with the free action of the limbs), as much as possible, to the degree of temperature? And ought not the several articles, of approved good quality, to be provided free of charge by the Government? In the United States army we perceive that for a term of five years eight coats are the allowance, one great coat, thirteen trowsers, fifteen flannel shirts, eleven drawers, twenty booties—these all served out periodically.*

2ndly, As to head-dress or covering: Ought it not, for the two extremes of climate, to be specially considered, so as to afford as much security as possible from the effects of extreme cold, in a Canadian winter, and of extreme heat and exposure to the sun in a tropical climate? Is not a peak objectionable; ought not the degree of light acting on the retina to be regulated by the eyelids? The Highlander does without a peak to his bonnet, the Asiatic without it to his turban. Ought not the hair to afford sufficient protection to the neck? The Spartan troops wore their hair long, and took a pride in it. In the construction of the cap or helmet, it may be worthy of consideration whether there might not be a thin polished metallic external covering with advantage, such a covering having the property of casting off entirely the sun's rays, or if perforated for ventilation, the greater proportion of them, as shown in experiment No. 5, already described.†

3dly, As to the neck, ought not the stock to be banished altogether, and the part to be protected by the beard in front and the pendant hair behind? Amongst Oriental people the neck has no other protection; and certainly no part needs freedom from restraint more, both on account of the movements constantly required of it, and the important vessels and nerves which, seated in its yielding structure, are liable to suffer or occasion suffering, even death, from pressure. And ought not the moustache as well as the beard be worn? It is a defence to the lips on exposure to the sun, a protection to the nostrils from dust and insects, and as a respirator may afford some security from malaria and from chilling and scorching blasts.

4. As to the loins and abdomen, should they not have the support of a belt or sash? A world-wide experience is in favor of this, and the warmer the climate, generally the more remarkably has the custom of girding the loins been adopted. The first lesson we had on the subject was on a voyage in the Mediterranean long ago, when we had the pleasure of an agreeable companion in a merchant on his way to Egypt; he assured us that in his periodical visits to that country, he, after his first, always had recourse to

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* Regulations for the United States Army, p. 184. 1857.

† In connexion with this subject the reader's attention is specially directed to Mr. Julius Jeffrey's work, 'The British Army in India,' of which an analysis was given in the article entitled 'Our Indian Army,' in the January number of the present year.—Ed.]

the sash, and chiefly as a defense from bowel complaints. In England, he said he could do without it, but not in Egypt, with impunity. According to our experience, the best support is afforded by a narrow knit elastic worsted sash not exceeding two inches in width, and in length not less than seven yards. Such a support we believe may have more uses than one: it may afford security against lumbago, it may aid in preventing accumulation in the large intestines and undue distension of the gall-bladder and urinary bladder, and so conduce to the prevention of constipation, and may even check the formations of calculi, biliary and urinary. It is very remarkable, the precautions taken in the East to the girding of the loins, and this by the people of highest and earliest civilization; and we witness the same in the dress of the Roman soldier, and our own Highlanders.

5. As to the feet, might not the sandal in a hot climate and the mocassin in a cold climate (from necessity already adopted by our troops in Canada), be substituted with advantage for the laced shoe or boot? And might not the men with advantage be taught to make and mend them? Those who have had any experience in a protracted campaign, especially out of Europe, know the evil consequences to an army from the failure of boots or shoes; and even when these are not deficient, as at home, how difficult it is to have them provided of serviceable leather, and so to fit as not to occasion footsore. The evidence before the Sanitary Commission is strong on this point, as is also the recent Weedon inquiry. If shoes or boots are to be used, they ought, we are of opinion, to be made, if possible, of the leather of the country, such leather being most suitable to the climate, the leather of a cool or cold climate being close and compact in its grain, of a hot climate loose and porous; the boots should be so made as to exclude sand and gravel. Till the Turkish troops, with their change of dress from the Asiatic to the European form, adopted boots, corns and the other deformities of feet, the effect of undue and irregular pressure, were unknown to them.

6. As regards the skin—the surface of the body generally—might not oil be applied to it with advantage, as an aid to dress; in wet weather being conducive to dryness, in cold weather to warmth, and in hot weather and on exposure to parching winds checking excessive perspiration and too rapid cooling by evaporation? Many facts might be adduced in proof of the salutary effects of this use of oil. We see how aquatic birds, guided by an unerring instinct, have recourse to it, pruning their feathers with the oil with which nature supplies them, a special gland for the purpose being provided for its secretion; we see, too, how the shepherd, taught by experience its protecting power, lubricates the fleeces of his flock with oil or fatty matter. The birds thus prepared are proof against wet, even when under water; and the flocks can endure the mountain blast, with all the inclemencies of its winter climate, with comparative impunity. In corroboration, we might refer to the great use of oil amongst the ancients, and its use at the present time amongst many tribes of people much exposed to the weather, such as the North American Indians, who carefully prepare and preserve the marrow of the animals they kill in the chase, for the purpose of anointing their skin before starting on their hunting excursions. We might support these views by authority, and that without insisting on the statement of Johannes de Temporibus, reported to have lived 300 years, "that he owed his longevity to the use of honey within and oil without." Lord Bacon, in his peculiar language, says, "that anointing with oil contributes to health in the winter, by excluding cold, and in summer by keeping in the spirits, and preventing their dissolution, as also by fencing against the force of the air, which is the most predatory."

We have said that these suggestions are offered for troops in the field; on home service, and on garrison duty, whether at home or abroad, the dress and its materials might be modified. Cotton for under-clothing might be substituted for flannel, being recommended by its greater cheapness and durability. During a time of peace we have compared the health of two regiments on the same station, and that in the Mediterranean, the one wearing flannel shirts, the other cotton, and we found no material difference. It is in an active campaign that the constitution of man is most tried, it is then that his health and life are endangered, and it is then that all possible precautions, hithetro much neglected, ought to be taken to preserve his health and efficiency. It should be kept in mind that every hard exhausting march is productive of a certain amount of sickness and mortality, every privation of food is productive of the same, and the same from any deficiency of clothing or cover, and with a degree of certainty almost admitting of precise calculation.

On a previous occasion we compared the dietary of our army and navy, so much to the disadvantage of the former. The same result would follow from a comparison of their dress; the one so little judicious, so little in conformity with the requirements of the service and the preservation of the health of the men; the other, so rational and agreeable to the wearer, and so suitable to his duties. We are essentially a naval not a military people; and apart from the national taste, as displayed in its "efflorescence" in our yacht-loving gentry and the popularity of the naval service, have we not proof of it, in the careful attention given to our navy, and the sound judgment and great ability displayed in the regulating of it, and that for a continuance, contrasted with the little serious attention shown to the army and that little by fits and starts on emergencies, and the caprice, rather than the judgment, by which it has too often been conducted? Our scanty literature of the subject—that of army organization and administration—may be noticed in additional proof; the only work we have to refer to, and that quite recent and limited in its scope, and hardly yet an authority, being Mr. Fonblanque's of the Commissariat Department.

Dress, in relation to the treatment of disease, has partaken too much of the common neglect of dress. How little attention has it received? How much more has it deserved? How much, probably, might be effected by means of it, rationally regulated? We know of some of the evils resulting from mistakes on the subject, and of the good produced by the correction of those errors, as in the instance of the exanthemata and of fevers in the removal of a heap of bed clothes from those oppressed by heat.

This was an important correction. Another abuse still exists of a like kind, and hardly less deserving of censure: we allude to the popular error of heating the chest by unduly warm clothing, in persons labouring under pulmonary disease, especially phthisis. How often, with a hot skin and rapid pulse, do we find such patients not merely wearing flannel as an under vest, to which, if light, we would not object, but also in addition wrapped in fleecy hosiery, or covered with hare-skin, or some "comforter," as it is called, the work of kind female hands. Such adjuncts, specially intended to guard the weak organs, cannot but have an injurious effect in keeping up an undue heat, and so accumulating the diseased action. And is not the keeping the chest cool in tuberculosis in harmony with the beneficial effects of mountain air, the reverse of mild or warm, in cases of this disease, now so well established? We have a very limited experience of the beneficial effects of walking in a dress of flannel so as to promote perspiration, in inveterate chronic rheumatism, changing the dress in a warm room for a dry one immediately after the exercise.[*] Flannel and gout are old associates; and severe burns, as regards their treatment, are coupled in idea with cotton-wool. Judiciously applied, we have proof that they are both useful. They may act probably by regulating the temperature of the part, to the conducing of a healthy circulation in the affected tissues, and through the hygrometrical and radiating powers may even be capable of producing a cooling effect.

In the warding off of disease, how great may be the influence of dress! There are facts tending to prove that the body, well wrapped in flannel or clad in any warm clothing, so as to prevent a check of perspiration and chill from the night air, is capable of resisting malaria, that kind productive of ague and remittent fever, and in the most malarious localities, such as the Pontine Marshes. We can speak from personal experience of the soothing effect, not merely of discarding a night-cap, but of placing the head wet on the pillow, after ablution, in a hot climate, thereby promoting sleep, without risk of catching cold.[†] We have heard of a surgeon, now at the head of his profession, and approaching the octogenarian period, remarkable for his health and juvenility of appearance, whose under dress throughout has been silk. How much of his good health he has owed to this peculiarity of dress, who can say? But we must infer that he thinks well of its influence; and, if an electrical atmosphere can conduce to health, the silk, in contact with fabrics of other kinds, may have some virtue. These are a few desultory remarks on a subject which we could wish to see treated more *in extenso*, believing that it deserves it, and that it would not prove ungrateful for the attention.

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[*] See Mr. Chenevin's case, as recorded in an early volume of the Medico-Chirurgical Transactions.

[†] As regards temperature, attention to the feet, and in all climates, is hardly less necessary than attention to the head. Fortunately, a foot-bath of some kind is easily attainable; if heated and foot-sore after a long march or walk, how refreshing is such a bath of tepid or cool water in a hot climate, or during our summer heat; if in a cold climate and after passive exercise, on horseback or in a carriage, how grateful is one of hot water! And it is hardly less so, if not taking any exercise, as when at night-time the feet may be cold before retiring to rest, then warmth thus imparted has a calming effect, promotes sleep almost as much as the contrary treatment of the head, as described above, does in a hot climate. The effect of the bath, whether cooling or warming, is not confined to the feet, through the circulation, in the returning blood, the system becomes a partaker of it. How much is it to be desired that we followed the Orientals in the habit of foot-washing, a habit so sadly neglected by the English people of the working class. We have been tempted to point to this, though not strictly belonging to the subject before us; and we would add, speaking from some experience, that even the nightly bathing of the feet in hot water has no relaxing, debilitating tendency, but believe even the contrary, as conducive to their healthy condition.

On the dress of the people at large, in its minute details, we shall not at present enter; our limited space forbids it, enforced further by the persuasion that in a matter in which fashion is so much concerned, the pages of our Review are not the fit medium for making any salutary impression on the public mind, especially of the women kind: we have ennunciated what we believe to be the principles of rational dress, and willingly resign to 'Punch' and the 'Times' the reformatory pen; if we may venture to express a hope, it is, that the one will not spare its jest, nor the other its fulmination, so long as the power of either of them continues to have effect, that is, so long as the one can raise a smile or excite a laugh, or the other produce a serious thought or a rational conviction.

Review VII.

1. *Lehrbuch der Physiologie für Akademische Vorlesungen und zum Selbst-studium.* Von D. Otto Funke, Professor an der Universität Leipzig. Zweite Auflage. Two Volumes.—*Leipzig*, 1850.
Text Book of Physiology for Academical Lectures and for Private Study. By Professor Funke. Second Edition.
2. *Traité de Physiologie.* Par Dr. F. A. Longet.—*Paris*, 1850–57.
Treatise on Physiology. By Dr. Longet. Two Volumes.

Philosophy has undergone an important change. Science has passed through a mighty revolution. The fancies of alchemy have been replaced by the facts of chemistry. The transcendental views of astrology by the sober calculations of astronomy; and now also we find a peculiar spirit pervading the physiology of the present day. The cord that has so long bound it to the metaphysical dreams of the middle ages is fast being gnawn asunder. Men doubt, where they once blindly credited; and inquire into what they formerly took for granted. They cease to believe that causes of which they are ignorant, originate in a supernatural agency, and no longer fear to affirm that the transformation of dead matter into living tissue is dependent upon something else than the spectre called "vital action." True Physiology, by which term we mean physiology as an exact science, does not yet exist. But nevertheless no one can shut his eyes to the fact that an important change is taking place in this department of knowledge; and that just in the same way as the astronomy of the nineteenth rose out of the astrology of the eighteenth century, so will true physiology rise, sphinx-like, from the ashes of the pseudo-physiology which still encumbers us. The new school that has risen up in Germany, in France, and in England, has sent forth pupils who have thrown off the yoke of credulity, and who no longer fear to be branded as sceptics, since they have learned to regard the brand as the Victoria Cross of the true science they are fighting to establish.

The members of this, the chemico-physical school, are often tauntingly asked, what practical good their researches do? They are told that, in spite of all their boasted knowledge, medicine is still left in the "dark ages." That there are still but two diseases which she can cure—itch and ague. That she has still but two remedies—sulphur and quinine. Now is this true or is this false? We do not hesitate to affirm that the injustice of the reproach is only equalled by the ignorance of those who make it. It is humiliating to have to refute such an assertion; nevertheless we shall bestow upon it a passing remark.

The chemico-physical school has taught us many important facts. From its members we have derived a clearer, and we might say simpler, conception of what life is than we previously possessed. They have shown us, for example, that absorption follows the ordinary physical law of diosmose, that the crystalline lens acts according to the law of optics, that the limbs move in obedience to mechanics, and that digestion, respiration, &c., are performed on purely chemical and physical principles. In a word, they have pointed out to us that the phenomena of life are the effects of a combination of the natural laws which govern the universe at large. It seems to us a strange thing that there should still be found individuals in the ranks of the medical profession who regard physiology as a science apart from medicine, and talk about a course of lectures upon it as if they were lectures upon comparative anatomy and zoology. This arises from a total misconception of what physiology is. For any one at all acquainted with physiology must be fully aware of the fact, that this branch of study and rational medicine are inseparable. The former indeed occupies, in relation to the latter, a much more intimate position than anatomy does to surgery. A surgeon may successfully amputate a limb with but an imperfect knowledge of the position of the arteries, for, during the performance of the operation, his eyes will tell him where to place the ligatures. A physician, however, cannot rationally treat the disordered function of any organ without a previous knowledge of its healthy action. Nay, more, in order to be a good physician, he must not only know the normal function of the organ in question, but he must also be fully acquainted with the variety of influences which the functions of the various organs exert upon each other, both in health and in disease. If the practioner be ignorant of these when he comes to the bedside of his patient, he will find it impossible to acquire the requisite information while there; and his ignorance will lead to bewilderment in diagnosis, absurdity in theory, empiricism in treatment.

We can see no line of demarcation between physiology and pathology. The same laws which act in health act also in disease, and it is only on account of a change in their rhythm, or in their force, that the result of their action is different. Hence it is that a knowledge of these laws is essential to the physican, and hence also it is that physiology, which teaches them, is synonymous with rational medicine. This is no new view of the case; for in one of the oldest and largest of our British medical schools (Edinburgh), the course of lectures on physiology is, and has long been, termed "Lectures on the Institutes of Medicine." This being true, one feels inclined to ask, how it has happened that the gentlemen who lately framed the new code of regulations at the College of Surgeons, should have thought that physiology—the principles of medicine—could be properly taught in forty-eight lessons? Did they imagine that the clinical physician was to teach his students physiology at the bed-side? Surely not. One might as soon expect the hospital surgeon to demonstrate on the limb of a patient the origin and insertion of the various muscles and ligaments, as expect the clinical physician to explain the course of the circulation, the mechanism of respiration, or the chemistry of digestion in the wards of an hospital. Now, as it cannot be taught there, and it is impossible to teach it in forty-eight lessons, it is perfectly clear that it will never be taught at all. The thoroughly practical men, then, which our schools are expected to produce under the new regulations, will approach much nearer than they have hitherto done to the skilful old woman, the Esculapius of the village. The practice of the one being, like that of the other, guided by no principles but those derived from bedside experience.

Some, we know, are bold enough to assert that it is quite unnecessary for a medical man to be acquainted with the normal functions—what he has to treat being the diseased; and that if he understands the derangements of an organ he knows enough. To this class of thinkers we would address the question, "When your watch is out of order, do you take it to a workman who neither understands the construction of its different parts, nor the offices they perform?" If you do not, why do you ask people to take the human machine, when it is out of repair, to an individual who does not comprehend the construction and function of its various parts? You will tell us, perhaps, that you never asked people to do any such thing; what you wish being, for them to go to men who have learned the functions and diseases of organs at the bedside instead of in the lecture room. Our answer to this is, that such a thing is utterly impossible. Before one can know the diseased, he must first of necessity understand the healthy functions; and, no matter however talented and diligent he may be, he can never understand the healthy functions by simply regarding them in a diseased condition. Dr. Marshall Hall spoke well when he said, that "to be a good physician a man must be a good physiologist."

The flexibility of our pen has led us to make remarks other than we had intended when we at first took it in hand. But to make up for the digression, we shall at once enter upon the consideration of the books before us.

To many of our readers the name of 'Funke's Physiology' may sound new. The work is not so new, however, as it at first sight appears; for on closer inspection it turns out to be an old book in a new cover. It is, in fact, a new and modified edition of 'Wagner's Physiology,' which had for some years been out of print. The last edition of Wagner's book was published in 1844, and four years ago a new edition of it appeared, edited by Dr. Funke. The latter differed so much from the former edition, however, both as regards plan and substance, that the editor replaced Wagner's name by his own. The copy that we are now about to examine is a still newer edition, the first volume of which appeared in February, 1858, the last about two months ago. On seeing that the book was a manual for students, we did not expect to find in it much material for criticism; for in general such works neither contain a great amount of original matter, nor give a detailed account of unsettled questions. A careful perusal of 'Funke's Physiology' has, nevertheless, taught us that not only the plan and style, but even the substance of a mere compilation may prove a fruitful field for the reviewer. In order to produce a good text-book on physiology, the compiler not only requires to have a very extensive knowledge of his subject, but must possess a remarkably sound judgment to guide him in the selection of material. Again, if the writer presumes to a still higher office than that of a mere collaborator, and attempts to inculcate a new doctrine, or extract truth from the midst of conflicting opinions, extensive knowledge and sound judgment become still more essential. On examining the work before us, we find that Dr. Funke has not condescended to be the mere compiler of the labours of others, but has frequently taken to himself the duties of an advocate, pleading now the case of this, then of that theory. Nay, more, he has occasionally assumed the office of a judge, and after summing up the cause, has pronounced judgment in accordance with his own views. Consequently, our author has laid himself open to criticism, as will be seen in the following analysis of his book.

The introduction need not detain us; for notwithstanding its being headed by the attractive title, "On the Physiology of the Metamorphoses of Animal Tissues," we have discovered in it nothing new either in the way of fact or theory. We at once pass on, then, to about the middle of the first chapter, where the author speaks of the quantity of blood contained in the human body. This is a point of some importance, and one about which, unfortunately, there still remains considerable difference of opinion, although nothing like what formerly existed when some authors gave the amount of blood in an adult as low as eight, while others reckoned it as high as one hundred pounds. Within the last few years several physiologists, seeing the necessity of ascertaining, with something like exactitude, a knowledge of the amount of blood circulating in the human body, have attempted in various ways to collect and weigh it. One and all of them, however, have discovered the task to be much more difficult than they had at first anticipated. The amount of fluid poured from the vessels of an animal bled to death, has been found to be far short of the actual amount present in the body. Life becomes extinct after the removal of a mere fractional part of the blood, and no sooner has death supervened than the flow is arrested. Even the application of mechanical pressure to the limbs and internal organs has proved insufficient to empty the vessels of their contents. Hence it is that observers have been driven to attempt the solution of the question by a variety of other means. Some have measured the volume of liquid which the bloodvessels of a dead body are capable of containing (Herbert), and have resigned it as fallacious. Others have invented a method which they supposed would be more trustworthy than the preceding. We allude to that adopted by Valentin. Being aware that the absolute quantity of a solution, which contains a definite per-centage of salts, may be ascertained by adding a known amount of water to it, and determining the per-centage of salts in the diluted mixture, Valentin removed by venesection, from a living animal, a small quantity of blood, carefully weighed, and estimated the amount of solids it contained. Then, after injecting into the animal's circulation a measured quantity of water, he immediately withdrew another portion of blood, and ascertained, as before, its per-centage of solids. Out of the relative concentration of these two portions of blood he was able to calculate the amount of fluid circulating in the system. His experiments, which were made upon sheep, dogs, rabbits, &c., led him to the conclusion that herbivorous, had less blood than carnivorous animals; and that the proportion of blood was to the weight of the body from one to four, to one to six. Taking the average value, he reckoned that the human body would contain one-fifth of its weight of blood. It is easily seen that the method pursued by Valentin is open to many objections. The injection of water into the circulation has a very important effect upon the various secretions, and although the second portion of blood is withdrawn almost immediately after the water is injected, yet, during the interval, a quantity of fluid must escape by the renal, cutaneous, pulmonary, and other secretions, and thus give rise to considerable inaccuracy in the calculations. It is generally agreed that Valentin's method yields too high an estimate.

Drs. Ed. Weber and Lehmann proceeded in another way to elicit how much blood circulated in the human frame. Their observation was made upon a condemned criminal. The individual was weighed before and after decapitation, in order to ascertain the quantity of blood that flowed from the divided vessels. Some of the blood, too, was collected, and the per-centage of its solids determined. The vessels of the body were then injected with water, and washed out until what came away was no longer coloured. The amount of solids in the collected washings was next determined, in order to discover how much blood had remained behind in the vessels. The total quantity of blood found was fifteen pounds, equal to one-eighth the weight of the body. This method, although not free from sources of error, is yet, we think, a much more trustworthy one than that adopted by Valentin. The only objection of moment that has been advanced against it, is grounded upon the fact that, during the washing out of the vessels, some of the solids of the tissues must have been, obediently to the laws of diosmose, extracted, thereby augmenting the amount, and increasing the calculated quantity of blood. We have some doubts with regard to the value of this objection. Any one who has attempted to inject water through the vessels of a dead animal, must have noticed that a large amount of water is forced through the walls of the capillaries into the surrounding tissues, and distends them. During its passage the water carries with it a certain portion of the solids that are present in the capillaries. Weber's injection, being rapidly performed, would therefore not have time to take much more from the parenchyma of the organs than it would have previously driven by mechanical force into it. If this supposition be correct, the estimate he gave would, after all, not be so very far from the truth.

The newest method of determining the quantity of blood is that proposed by Welcker. In a given quantity of blood there is a definite amount of colouring matter, which when dissolved in a certain quantity of water, yields a precise shade of colour. Welcker has made a colour-scale (*Farben-scala*) by taking and diluting blood with from four hundred to six hundred and sixty-six times its measure of water, and producing a stain upon paper with a drop of the dilution.* A scale of a known value being thus prepared, the next thing is to get the whole blood from the animal. This he does by first emptying the vessels, as far as possible, by dividing the larger arteries and veins in the neck. The remaining blood is then extracted from the tissues by hacking them into small fragments and washing them thoroughly with water.† The washings are then added to the blood, and the whole still further diluted till a drop on a piece of paper gives the same shade of colour as that yielded by one of the pieces in the colour-scale. By a simple calculation the total amount of blood that was present in the body of the animal is then easily ascertained. As venous gives a darker stain than arterial blood, the scale is made from a mixture of the two. At first sight Welcker's method appears to be very difficult, and one likely to give uncertain results. The contrary has proved to be the case, however, for in the 'Jahresbericht' for 1854, Henle states that Dr. Welcker, in the presence of the members of the Natur forscher-Versammlung, estimated a quantity of blood which had been poured upon a towel with nearly perfect exactitude. And since then we find other gentlemen have adopted the method with success (Bischoff, Heidenhain). Welcker has very recently published the result of his experiments,* and gives in the subjoined table the average quantity of blood in

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* Heidenhain has adopted Welcker's method, but made his scale by diluting blood with from 500 to 1000 parts of water.

† Welcker found that by decapitation only from sixty to seventy per cent. of the whole blood is obtained. Bischoff observed in the case of three guillotined criminals, that no more than seventy-four per cent. of the blood flowed from the vessels.

	Species examined.		The relative weight of the blood to that of the body.		
			Ranged between		Average.
Fish	3	....	1 : 74 — 1 : 19,4	....	1 : 49
Amphibia	4	....	1 : 33 — 1 : 15,9	....	1 : 20,5
Amphibia (scaly) . . .	4	....	1 : 20,4 — 1 : 15,4	....	1 : 17,2
Birds	4	....	1 : 13,1 — 1 : 10,9	....	1 : 21,1
Mammalia (man included)	8	....	1 : 18,1 — 1 : 12,0	....	1 : 14,7

Thus we see that fish have proportionally less blood than amphibia, amphibia less than birds, and birds still less than the mammalia. In the same paper we find it mentioned that, although the quantity of blood increases with the weight of the body (in the same species), yet young have proportionally much more blood than old animals. The female, too, is generally found to have less blood than the male.

Bischoff† has lately estimated by Welcker's method the quantity of blood contained in the bodies of two guillotined criminals, and found that the proportion of blood to the weight of the body was in one of them as one to fourteen, in the other as one to thirteen. It appears, then, from the most recent researches that the quantity of blood in the body is smaller than was supposed (Valentin, one-fifth; Weber and Lehmann, one-eighth), and that on an average it may be reckoned at one-thirteenth or one-fourteenth.

To return to our author. In the chapter he has devoted to the consideration of the quantity of blood-corpuscles, we find nothing new. He still gives Vierordt's estimate—namely, that the male has 5,000,000, and the female only 4,500,000 in the cubic millimetre (1 mm. = $\frac{1}{25}$th of an inch).

We will now direct the attention of our readers to a more important question, at least as regards the practical physician. We allude to the form which the blood corpuscles assume in health and in disease. Every one who has paid attention to the microscopical examination of blood taken from patients, must have been puzzled with the diversity of forms which they present, and the changes they undergo, even while the eye of the observer is upon them. With regard to these changes our author remarks:

> "When a drop of blood, placed under the microscope, begins to evaporate, the cells immediately commence to alter their shape. They lose their circular form, become lengthened, angular, notched, and jagged, so that some of them look like raspberries, and appear to be composed of a group of small balls. This appearance is chiefly due to the evaporation of the contents of the corpuscles through the outer membrane, and a consequent shrinking of it. In the case of the raspberry-shaped corpuscles, it may also possibly arise from a secretion of granules from the cell wall, analogous to the formation of granules in the blood corpuscles of fish and amphibia. When a thin layer of blood is allowed to evaporate on a slide, the cells generally dry without undergoing any distortion." (p. 13.)

We cannot allow this quotation to pass without some comment, for the views therein contained are in some respects contrary to our own. From the above paragraph it would appear that Funke thinks that the alteration in the shape of the blood corpuscles is entirely due to exosmose. We admit that when a drop of blood is permitted to dry on a slide under the microscope, many of the cells become serrated; but we are not prepared also to admit that this change in their form is entirely due to exosmose of the liquid contents of the corpuscles. For we have as frequently seen it take place in diluted as in concentrated blood. Even in the example cited by our author, we think if the drop of blood be carefully examined it will be generally found that the serrated or jagged cells first make their appearance at the circumference. Now, although it is at this point that evaporation occurs when a covering-glass is used, and at first sight it would appear to be the part most concentrated, yet it is not in reality so. The circumference is in fact the most diluted portion of the whole drop. The pressure of the covering-glass drives the liquid serum from the centre to the circumference, and if the corpuscles in the centre of the field be examined, they will be found to retain their circular shape long after those at the periphery have become altered. If a hair or fragment of cotton-fibre be introduced under the covering-glass, in order to raise it more at one point than at another, the serum of the drop will collect around it, and the blood-cells in this neighbourhood will be observed to be more serrated than elsewhere. Again, if a little water be added to a drop of blood in which no serrated cells are to be seen, a great number will probably appear in the course of a few seconds. In fact, we believe that it is the dilution, and not the concentration of the blood, which most favours the production of the serrated appearance. If Funke's statement be correct, how does it happen, as he himself says, that "when a thin layer of blood is allowed to evaporate upon a slide the cells become dry without any distortion taking place?" The cells in this case ought, according to his previous statement, to have all become serrated. When we examine the blood of patients suffering from diseases which render it thin and watery—such, for example, as scurvy—we often find that the blood corpuscles are nearly all serrated.

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* Zeitschrift für rat. Med., Dritte Reihe, Band iv. p. 145.

† Zeitschrift für wissensch. Zoologie, Band vii. 3, p. 331; and Band ix. 1, p. 65.

Lately we had occasion to examine the blood of a woman labouring under a rapidly-growing fibro-plastic tumour of the uterus—she died a few days afterwards—a gentleman who was standing at the bedside remarked how very watery the blood looked while oozing from the finger. On placing a drop under the microscope, many serrated cells were seen; and for experiment's sake, some water was added, in order to render it still more diluted. The immediate result was a very large increase in the number of serrated corpuscles.

It has been thought by some that stellate or serrated cells were characteristic of fever. A tolerably extensive observation of blood in different diseases has led us to the conclusion that this appearance is limited to no particular class of disease, but is to be found in all cases where the blood is in a poor condition, whether it results from sickness or want of nourishment. We must at the same time add that the cells of some bloods, which do not belong to this category, have a much greater tendency to become serrated than others, and consequently it cannot be mere dilution which alone favours it. The tendency must pre-exist, the water only calls it into play. The cause of this tendency, however, we are not in a position to point out.

Richardson has observed that the blood of jaundiced people and of great smokers has a dirty, dingy hue, is thin, and possesses a feeble coagulating power, in consequence, he says, of the presence of free ammonia. Further, that the blood-corpuscles do not in those cases run into rolls, but remain scattered over the field, are irregular in form and stellate.

Long before the publication of Dr. Richardson's valuable work we had noticed that the blood-corpuscles of jaundiced patients were large, flabby-looking, irregular in shape—not, however, stellate—and had a tendency to elongate. We have even noticed them sending out little processes, as if budding; occasionally, one would divide into two, or on the other hand, two cells might coalesce into one. With regard to the condition of the blood of great smokers—if individuals who smoke ten or twelve cigars daily deserve that title—we cannot say that the appearances described have attracted our attention. Since reading Richardson's statement with regard to the presence of free ammonia, and its action upon blood-corpuscles, we have made a few experiments. On adding a dilute solution of ammonia to a drop of blood placed under the microscope, the rolls of normal blood-corpuscles break up. The cells separate, become of a dull yellow hue, large in size, irregular in shape, oval or pointed, and flattened on their sides when they come in contact with each other. When a stronger solution of ammonia is employed, the cell walls dissolve, and the contents flow out, giving to the field a uniform yellow hue. The addition of acetic acid to this causes the whole to take on a granular appearance.

We have observed that several poisons, especially that of the African puff-adder, cause the blood to assume an appearance very analogous to that produced by ammonia. Kölliker noticed that a solution of urea induced the corpuscles of frog's blood to become stellate. Be the rationale of these things what it may, we have little doubt that when the microscope has been longer employed in clinical research, and by individuals who have been properly trained to use it, we shall find in applying it to the blood an important assistance to our diagnosis.

Funke has, in his text-book, given us no less than seven pages on the cause of the coagulation of the blood, nearly the whole of which are devoted to the exposition of the views recently put forward in Germany by Bruecke and by Zimmermann. And strange to say, he has taken no notice whatever of our countryman's elaborate researches, except in a note of three lines at the end. The note we shall presently translate. We must here notice a point which we would have gladly omitted—namely, that in the work under review, which is in many respects deserving of commendation, the author displays the grossest ignorance of English medical literature—he seems indeed to be perfectly unaware of the existence of our periodicals—and consequently many points of importance which have been fully investigated and settled in England, are still brought forward in the most pompous manner. Can it be believed that an author writing a text-book on physiology in 1858, still declares that in asphyxiated and lightning-struck individuals, "the blood does not coagulate in the vessels?" In the first case, on account of the absence of oxygen; in the second, because of the decomposition of the fibrin by the action of electricity? It would take up too much of our space to cite all the paragraphs in which our author displays a similar amount of ignorance of English medical literature. We say this with no wish to damage his work in the eyes of the public, but with the desire that if this review should ever chance to meet his eye, he may take the hint, and before publishing another edition of his text-book, glance over some of our English periodicals. It will do him no harm, and we venture to say his book will be much the better for it.

To return to what our author says regarding the coagulation of the blood. The questions to be settled are, "What keeps the fibrin dissolved in the circulating blood?" And, "What change occurs in the blood when it coagulates?" Bruecke* says that the result of his labours upon the cause of the coagulation of the blood led him to the conclusion that the fibrin is retained in a fluid state through an influence exerted upon it by the coats of the living bloodvessels. This conclusion is, to our mind, a very unsubstantial one. The coagulation of the blood is favoured by the presence of foreign substances. Virchow, Bruecke, and Van der Kölk, observed that a drop of mercury, or a fragment of platinum wire, put into blood became coated with fibrin before the remainder of the blood coagulated. The blood in an obstructed vessel, as is well known, coagulates sooner upon the walls then in the middle. These facts, however, prove but little; for it cannot be supposed that the presence of a foreign body, such as platinum wire, can exert any other than a mechanical action, similar to that of a thread in a solution of sugar. Who can say that the solidification of fibrin is anything more than a physical change in the position of its atoms, just as occurs in the crystallization of sugar? Zimmermann thinks that the coagulation of fibrin is nearly allied to the process of fermentation, and is brought about by a kind of putrefaction occurring in some of the constituents of the blood, after its removal from the body. He says, moreover, that this fermentation may be regarded in the light of an oxidising process. If so, our author inquires, why does it not take place equally well in the circulating blood, which always contains plenty of oxygen? Zimmermann has not answered this question satisfactorily. Still less has he shown good grounds for adopting the view of a putrefactive process occurring in the blood at the moment of its coagulation. In fact, it appears to us that the chief evidence he has adduced in its favour, is founded upon the circumstance that matters which possess the power of hastening the putrefaction of blood, have also the power of promoting the solidification of its fibrin. Funke has not troubled himself with the consideration of the ammonia theory. He seems to look upon it as beneath his notice, and dismisses it in the following note:—

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* See vol. xxxvii. of this Journal.

> "We might bring forward abundance of theories regarding the coagulation of the blood, not, however, a single one well founded, although some sufficiently adventurous. As, for example, that of Richardson, who thinks that the presence of ammonia in the blood is the cause of the fibrin being in solution, the evaporation of the ammonia the cause of its coagulation. Zimmermann has given himself unnecessary trouble to overturn this hypothesis. For not even has the presence of ammonia in normal blood been satisfactorily proven." (Note 3, p. 30.)

Even if Dr. Funke's views differ from those entertained by Dr. Richardson, he might at least have mentioned some of the facts which that observer brought forward in support of his theory.

We shall now terminate our remarks upon the blood by a rapid glance at its crystallization, or we should rather say, "the formation of crystals in blood." Our author commences the chapter he has given up to this subject, by saying that—

> "Under certain external influences, the organic colouring matter, which is an essential part of the contents of the red corpuscles, can be made to assume a crystalline form in the drawn blood of every animal. The contents of the cells of each kind of blood having their own characteristic form of crystals. The chief point in crystallizing the 'hæmato-globuline' or 'hæmato-crystalline' being to remove it from the interior of the cells."—(p. 31.)

From this paragraph one would suppose that Funke considered it a very easy matter to obtain crystals from all kinds of blood. Indeed, a little farther on in the chapter we find him saying that crystals form completely and readily under the influence of light, if blood has been first exposed to a current of oxygen, and afterwards to a stream of carbonic acid gas. Which, he says, is easily done by exposing a drop to the action of the air for a few minutes, diluting it with water, and then, before covering up with thin glass, breathing upon it. This and several other methods, we have frequently adopted, but not with invariable success. Occasionally we have found certain bloods crystallize rapidly, and without the slightest care being bestowed upon them; while at other times we have failed, in spite of the utmost attention, to obtain by the above method a single crystal. This, we think, in many cases arose from the circumstance that the hæmato-globuline was not always in a condition favourable to crystallization. There is a method, however, which has never failed in our hands—we allude to that described by Teichmann. To dried and powdered blood is added some common salt, and the mixture of the two is dissolved in glacial acetic acid with the aid of heat. The hæmato-globuline crystallizes on cooling. The crystals so obtained are invariably small and generally rhombic prisms.

Funke's second statement in the above quotation, namely, that the contents of the blood corpuscles in each species of animal have a characteristic crystalline form, is, we believe, to a certain point established. Crystals obtained, for example, from guinea-pig's blood are tetrahedral, from squirrel's hexagonal, from horse's rhomboidal, while from man's they are sometimes prismatic, occasionally laminated rectangles, but most frequently rhomboids.

No crystals have ever yet been found in freshly drawn healthy blood; but it is no uncommon thing to find them deposited in the body in apoplectic clots, aneurismal tumours, &c. They occur very frequently, too, as pointed out by Dr. Parkes, in half-putrid blood.* There is one curious case on record where a mass of crystals, the size of a walnut, was found in a cyst in the liver (Robin). The crystals were of a beautiful scarlet colour, and free from other matters. Nature seemed to have, by some means or other, deposited them in that position in a state of purity. Funke mentions having seen crystals in the interior of the blood corpuscles of some fishes; and further remarks, that they have never yet been found in the corpuscles of any other animal. We are rather surprised at the statement made in the latter half of this sentence, for on referring to an article by Dr. Sieveking, on Albuminous Crystallization, we find it stated that—

> "Kölliker is the first who observed them (crystals) in the corpuscles. In the blood of a dog's liver he found a great number of blood-globules, containing from one to five rodlets, of a dark yellow colour, which remained unchanged in water, but seemed to disappear under the influence of acetic acid. He next observed the same appearances in the splenic vein of a dog."†

Funke, we find, denies the existence of white crystals, and ridicules the idea of such ever occurring, saying, that inorganic salts have been confounded with them. Now, we do not doubt that this has frequently enough occurred, and we fear we have evidence of it in the last number of the 'Pathological Society's Transactions,' where a gentleman has given a drawing of crystals from a clot in the corpus luteum, that have no resemblance whatever to crystals of an organic nature, but look like badly formed phosphates or chlorides. Very good and trustworthy observers, however, have described white blood crystals, and we see no reason to doubt their existence, as it frequently happens that one part of a crystal is far more deeply tinged with colouring matter than another.

Now comes the important question, "What is the composition of blood crystals?" We shall not at present relate all the theories that have at various times prevailed regarding their nature, as that has already been done by Dr. Sieveking, in the article above alluded to. We will merely mention that at one time they were looked upon as being inorganic, at another as a combination of organic and inorganic substances, while more recently they were thought to be composed entirely of organic matter. In fact, until we perused the work before us, we imagined that all were agreed in regarding them as a mixture of hæmatin, and a protein substance. Funke, however, seems still determined to hold an opposite opinion, and has (we think) wasted two or three pages in trying to prove that hæmato-crystalline is not a compound, but a simple body. We have purposely used the word "wasted," because his arguments are mere gratuitous assertions, and in the end he has been forced to state facts that are diametrically opposed to his theory.

Lehmann found that on boiling an aqueous solution of crystals he obtained a coagulum which weighed less than the crystals he had dissolved, and that there remained behind a liquid with an acid reaction, although the solution of the crystals before being boiled was distinctly neutral. Moreover, he ascertained that the liquid contained a quantity of phosphoric acid salts, along with an undefined organic acid. Further, the same observer by chemical agents obtained from his crystals a colouring matter and a protein substance, the former of which he was able to crystallize, but not the latter. Nay more, on incineration, Lehmann's crystals leave an ash, composed chiefly of iron, but containing also phosphates, chlorides, and traces of sulphates. How, then, can Funke, who is fully cognizant of these facts, still attempt to maintain that hæmato-crystalline is a simple substance? All observers agree in stating that hæmatin exists in the crystals—which, by the way, our author says is an optical delusion, there being, according to him, no such body in nature—and the existence in them of a protein substance cannot now be denied, since it is found that their watery solution is coagulated by heat and by alcohol. From our own, and the observations of others, it appears that several kinds of coloured crystals form in blood, some being soluble, others insoluble, in water. The crystals, too, which are obtained from the blood of different species of animals, if we may judge from their shape, have certain peculiarities in their chemical composition. Still, however, we think that the difference among them results not from a variety in the kind, but merely in the proportion of their ingredients. We cannot for a moment admit, as our author would have us to do, that hæmato-globulin or hæmato-crystalline has in any case a definite chemical composition; indeed Lehmann, who so long inclined to such an opinion, has at length acknowledged that *his* crystals have sometimes a greater, sometimes a lesser, proportion of ash.

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* Medical Times and Gazette. 1852.

† Medico-Chirurgical Review, vol. xii. 1853.

Before leaving this subject, we may mention that Funke lays claim to the discovery of blood crystals. Had our author been better acquainted with our English medical literature, he would not have fallen into this error, for he would then probably have known that Sir Everard Home found them, as early as 1830, in an aneurismal tumour, while his own researches were not published till twenty-one years afterwards! Reichert and Kölliker had also described blood crystals in 1849. Funke, whose first paper only appeared in 1851, gets over this difficulty by saying that the authors who preceded him entirely misunderstood their nature (*vollständig verkannt*).* We think, however, that after reading what has been said above, our readers will agree with us in saying that, if there is any misunderstanding, it is most probably upon our author's, not upon his predecessors', side.

Some of the gentlemen who honour us with a perusal of this article may perhaps observe with surprise that in the foregoing remarks no allusion has yet been made to the 'Treatise on Physiology,' by Monsieur Longet, notwithstanding that the title of his work is placed on the first page of our review. To these readers we beg to remark that our silence on this point must not be attributed either to negligence, or to any desire of passing over his labours in silence, but to the simple and patent fact that hitherto we have been speaking on subjects which are not discussed in Longet's treatise. Many of our readers are undoubtedly aware that, notwithstanding that Monsieur Longet's book on physiology has been coming out in parts during the last seven or eight years, it is still incomplete; and it so happens that the part on the circulatory system, which we have been discussing, is the very one that is still wanting. We have therefore, had as yet no opportunity of introducing the name of Longet into our review.

For the benefit of those who are not acquainted with the treatise now before us, we may mention that its author reversed the usual order of publication, and issued the second volume before the first. Indeed, it is now only the first part of the first volume which is wanting in order to render the work complete. So long a period has elapsed since the publication of the second volume, 1850, that we need scarcely do more at present than allude to its contents. It is divided into three books, the first of which is devoted to the Consideration of the Different Senses: the second to the Cerebro-Spinal Nervous System; and the third and last to Reproduction.

While examining the properties and functions of the nervous system, our author makes the following remark regarding the different views at present held as to the nature of nerve force. Some observers, and perhaps the majority, he says, are strongly inclined to the opinion that nerve force is identical in its nature with electricity; while others, again, think that although it closely resembles electricity in many respects, it is nevertheless a peculiar principle. After having examined with impartiality the numerous facts upon which both of these theories are founded, it does not appear to him that there are at the present moment sufficient grounds to authorize his adopting the view that there exists exactly the same kind of analogy between nerve-force and electricity, as between heat, light, and magnetism. He therefore still inclines—or, we should rather say, inclined, for as nearly eight years have elapsed since he made the above statement, it is not at all improbable that ere now he has found good reasons for altering it—to the opinion of those who regard the active principle of nerve matter as a force *sui generis.*

Longet commences the part of his book on Reproduction with an introductory chapter on Spontaneous Generation. This subject, a few years back, when one of our countrymen thought he had discovered a means of creating animals by an electric current, would have formed a good theme for the critic; to enter upon it now, however, would only be wasting the time of our readers. The chapter contains an admirable *résumé* of the question as it now stands, and may be referred to with advantage by those of our readers who feel interested in the subject.

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* Note, p. 32.

Our author enters very fully into the question of reproduction. He examines the various phases which it presents through the whole animal series, beginning with the lowest types at the bottom of the scale, where it takes place by a simple scission of the body, and gradually ascending upwards to the highest forms of development, in whom generation can only be accomplished by a voluntary combination of different sexes.

We shall now briefly notice that part of Longet's treatise which last issued from the press. It forms a volume of two hundred and eighty-four pages, and is exclusively devoted to the function of digestion. In our opinion this, the last, is by far the best part of the whole work. The subject is fully investigated in all its bearings—so fully, indeed, that it will be impossible for us to give anything more than a meagre abstract even of a portion of it; we must therefore take the liberty of referring our readers directly to the original.

Our author begins this department of his work by describing the different forms of digestive apparatus met with in the various classes of animals, commencing, as in the case of reproduction, with the lowest, and gradually ascending the scale to the highest. He then describes the different sorts of foods, dividing them into the albuminous, saccharine, oleaginous, and inorganic. Next considers the physical changes they respectively undergo during the digestive process. Lastly, he examines the nature and chemical properties of the saliva, gastric juice, bile, pancreatic and intestinal secretions, pointing out how it is that these digestive fluids or solvents are not brought into action at every stage of digestion, or even upon each kind of food; but that one acts only upon this, another only on that aliment. For example, he shows how the saliva attacks amylaceous and not albuminous matters, how the gastric juice disolves fibrin, but not fat. How bile acts on oleaginous, and not on saccharine substances. Our author likewise indicates that the mode and power of action of the various digestive secretions are not entirely dependent on their acid or alkaline properties, but upon their containing peculiar and various kinds of organic substances. Thus, for example, he has been able to prove that the acidifying of the alkaline saliva will never endow it with the digestive powers of the acid gastric juice, nor will the neutralization of the latter enable it to embrace the properties of the former.

Longet has also pointed out a very simple, and therefore useful, means of distinguishing between albumen that has been digested from that which has merely been dissolved. If, for example, a small quantity of grape sugar be added to a simple solution of albumen, and then the sulphate of copper and potash test be employed for its detection, the presence of the sugar will be made manifest through the reduction of the red oxide of copper. On the other hand, however, if grape sugar be added in similar quantity to a solution of albumen that has undergone the action of the gastric juice, the same test applied in the same way will be found totally inadequate for its detection. The mixture, in the latter case will at the same time become of a beautiful violet colour.* We may here mention, however, an important fact, which appears to have escaped the notice of our author—namely, that digested albumen is not the only substance which possesses the power of masking the presence of sugar, and giving to the mixture with sulphate of copper and potash a fine violet hue. Bernard has pointed out that gelatine, and Harley that casein and meta-albumen, possess the same property in a high degree.

We shall now briefly direct the attention of our readers to the summary on the digestive function as given by Funke. It need not detain us long, as digestion seems to be a study to which Funke has not specially directed his own researches. He says that all substances that are soluble in acidulated water are soluble in gastric juice. By this, however, he is not to be understood to mean that the action of the gastric juice is identical with that of an acid, for at p. 264 we find him saying that the action of the gastric juice on protein substances is very nearly allied to that of a ferment. Our readers will perhaps understand us better if we say that he adopts Liebig's fermentation theory of digestion.

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* Longet, vol. i. part 2, p. 154.

It is well known, as we have already hinted, that all kinds of food are not digested in the stomach, some passing through it—such, for example, as fats—without undergoing any chemical change; others, again, being only partially transformed there. The transformation of starch into sugar, too, may be cited as an example of a change which, although it occurs to a certain extent in the stomach, is not due to the action of the gastric juice, but to the swallowed saliva. Bouchardat and Sandras have likewise pointed out that cane is converted into grape sugar in the stomach; and, according to Funke, this is brought about neither by the action of the gastric juice, nor by that of the swallowed saliva, but by some element of the food taken along with the sugar. "Which it is, and how it acts, being still unknown." (p. 267.) Frerichs contradicted the statement of Bouchardat and Sandras, but it has been again confirmed by Lehmann and Harley. The latter of these gentlemen has, moreover, shown that the transformation of cane into grape sugar in the stomach is not due, as Funke supposed, to the action of some element of the food, but to the free acid of the gastric juice itself.

We just now said that fat undergoes no chemical change in the stomach; we might have added, however, that it is there brought into a state of fine subdivision by the combined action of the heat and motion of the organ. Muscular fibre, on the other hand, undergoes an important change; its connective tissue is dissolved away, and the fibres separate and break across into short segments. The fibre itself also gets clearer and paler, the striæ occasionally entirely disappearing, and thereby making the fibre look like a piece of tendon. Frerichs said that, after weeks of artificial digestion, muscular fibres were not entirely dissolved. On this point our author remarks (p. 269), that although it be true that all the fibres of a piece of muscle are not completely digested in the stomach, Bernard is nevertheless wrong in stating that none of them are dissolved during their sojourn there.

Funke makes a statement regarding the muscle of fish which, it must be acknowledged, we read with some degree of surprise—namely, that its muscular fibres are more difficult of digestion than those of the mammalia. He further remarks that the flesh of old is not so readily digested as that of young animals. These views are opposed, we think, to those generally received. Fish, with the exception of salmon and a few others, has generally been considered as a light food; veal, on the other hand, a heavy one. Some substances, although they may remain in the stomach for an indefinite period of time, are still able to withstand the action of the gastric juice. Among these may be mentioned elastic tissue, hair, horn, vegetable cellulose, and spiral vessels, and, according to Funke, not improbably the cells of cartilage. Gelatine, again, although it is derived from cartilage, is speedily dissolved and transformed by the gastric juice into peptone (Longet).

While reading the chapter in Funke's 'Physiology' devoted to the consideration of the biliary function, we were struck with a very remarkable passage at page 278, where he says that we have no proof whatever that the bile, during its temporary stay in the intestinal canal, takes any share in the digestive process. He thinks, moreover, that as bile does not pre-exist in the blood, but is merely secreted from it by the cells of the liver, and as it is not reabsorbed into the circulation with its constitution unchanged—the blood of the portal vein containing neither the acids nor the colouring matter of the bile—it is highly probable that bile is not emitted into the intestines in order to take a share in the function of digestion, but is there merely as a secondary product (*nebenproduct*) of the hepatic blood-formation. Further, according to our author, it would appear that after the bile has been modified in the intestines—without, however, taking part in the digestive process—it is returned to the general circulation in order to perform a part in the metamorphosis of tissue. Few, we think, will be at present inclined to adopt this novel view of the biliary function. It is but a theory built upon the shifting sands of hypothesis; and, as such, is more likely to raise an evanescent smile than a persistent reflection. We feel almost inclined to return upon Funke, as regards the bile, a remark which he has made upon Bernard—namely, that he totally misunderstands the nature and importance of digestion.* The recent researches of Bidder and Schmidt have clearly shown that when bile is absent from the intestines, few fats are absorbed into the general circulation. And still more recently, Marcet has pointed out that bile acts by emulsioning the fatty acids of our food. Longet has adopted the general view, that the bile plays an important part in the digestive process.

Quitting the interesting subject of digestion, Funke next directs his attention to the function of respiration, and commences this subject with a chapter on the Histology of the Lungs. When upon the anatomy of the pulmonary organs, he takes no notice whatever of the epithelium lining the air cells; and, if we may judge from his silence, the names of Rainey, Clarke, and Hall must be equally unknown to him. On commencing the perusal of his remarks upon the chemistry of respiration, we found him recognising the fact that the whole process of nutrition depends upon the absorption of oxygen by the blood, and consequently expected to find him keeping pace with the literature of the day. But, alas! we are sorry to say that he still pertinaciously holds to the exploded doctrine of oxygen entering into no chemical combinations with the constituents of the blood. We would therefore take the liberty of recommending him to peruse an article upon that subject which appeared in this Journal in 1856, where he will find it proven by direct experiment, that no sooner has the oxygen passed through the walls of the pulmonary capillaries than it commences to enter into chemical combinations with various constituents of the blood, in order to prepare them for the part they are to play in the process of nutrition.

It was our intention to have reviewed our author's remarks on the nervous system; but having already expended all the space allotted to us for this review, we must delay the consideration of the rest of the volume until we find an opportunity of doing so in conjunction with some affiliated work.

In conclusion, we would strongly recommend to our readers the perusal of Longet's and Funke's treatises on physiology; and at the same time inform them, that although we have felt it our duty to criticise Funke's work rather severely on one or two points, we have not formed a poor opinion of it. On the contrary, we freely acknowledge that its perusal has been to us a fruitful source of pleasure and profit. And we think that if its author bestows a little more care upon the next edition, his work will not fail to acquire a very high place among the text-books on physiology.

Review VIII.

Lectures on the Diseases of Women. By Charles West, M.D., Fellow of the College of Physicians, &c. Part II. *Diseases of the Ovaries, &c.—London,* 1858. pp. 247.

Within the period he mentioned, Dr. West has well and ably fulfilled his promise, and given to the profession his views on those varieties of female diseases which were omitted in his former volume. Our readers will remember, doubtless, the favourable opinion we expressed of that volume; and we are happy to state that our judgment has been fully confirmed by a perusal of the present. To a singularly easy and pleasant style, Dr. West adds a lucidity of arrangement without pretence, and a felicity of expression, which must necessarily make him a favourite with his readers; whilst he exhibits such a careful avoidance of exaggerated statements, and such a sober care in making deductions, that confidence follows as a matter of course.

The first subject, occupying the two first lectures, is what has been latterly termed pelvic abscess, the result of inflammation of the uterine appendages and the cellular tissue; and although there is nothing new, yet what was scattered before is here collected, with valuable additions from Dr. West's own experience, so as to present a very complete account of this frequent and troublesome disease. The treatment by leeches, poultices, &c., is in entire accordance with our own experience, and also Dr. West's cautious advice about puncturing the tumour. There is a short but graphic account of peri-uterine hæmatocele, contrasting it with the inflammatory pelvic tumours.

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* "Bernard welcher überhaupt Natur und Bedeutung der Magenverdauung gänzlich verkennt," p. 262.

But the great bulk of the volume is made up of the lectures on Diseases of the Ovaries; and we feel bound to say that we know of no better essay on the subject—none so good in so short a space. The author commences with ovaritis, and very properly by a confession of the imperfect state of our knowledge on the subject; then he gives the pathology, such as we know it, of the disease, the symptoms which it induces; and lastly, the peculiarities of chronic inflammation of the ovary. We quite agree with Dr. West that many of the symptoms which have been supposed to indicate an inflammatory or sub-inflammatory condition of these organs are rather neuralgic in their character; but we do not quite concur in his opinion that it is so very difficult to cure. Chloroform alone, or chloroform and oil, camphor liniment with belladonna, and the tincture of aconite, are the remedies of which Dr. West speaks most favourably.

Passing on to the consideration of ovarian tumours, Dr. West, following Mr. Paget's arrangement, divides them into, 1, simple cysts; 2, compound or proliferous cysts; 3, alveolar or colloid cysts; and 4, cutaneous or fat cysts; and upon each subject we find accurate and sufficient pathological information. Of the two, we prefer Dr. West's theory as to the formation of cysts from the Graafian vesicle, to Scanzoni's. As regards the possibility of a single cyst becoming a compound one, Dr. West observes:

> "My belief, though I cannot adduce absolute proof of its correctness, is, that such a change may take place, and that a cyst originally barren may become proliferous; that its continuing simple is rather a happy accident than a condition on the permanence of which we can reckon with any certainty." (p. 72.)

In this we quite concur, and so far as a long clinical observation deserves the character of proof, we have had proof of it.

Of 415 cases, Dr. West finds that the right ovary alone was affected in 201; the left in 148; and both in 66 cases; and he has shown that the disease may occur before pregnancy but after marriage, during pregnancy and after delivery. It has been our lot to see one case of pregnancy, notwithstanding the existence of a tumour of both ovaries.

In answer to the query of what is the course and tendency of these tumours, we are told that some—we fear but few—may be absorbed, or at least remain stationary; that others may be evacuated through the Fallopian tube, vagina, or intestinal canal, or into the peritoneal cavity. Many, the greater number, probably, persist, and undergo the various changes so familiar to us in ovarian dropsy, and of which Dr. West has given an admirable description; ultimately proving fatal, either by pressure upon other organs, or by gradually undermining the health. The symptoms to which they give rise consist in disordered functions, or pain, effects of mechanical pressure, or general cachexia; with certain accidents or results of attempts at relief.

We strongly recommend to our readers the section on the diagnosis of these tumours. With the following remarks we fully agree:

> "The difficulties which we encounter in the diagnosis of tumours of the ovary vary according to the size of the growth and the situation that it occupies. So long as it remains principally within the cavity of the pelvis, it for the most part yields but an indistinct sense of fluctuation, even though its contents should be entirely fluid, and it may then be hard to distinguish between it and the results of inflammation of the broad ligament, or between it and a fibrous tumour of the womb, or the retroflected uterus itself, especially if the organ is enlarged by pregnancy. When the growth has ascended into the abdomen, the distended bladder, the pregnant uterus, the enlargement produced by ascites, by tumours of the uterus itself, or by tumours of other organs, as the liver, spleen, omentum or mesentery, present so many separate sources of error, against which we need be on our guard; whilst last of all, the caution is not superfluous which warns us to be on our guard against imaginary tumours, such as are produced by flatus in the intestines, or by fat in the integuments or loading the omentum,

or against those still more unreal swellings which have no existence at all save in the disordered fancy of the patient." (p. 108.)

Or, Dr. West might have added, those phantom swellings which are due to none of the preceding causes, and which disappear under chloroform. That in many cases the diagnosis will be easy if Dr. West's directions are kept in mind is true, but some cases will prove a sore puzzle we are sure, and indeed this is proved by some of the instances given by Dr. West.

Dr. West's therapeutical division of ovarian dropsies is admirable; these are, 1, Cases which *may* be let alone; 2, Cases which *must* be let alone; and 3, Cases *justifying or absolutely requiring interference.* Happy the practitioner who has the sense to recognise in time the two first classes of cases; not that we mean that absolutely nothing is to be done, as many inconveniences may be removed as possible, but the tumour is not to be experimented upon—"*Quieta non movere.*"

We do not gather that Dr. West has any faith more than ourselves in the power of medicine, internal or external, in the removal or even diminution of these tumours. It must be our object that they be not irritated.

No one, we suppose, regards tapping as more than a temporary remedy, although in some few cases the cyst does not refill; and our general impression is, that it merely postpones the fatal termination in the best cases, whilst in the worst it may hasten it. Dr. West considers in detail the various modifications of this operation which have been proposed, such as tight bandaging, subcutaneous puncture per vaginam, keeping the cyst empty or open, and the employment of medicated injections. He does not seem to have much confidence in the first four of these plans; upon the fifth, iodine injections, he has entered into full details. After quoting the experience of others, he adds eight cases of his own. No case proved fatal, and in three there was no constitutional disturbance. Three of the cases were cured as regards the first cyst; in one there was "possible retardation;" in another, "marked retardation;" in one, "slight improvement;" and in two, "no benefit." But as Dr. West modestly observes, if these cases are too few to justify any decided conclusion, they at least show ground for a more extended trial. He is decidedly opposed to the use of very strong solutions, and to permitting them to remain permanently in the cyst.

There remains still for consideration the one great remedy about which opinions are so much divided—viz., ovariotomy, and on this we shall let Dr. West speak for himself:

> "Some points, indeed, must be left unsettled; but still there appears to me to be ground sufficient for some conclusion, and that I fear must be unfavourable to the performance of ovariotomy."

And for these reasons:

> "1st, The rate of mortality from the operation does not appear to be in course of diminution, as the result of accumulated experience and increased dexterity gained by its frequent repetition; 2ndly, Unlike most operations in which anything like the same rate of mortality occurs, it is scarcely admissible in the doubtful or desperate cases to which the Hippocratic axiom, 'ad summos morbos, summæ curationes,' applies. The cases in which it may be hoped that the disease, if left alone, will advance tardily or become stationary, those in which something may be anticipated from other less hazardous forms of interference, are the very cases that yield the successes on which it has been sought to establish the merits of ovariotomy. It is proved to be very hazardous indeed in the young; it is believed by some very competent surgeons to be attended by so much danger in those past the middle period of life, that they have proposed to regard the operation as counterindicated in all women who have exceeded the age of forty-five years. The compound cysts, the cysts with solid matter, the malignant, the quasi-malignant growths—those, in short, whose rate of progress is commonly most rapid, which are the most burdensome to the patient, are attended by the greatest suffering and admit of the least palliation by other means, are precisely the cases in which the surgeon shrinks most from ovariotomy. In the table drawn up by Mr. Humphrey, who

himself is an advocate of the operation, cases of this description yielded when operated on, 19 deaths to 20 recoveries; in my own table, deduced from a rather larger collection of facts, 56 deaths to 62 recoveries. 3dly, Not only is the operation so hazardous in those very cases where it is really most called for, that many surgeons shrink then from its performance; but even in instances that may be selected as the most favourable, we have no sure grounds on which to rest our prognosis as to its issue." (p. 172.)

We are not quite prepared to endorse Dr. West's judgment in this matter, and we cannot help hoping that an improved diagnosis, by enabling us to select the more suitable cases, will lead to a diminished mortality. Dr. West's investigations of the subject are so detailed, and elaborated with such laudable care, that they demand most respectful attention from all who are called to treat such affections.

The remaining portion of the volume has its peculiar interest; but our limits only allow us to commend the whole to the careful attention of the reader.

Review IX.

Œuvres d'Oribase; Texte Grec, en grande partie inédit, collationné sur les Manuscrits, traduit pour la première fois en Français: avec une Introduction, des Notes, des Tables, et des Planches. Par les Docteurs Bussemaker et Daremberg.—*Paris,* Imprimé par Autorisation du Gouvernement à l'Imprimérie Impériale. Tome I., 1851, pp. lx. et 692; tome II., 1854, pp. xii. et 924; tome III., 1858, pp. xxviii. et 724.

The Works of Oribasius; the Greek Text, partly hitherto unpublished, corrected by the collation of MSS., translated for the first time into French: with an Introduction, Notes, Tables, and Plates. By Dr. Bussemaker and Dr. Daremberg.

Just ten years ago (April, 1849) we gave a short account of Dr. Daremberg's projected 'Bibliothèque,' or collection of the old Greek and Latin medical writers. The plan was at that time suspended, in consequence of the political and social embarrassments which then prevailed in France; and, indeed, the difficulties that have arisen to thwart the execution of the work would have been more than sufficient to discourage any ordinary editor. But Dr. Daremberg is a man of no ordinary zeal and perseverance; and accordingly, after repeated applications to successive Ministers for their assistance, and after repeated disappointments, in consequence of the overthrow either of a ministry or a government, he has at last had the satisfaction of seeing the work, which was commenced under the auspices of Louis Philippe, completed (or at least in a fair way of being completed,) "par autorisation du Gouvernement à l'Imprimérie Impériale."

But the progress of the 'Bibliothèque,' from its commencement to its partial completion, deserves to be related rather more in detail; both because of its literary importance, and also because of the peculiar differences it exhibits from our English mode of conducting similar undertakings. It was as far back as 1843 (sixteen years ago!) that Dr. Daremberg, who was already favourably known by his inaugural dissertation on Galen's knowledge of the nervous system, conceived the idea of publishing a collection of the ancient medical writers, more comprehensive than Kühn's, and more carefully executed. After consulting MM. Littré and Andral, by whom the plan was favourably received, he applied to the Minister of Public Instruction, which post was then filled by M. Villemain; and at his request submitted to him a plan of the work, which received his official approbation, November 28th, 1844. He was then sent by the French Government on a mission to Germany to examine MSS. relating to the work; much in the same way as Dietz had, with a similar object, visited most of the great European libraries at the expense of the Prussian Government. After his return, in 1846, at the request of a fresh Minister, he submitted his plan to the Academy of

Inscriptions and the Academy of Medicine, both of which learned Societies expressed their approbation, and recommended it to the favourable notice of the Government. He was in consequence sent on a literary mission to this country, where he visited the principal libraries at London, Oxford, and Cambridge. On the 22nd of February, 1848, he received an official assurance that the Government would assist his work by bearing part of the expense. "But on the morrow," says he, "many other hopes besides mine were overturned." However, towards the end of the same year, in consequence of a fresh application to another Minister, the Republican Government authorized the printing of his edition of Oribasius at the National (no longer the *Royal*) press. In the early part of 1849 he paid a second visit to England, in company with Dr. Bussemaker, more especially for the purpose of collating a MS. of Oribasius in the library of St. John's College, Cambridge;* and, towards the end of the same year, he was sent by the Government to visit the libraries in Italy. In 1851, the official promise of assistance was once more ratified, and the first volume of his work appeared.

Among the large number of writers to be comprehended in his 'Bibliothèque,' it was a matter of comparatively little importance which author should be published first; and accordingly Dr. Daremberg was induced to commence with Oribasius by his meeting with Dr. Bussemaker, who had already published a portion of this writer's principal work, and who was then occupied with preparing a complete edition of it. (Tome i. p. v.) Upon the whole we think the choice of Oribasius a very judicious one. Though less known to the medical world than Hippocrates, Galen, Dioscorides, Aretæus, and Celsus, and therefore perhaps less generally interesting, this writer's works are much more rarely met with, and also offer much greater opportunities for the improvement of the text by the publication of numerous inedited passages taken from MSS. which had never been examined. But before saying anything more about the manner in which Drs. Bussemaker and Daremberg have executed their task, perhaps we may as well give a few particulars of Oribasius himself; and as the editors (by what appears to us a strange oversight,) have omitted all notice of his personal history, we shall have recourse to Dr. William Smith's 'Dictionary of Greek and Roman Biography,' which contains a tolerably full account of a life more than usually eventful and interesting.

Oribasius was born either at Sardes or Pergamus, probably about A.D. 325. He belonged to a respectable family, received a good preliminary education, and early acquired a great professional reputation. He became acquainted with the Emperor Julian before he ascended the throne, and was almost the only person to whom the young Prince imparted the secret of his apostasy from Christianity. He accompanied Julian into Gaul, A.D. 355, and it was about this time that, at his command, he began the compilation of his principal medical work. He is supposed to have been in some way instrumental in raising Julian to the throne, A.D. 361, and was in consequence appointed Quæstor of Constantinople, and sent on a mission to Delphi, to endeavour to restore the oracle of Apollo. He accompanied the Emperor in his expedition against the Persians, and was with him at the time of his death, A.D. 363. By the succeeding Emperors he was banished from Constantinople, his property was confiscated, and he was even in danger of his life. The cause of his disgrace is not known, but we may conjecture that he had made himself obnoxious, either in the discharge of his duties as Quæstor, or by his enmity against the Christians. In his exile, Oribasius exhibited proofs both of his fortitude and of his medical skill, and is said to have gained such influence and esteem among the "barbarian kings" (as they are called), that he became one of their principal men, while the common people looked upon him as almost a god. It is supposed that his banishment did not last many years. After his return he married a lady of good family and fortune, and had by her four children, one of whom was probably his son Eustathius, for whose use and at whose request he made an abridgment of his principal medical work, which is still extant. He also had his property restored out of the public treasury, and lived to a good old age, as he was alive as late as the year 395.

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* The results of his two visits to this country are *partly* given in his 'Notices et Extraits des Manuscrits Médicaux Grecs, Latins, et Français des principales Bibliothèques de l'Europe.' The first part contains a *catalogue raisonné* of the Greek medical MSS. now existing in England, with the exception of those in Caius College Library, Cambridge, which will be noticed in the *second* part of the work.

Of the personal character of Oribasius we know little or nothing; but it is clear that he was much attached to Paganism and to the heathen philosophy. He was an intimate friend of Eunapius, who inserted in his 'Vitæ Philosophorum et Sophistarum' an account of his life, in which he praises him very highly. He was the author of several works, of which we possess three that are generally considered to be genuine, viz., 1. The Συναγωγαὶ 'Ιατρικαὶ, *Collecta Medicinalia*, sometimes called 'Εβδομηκοντάβιβλος, from its consisting of seventy (or seventy-two) books; 2. The Σύνοψις, *Synopsis*, consisting of nine books, addressed to his son Eustathius; and 3. The 'Εὐπόριστα, *De facile Parabilibus*, in four books, addressed to his friend Eunapius. It is the first of these that is now being published by Drs. Bussemaker and Daremberg, and to this we will confine our observations.

It contains little original matter, but is a judicious collection of extracts from Galen and Dioscorides, and other writers whose works are no longer extant. It had become scarce, on account of its bulk, as early as the time of Paulus Ægineta, in the seventh or eighth century. It was translated into Arabic in the ninth century; but in the following century, though Haly Abbas was aware of its existence, he says he had never seen more than one book out of the seventy. More than half of the work is now lost, and (with respect to practical medicine) the most valuable and interesting portions. The state of the Greek text before the present edition may be briefly noticed, and we will at the same time mention the amount of hitherto unpublished books and fragments which are now for the first time brought to light. The first fifteen books were published by Matthaei at Moscow in 1808, but with the omission of all the passages taken from Galen, Dioscorides, and Rufus, as these were already to be found in the published editions of the works of those writers. As, however, the present editors have undertaken to publish the work of Oribasius as nearly as possible in the state in which he left it, all these passages have very properly been restored to their respective places, with the important exception of the whole of the eleventh, twelfth, and thirteenth books, which, being simply a transcript of the descriptive part of the Materia Medica of Dioscorides, are omitted altogether. Of the sixteenth book a short fragment is published for the first time.

With respect to the subject matter of these sixteen books, it may be enough to state generally that the first four treat of the different kinds of food; the fifth, of drinks; the sixth, of exercises and gymnastics; the seventh and eighth, of bloodletting, purging, and other evacuations; the ninth and tenth, of climate, localities, baths, and other external remedies; the eleventh, twelfth, and thirteenth, of materia medica; the fourteenth and fifteenth, of simple medicines and their properties; and the sixteenth, of compound medicines. The seventeenth, eighteenth, nineteenth, and twentieth books appear to be entirely lost. Sixteen chapters belonging to the twenty-first and twenty-second books (treating of general physiology, and of the function of generation,) are published for the first time. These are followed by forty-five chapters, which are evidently in confusion and out of their proper place and order, and which treat of hygiène, general pathology and symptomatology, and physiology. Dietz considered all these to belong to the twenty-first and twenty-second books, but the present editors (for reasons which appear to us to be sufficiently convincing), have preferred styling them extracts from *uncertain* books: they have never before been published. The twenty-third book is lost. The twenty-fourth and twenty-fifth books, treating of descriptive anatomy, had been published twice before, but are here reproduced in a corrected and somewhat enlarged form. One of the happiest discoveries made by the editors was the finding, at Heidelberg, a MS., which they have been able to prove to be the original from which all the other existing MSS. of these two books were transcribed.

All the books from the twenty-sixth to the forty-third (both inclusive), have disappeared. The forty-fourth book, which relates to abscesses and tumours, and which finishes the third volume of the new edition, had previously been published by Cardinal Mai in the fourth volume of his 'Classici Auctores,' &c., with the omission of all the extracts from Galen. It had also been reprinted as an inaugural dissertation for his medical degree at Groningen by Dr. Bussemaker. It is now reproduced in an enlarged and corrected form; indeed so much improved, that Dr. Bussemaker's former edition (though by no means a despicable performance,) is not even alluded to, as if it were quite unworthy of being kept in remembrance.

With respect to the remaining portion of the work, we will merely state that nothing is extant after the forty-ninth book, with the exception of a few fragments.

From the above remarks our readers will be able to form some idea of the extent to which the text has been enlarged. But it has also been improved by numberless verbal corrections, some derived from the collation of MSS., some from Galen's works, (in the passages taken from this author,) and some few from mere conjecture. In the more purely philological part of their work the editors have also had the assistance of M. Dübner, one of the most competent scholars now in Paris, so that every care has evidently been taken to render the execution of the work as perfect as possible.

The Greek text is accompanied by a French translation on the same page, beneath which are the various readings. Dr. Daremberg appears to have twice changed his opinion (or at least the plan of his work), with respect to the language in which his translation should be framed. In the "Prospectus and Specimen," published in 1847, he says (p. 35), that he at first intended to give a *French* translation to all the authors composing his 'Bibliothèque,' but that grave considerations induced him to alter his plan; and, in fact, the published Specimen has a *Latin* translation, and not a *French* one. The reasons for this change were (briefly) as follows:—His collection is intended not only for Frenchmen, but for the learned of all nations, and Latin is still the universal language of scholars: all the learned physicians and philologers whom Dr. Daremberg consulted on the subject, agreed in recommending a Latin translation in preference to a French one; several of the works to be comprised in the collection will have no real interest except for scholars, and, in fact, will hardly bear to be translated into any modern language;[*] it would have been impossible to find in France (or any other single country,) a sufficient number of persons able and willing to translate into the vernacular tongue all the works intended to form part of the 'Bibliothèque;' and, lastly, it was hoped that a separate translation of the principal authors or treatises might at some future time be published in French.

He was, however, induced to return to his original intention of giving a French translation, by the unanswerable argument, that neither of the Academies to which his plan was submitted approved of a Latin one, and that both the Minister of Public Instruction and the publisher declined to assist in publishing one (tome i. p. xlvi). Dr. Daremberg avows his own predilection in favour of translations in modern languages, on the grounds that these alone afford any real assistance in the explanation of difficult passages, and also that these alone "allow the translator to make no sort of compromise with the text" ("les seules qui permettent de ne faire aucune espèce de compromis avec le texte"), by which somewhat obscure and affected expression we imagine him to mean that it would be scarcely possible for a person who was translating Hippocrates into *English*, to shirk the difficulties he might meet with, which, if he were making a *Latin* version, he might easily do, and might make his reader (and perhaps himself also) believe, that, by putting down a Latin word in the place of each Greek word, he was really executing a translation of his author. Certainly, we would not be thought to undervalue modern translations, and it is impossible to deny, that, in many Latin versions, we are apt to be treated in the provoking manner just alluded to; but, nevertheless, we are inclined to agree with "the foreigners," who (as Dr. Daremberg allows)

[*] The force of this latter observation we have ourselves felt very strongly when looking at Dr. Adams's Hippocrates, Aretæus, and Paulus Ægineta; and in our notices of two of these works we have remarked upon the unfairly ridiculous appearance given to certain passages by means of the English translation. Dr. Daremberg instances (among other works,) the chapters and treatises relating to pharmacy, as being unfit to be given to the public in a French translation. May we be excused for quoting our own observations relative to these passages of Paulus Ægineta: "The general principles of therapeutics were almost as well understood by the ancients as by ourselves; and a treatise on that subject, written by a sensible man two thousand years ago, would bear reading very well even in the present day. But a work on pharmacy, or one containing many medical prescriptions, certainly labours under great disadvantages when exposed to the public in an English dress. Accordingly, when some of the members of the Sydenham Society, upon opening the pages of Paulus Ægineta, were inclined to shut the book with feelings of mingled ridicule and disgust, on account of the uncouth names they met with, and the strange substances recommended as medicines, they should have borne in mind, that it is only of late years that these and similar substances have been expunged from the materia medica of modern European nations; that several of even Sydenham's prescriptions read somewhat strangely in Swan's or Pechey's translation; and that even in the case of our own pharmacopœia, the plain homely English names of *coltsfoot*, *bearberry*, *dandelion*, *pennyroyal*, *pellitory*, *buckbean*, &c., have not quite so imposing and dignified a sound as their Latin synonymes." (Brit. and For. Med.-Chir. Review, vol. ii.)

"have made serious objections against a French translation," and, upon the whole, to regret that the published work does not in this respect agree with the Specimen. We should have preferred having the modern translations (English, French, or German,) *separate* from the Greek text; for probably the greater number of those who would use them might be willing to purchase one or two inexpensive volumes, (such as Dr. Daremberg's own 'Œuvres Choisies d'Hippocrate,) but would be deterred by the size and cost of five or six large tomes like his 'Oribasius.'

We do not feel competent to express any opinion as to the style of the translation, but will simply state, that, in those passages which we have examined, we have found it accurate, and (for a French work,) not unusually diffuse and wordy.

The notes relating to the text, which are as brief as possible, are placed at the bottom of each page; those which explain the subject-matter of the work are longer, and are placed at the end of the volume. We have been much struck by their copious but not ostentatious learning, and the editors seem *in general* to have avoided the excessive diffuseness which is often so fatal a temptation to a learned man writing notes in his own language.*

We think that some information should have been given about the authors quoted by Oribasius. For instance, in the seventh book (ch. 19, tome ii. p. 64) there is an extract on scarification, from a lost work by Apollonius. Now, it appears from Smith's 'Dictionary of Greek and Roman Biography,' that there were nearly twenty ancient physicians of this name, and a person might possibly like to know *which* is the individual here quoted. This not unreasonable curiosity might have been satisfied in a very short note, stating that from comparing this chapter with a passage in one of the author's other works,† it appears that Apollonius *Pergamenus* is the writer here meant. Probably, however, all information of this sort is reserved for the 'Index Historicus' at the end of the work, in which form the greatest amount of matter can be comprised in the smallest space.

We are glad to find that the editors propose to furnish each author with a complete set of indices. In the case of Oribasius, there will be five—viz., 1, verbal; 2, historical; 3, geographical; 4, pharmaceutical; and 5, general index of facts, opinions, &c.

We cannot help especially noticing the typographical beauty of the volumes, and their general accuracy, which reflects the highest credit on all the parties concerned. We were sorry to find that in the second and third volumes, in order to economize space, and thereby save expense, certain typographical alterations were made, which detract slightly from both the beauty and the utility of the work; but we are bound to add, that, if the first volume had appeared in the less expensive form, no one would have reason to be dissatisfied. We may mention that this country may claim some slight portion of *reflected* credit from the excellent typographical arrangement of the work, as Dr. Daremberg states (p. xliv.) that in this respect he took as his model the Oxford edition of Theophilus, 'De Corporis Humani Fabrica,' which was considered, by one of our predecessors, at the time of its appearance in 1842, to be "decidedly the best edition of an ancient medical author with which they were acquainted."‡

We wished to have said something about the medical opinions of Oribasius himself, but find it impossible to do so, for in a work which is confessedly a mere compilation, even if there are any original observations of the writer, there are no means of distinguishing them. Freind quotes (from lib. xxiv. c. 8, tome iii. p. 311) his description of the salivary glands, as being "either omitted by Galen, or lost together with some other of Galen's works."§ It now appears that the passage in question is taken from the tenth (inedited) book of Galen's work, 'De Anatomicis Administrationibus.' Sprengel‖ says that he himself states that he had dissected apes; but, upon referring to the passage referred to,¶ we find (by the help of a very complete table of parallel chapters, given in the new edition) that it is not Oribasius himself who is speaking, but Galen.**

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* The difference in this respect between Latin and French notes is illustrated by the editors themselves; for we find that the same amount of matter which is given in *six* lines in the "Specimen" (p. 68), takes up *eight* longer lines in a smaller type in the published work (tome i. p. 575).

† Eupor. i. 9, p. 578 C. ed. H. Steph.

‡ British and Foreign Medical Review, vol. xv. p. 441.

§ History of Physic, vol. i. p. 18.

‖ Hist. de la Méd., tome ii. p. 185, French translation.

¶ Lib. vii. c. 5, § 24, tome ii. p. 85.

** De Anat. Admin. iii. 9, tom. ii. 896, ed. Kühn.

Nevertheless, the works of Oribasius are some of the most valuable medical remains of antiquity, and will always be consulted by those who wish to know the opinions or mode of practice adopted by the early physicians; and while we think that Dr. Daremberg might have found a more popular writer for the commencement of his 'Bibliothèque,' we think he could hardly have chosen one better fitted for exhibiting the extent of his qualifications for the task he has undertaken.

REVIEW X.

Die Physiologie der Thymus Drüse in Gesundheit und Krankheit vom Standpunkte experimentellex Forschung und klinischen Expahrung. Von ALEXANDER FRIEDLEBEN, Dr. Med. zu Frankfurt A.M.

The Physiology of the Thymus Gland in Health and Disease, elucidated by Experimental Inquiry and Clinical Experience. By ALEXANDER FRIEDLEBEN, M.D.—*Frankfort*, 1858. Pp. 336.

IT is not often that we are called on to review a work which bears upon its surface the marks of such patient, intelligent, and earnest inquiry as the one before us. Without losing space in preamble, we proceed to lay an outline of it before our readers.

The first chapter treats of the structure and development of the thymus. In respect to the latter, the author differs from J. Simon and other observers, who describe the first rudiments of thymic structure as a straight tube, which subsequently throws out laterally numerous secondary cavities, "sessile follicles." The author finds the first trace of thymic structure to consist in a narrow streak of blastema lying in the common connective tissue which surrounds the origin of the great vessels. On all sides of this streak there shoot out wide roundish glandular vesicles, slightly notched, and having, even from the first, undulating surfaces. These appear between the seventh and eighth week. They gradually contract at their point of origin, and completely separate, so as to form closed vesicles, which in course of continuous growth present more and more a mulberry appearance, by fresh offsets from the walls of the vesicles. The completely closed vesicles are now only united with the primitive streak by a short tract of connective tissue. At a later period, when the glandular vesicles have still further multiplied, the primitive streak and the connecting tracts have only become longer and firmer, and show no trace of interior nuclei, or of a cavity. In fact, they change into mere connective tissue traversed by elastic fibres; the primitive streak becoming the median cord which serves as a support to the soft gland-tissue. The follicles filled, or over-filled, with secretion, seem at some points to rupture occasionally, and form communications with each other. It is not rare, according to the author, to see in the midst of a large vesicle, structures which appear to be separated vesicles undergoing regressive metamorphosis. They are either empty, or have some fatty contents. They shrink up, and at last are converted into the so-called compound concentric bodies of the thymus, figured by Vogel in the 'Icones Physiologicæ.' Still later they change into simple concentric bodies, and finally vanish. Friedleben is very certain that these bodies are not found specially in wasting thymuses; they are often very numerous in the embryonal period, and in that of early infancy. The secretion of the thymus, the contents of the gland vesicles, Dr. Friedleben describes much as others have done; but he has examined and substantiated a very important point which he thinks all previous authors, except Restelli, have overlooked. This relates to the discovery in the blood of the thymic vein of numerous round nuclei, precisely similar to those which exist in the thymic cavities. Such nuclei were nowhere found in the blood of the jugular vein. Friedleben considers therefore the discovery of Restelli, that the secretion of the thymus—i.e., of the shaped particles formed by it—passes unaltered into the venous current, to be completely confirmed. How this communication takes place between the gland cavities and the bloodvessels he does not decide, but thinks it is by rupture of the soft structureless membrane of the vesicles.

We have repeated this observation ourselves; but in neither of two instances could we completely satisfy our mind that the dotted corpuscles found in the thymic cavities were present in the vein-blood. The characteristic dotted aspect is not very apparent until the corpuscles are floated in water, and we strongly suspect the alkaline blood-serum would much modify the appearance. Moreover, red globules, by pressure, &c., come to assume an appearance not much different from that of the dotted corpuscles. In one observation of thymic vein-blood, these corpuscles certainly appeared to be present in small numbers; but after shrinking the red globules with salt solution, and again expanding them with water, the dotted globules were no longer to be discovered. We therefore suspend our judgment for the present, neither altogether denying nor corroborating Restelli's announcement.

In the lymphatic vessels of a calf's thymus no such nuclei were found. This latter observation is contradictory to the result of Hewson's experiment, which he describes as follows:

> "The chest of a calf being opened, a ligature was passed round the lower part of the thymus, and the parts contained within the ligature were taken out. On examining these very attentively, a great number of lymphatic vessels, containing a fluid almost similar to chyle, of a white colour, but not quite so opaque, were seen coming from every part of the gland."

This fluid being examined with a lens of 1-20 inch focus, appeared opaque; and like a drop of milk. But on diluting it with a few drops of the serum of human blood, or with solution of Glauber's salt, the same appearance was exhibited as was observed in examining the fluid found in the lymphatic glands—viz., a great number of small white solid particles, exactly resembling in size and shape the central particles in the vesicles of the blood, or such as are found in the fluid of the lymphatic glands. The contents of the same thymus examined in the same way were found exactly similar to the fluid from the lymphatics. This experiment of Hewson seems to be referred to by Dr. Bennet, who, noticing Mr. Simon's objection to the possibility of the corpuscles of the thymus finding their way into the lymphatic vessels by reason of the limitary membrane which surrounds and encloses them, says, "But that they do find their way into those vessels was shown by Hewson and Astley Cooper, who found them there." We do not find that the latter mentions the point; and with regard to Hewson's experiment, we cannot but think it likely that in tying his ligature round the thymus he ruptured its delicate vesicles, and caused extravasation of their contents into the surrounding areolar tissue, which then might be taken up by the lymphatics.

The old doctrine that the thymus possesses a central receptacular cavity, which has been maintained by many, is strongly opposed by the author. He scarcely seems to us to have comprehended accurately Mr. Simon's description, who does not make any mention of a central cavity, except the primary microscopic rudiment, which becomes quite merged and lost in the multitudinous over-crowding growth of gland vesicles.

Chapter II. introduces us to the growth of the thymus—a subject which our author has laboured at with rare diligence. During eight years he was engaged in collecting suitable materials. He objects to the conclusions of preceding observers, that they made no selection of their cases, so as to obtain proper data for comparison, but mingled together the well-nourished and the lean—those who had died suddenly and those who had sunk after long and even colliquative diseases. For his own table he selected only the cases of well-nourished individuals dying suddenly. He is not content with simply determining the absolute weight of the thymus, but ascertains also the relative weight of the gland compared with that of the body, and similarly the relative values of the liver and spleen. The following table gives a general view of the results of these inquiries:

Age.	Length. Millim.	Weight absol. Grains.	Sp. gr.	Exponent of the weight of the body and of		
				Thymus.	Liver.	Spleen.
3rd to 5th month of pregnancy,	13·	5·1	1·112	903·53	19·3	1258·49
6th to 7th " "	22·6	38·8	1·075	355·55	20·99	818·0
8th " "	47·5	134·7	1·093	281·32	—	—
At full period	57·3	229·5	1·055	233· 1	21·40	311·08
Birth to 9th month of life	59·1	330·8	1·072	289·81	25·57	232·64
9th month to 2nd year	69·6	436·8	1·110	337·44	19·06(?)	178·54(?)
3rd to 14th year	84·4	432·0	1·047(?)	724·34	26·53	287·66
15th to 25th year	106·6	353·5	1·032	—	—	—
26th to 35th year	99·5	49·6	1·026	—	—	—
36th to 74th year	128·6	214·7	—	—	—	—

From these data he deduces the following conclusions:

1. From its first appearance in the embryo to the twenty-fifth year of life, the thymus constantly increases in length; subsequently it diminishes.

2. The absolute weight of the thymus increases without interruption all through the fœtal period to the end of the second year of life.

3. The specific weight of the thymus is higher in the first half of pregnancy, sinks gradually to the period of maturity, and after birth rises again to the second year; thenceforward it steadily decreases.

4. During pregnancy, the thymus enlarges nearly four times faster than the body, but from the full period up to puberty, the growth of the body preponderates over that of the thymus, so that in the age of boyhood, the increase in weight of the gland relatively to that of the body is not more than one-third.

5. The relative growth of the liver is greatest in the first half of pregnancy, sinks during the (rest of) embryonic independent life till puberty, but only in a limited extent.

6. The relative growth of the spleen, on the contrary, is lowest in the first half of fœtal life, and increases from thence till puberty, with slight variation. Thus the thymus and the spleen preserve a kind of alternation in their development, the relative weight of the one going up as that of the other goes down. Friedleben thinks this is significant of a connexion between these two organs.

7. The individual differences in measurement, as well as in weight, of the thymus, are so great, that it is impossible to determine normal maxima and minima.

8. The time of the greatest secretory activity of the thymus occurs in the second half-year of independent life; after this, it slowly lessens.

9. As its secretory function diminishes, the connective tissue of the thymus increases correspondingly with the growth of the body. This induces the involution of the organ.

These conclusions are in the main confirmative of those arrived at by Mr. Simon and several others, though they also go beyond and extend them in several interesting particulars. We confess, however, to a feeling of preference for the opinion of the observer just named, because, while expressed in terms less exact, it is for that very reason more true. Remarking on the various circumstances that influence development, he says:

> "These various influences do not admit of being weighed in the balance, nor can the vital maturity which they develop be measured by the vibrations of the pendulum. And if in assigning the highest development of the thymus to the *age of early growth*, I use an expression that is chargeable with some vagueness, I would observe that on that very account it is better adapted to our purpose."

Chapter III. examines the normal mode of disappearance of the thymus. Friedleben lays stress on the different conditions of the gland in atrophy from disease, and in its natural involution. He says they are not only physiologically, but morphologically, unlike. The normal involution of the thymus, according to him, proceeds from increasing development of its connective tissue, which causes a shrinking and retraction of the glandular structure by its pressure. At the same time fat-cells form in the increasing fibrous woof, while the contents of the gland cavities degenerate fattily. The author gives figures to show how the amount of fat in the thymus increases from 1·4 per cent. in a five months' old suckling, to 7 per cent. at the age of puberty, and subsequently even to 48 per cent. We entertain considerable doubts whether his view respecting the compression caused by the increasing fibrous tissue is correct. Our own examinations have not shown us any such notable amount of fibrous tissue, except in one case, where a man, aged thirty, had a thymus weighing 537 grains, in which the gland tissue appeared to be replaced to a great extent by fibroid stroma. The man died of extensive paralysis, without discoverable organic change in the nervous centres. The author observes that all parts of the thymus do not atrophy in a uniform simultaneous manner. As a rule, the lowest parts, the furthest removed from the chief trunks of the nutrient vessels, are the first to waste and disappear. He has discovered that the arteries contract and become obliterated, while the veins enlarge and become wider with varicose dilatations in the smallest branches. He thinks the venous enlargement must lead to a gradual obliteration of the smaller vein branches, a conclusion in which we cannot very readily follow him. Nor do we feel much encouraged to put implicit faith in the preceding statements when he adds, that in a woman, aged forty-five, not even the larger venous trunks of the thymus could be found, though the flattened organ was covered over with an abundant capillary network, which he supposes opened into collateral veins. Surely the development of collateral channels (he states before that the same thing occurs with the obliterated arteries) is not much like an organ undergoing wasting. He has further examined the condition of the sympathetic nerves that accompany the bloodvessels, and finds that they undergo degeneration after the attainment of adult age. Extirpation of the lowest cervical and superior thoracic ganglia gave rise to degeneration of the vaso-motor nerves on the left side of the thymus by the ninth day, as well as to hyperæmic injection of the gland in various spots, and to a relaxed and flabby state of its tissue, and a change of its shape. We must, lastly, take decided exception to the author's statement, that increased development of fibrous tissue takes place in all glands with advancing age. Some relative increase there may be as the proper tissue wastes, but scarce any otherwise. When such change does occur, it is a pathological process, quite independent of advancing age.

The fifth chapter relates to the chemistry of the thymus, which the author seems to have investigated very diligently. He strongly avers, in contradiction to Frerichs and Stadeler, that the *fresh* thymic tissue or its expressed fluid is constantly acid; and this, whether the gland belong to a fœtus or an independent being, to early or late life, or whatever the food may have been, or whatever illness the person may have died of. He also demonstrates the incorrectness of the view of these same inquirers as to the thymic tissue containing ammoniacal salts. In the thymus of the calf and ox he finds the following inorganic matters: Potash, soda, lime, magnesia, phosphoric acid, chlorine, sulphuric acid. The results of a quantitative examination of the earthy phosphates and alkaline salts at different ages are given in a table at p. 49, from which he deduces "the following highly interesting and novel conclusion," that from the period of embryonic life, especially after birth, the earthy phosphates are constantly increasing during the period of the animal's growth, and that the nearer it comes to the completion of its bodily development, and the time for the involution of the thymus, the more do these salts disappear and become replaced by alkaline salts. From $\frac{1}{15}$th of the amount of alkaline salts in the embryo, they mount up to $\frac{1}{2}$, and fall again to $\frac{1}{20}$th and even $\frac{1}{38}$th of the same. This their behaviour, is more remarkable because in two other organs (the spleen and liver) no similar increase and diminution is observed. In the spleen, indeed, there are notable variations of the amount of these salts, but in an opposite direction. In the liver they vary very little after the period of birth. The ashes of the blood-serum of the calf contain a somewhat higher per-centage of these salts than those of our blood-serum. During the suckling age of the calf, the proportion of potash to soda is 2 : 1; afterwards 1½ : 1. The change probably depends on the food containing an increased quantity of common salt. Further analyses of the ash of the thymus of oxen at periods of three weeks, twelve months, and fifteen months (vide

p. 52), show that the quantity of phosphoric acid varies from about six times to ten or sixteen times that of the chlorine, that there is more lime than magnesia (2½ to 6 times as much), and that the amount of sulphuric acid is considerable, contrasting strongly in this respect with the ashes of muscular fibre.

In all these points the author differs widely from Gorup-Besanez, which he ascribes to the circumstance that the latter worked with expressed juice only, while the author always used the whole gland smashed up. With advancing life the inorganic constituents of the thymus diminish, and also the water, so that the organic matters must be relatively increased. With respect to the organic constituents of the thymus, after all adhering areolar tissue had been dissected off as far as possible, and all the secretion expressed and removed, there remained a certain amount of a gelatine-yielding material. In a three-weeks calf this amounted to 2·547 per cent. of the fresh gland. In an eighteen-months ox to 3·030. No casein was detected, but albumen, amounting in a three-weeks-old calf to 12·294 per cent., and an eighteen-months-old ox to 11·555 per cent. of the fresh gland. Fat was present to the amount of 1·375 in a five-months embryo, in a three-weeks calf to the amount of 1·872, and in the eighteen-months-old ox to 16·807 per cent. of the fresh gland. Lactic acid was discovered in the fluid left after the coagulation of albumen to the amount of 0·156 in the three-weeks-old calf, and of 0·364 in the eighteen-months-old ox per cent. of fresh gland. All previous observers had agreed that sugar was not to be found in the thymus. Friedleben, however, has demonstrated its existence by using a considerable amount of material, and taking care to procure it in quite a fresh state. The tests employed were Trommer's fermentation and the production of alcohol .Its amount, on an average, in calves twenty days old, was 0·060 per cent., in eighteen-months-old oxen, 0·029 per cent. of thymus. With respect to hypoxanthin, leucin, acetic, succinic, and formic acids, Friedleben maintains that although they may certainly be obtained from the gland tissue, they cannot be considered as anything else than products of the chemical manipulation; they do not exist in the living body. We subjoin the analyses.

	Calf.		Ox.
Water	80·0	...	66·00
Albumen	12·5	...	11·60
Gluten	3·0	...	4·00
Sugar	0·1	...	0.03
Lactic Acid	0·2	...	0.42
Pigment	0·1	...	0·05
Fat	2·0	...	17·00
Salts	2·1	...	0·90
	100·	...	100·

Salts of the ashes:—

Sulphate of lime	1	...	1
Earthy phosphates	30	...	44
Alkaline phosphates	58	...	78
Chloride of Potassium	11	...	7
	100	...	100

Hypoxanthin, doubtful traces in both. Reaction of both acid.

From an inquiry into the chemical constitution of the human thymus at various ages, it results that the gland in general is subject to similar chemical alterations during the growth and development of the body, and during its own periods of evolution and involution, as the thymuses of calves and oxen.

The sixth chapter examines the influence of food upon the thymus. By three experiments, in which animals were deprived of food for a longer or shorter time, it is demonstrated that the thymus, from withdrawal of nutritive supplies, has the quantity of its secretion diminished; loses in breadth and thickness; nay, completely alters its shape by degrees; and, the longer the fast is prolonged, obtains a higher exponent as respects the weight of the body—i.e., suffers a disproportionately greater atrophy than the body itself. The liver meanwhile loses weight at nearly the same rate as the body, or even less rapidly; but the spleen experiences a disproportionately smaller loss in relative weight than the thymus. The secretion of the gland at the same time becomes altered morphologically and chemically.

From several experiments as to the effect of different kinds of food, it resulted: (1) That an abundant mixed nutriment, containing albuminous, fatty, and saline matters, increases the secretion of the thymus, and so makes the gland heavier. (2) That an exclusive fatty diet (with butter) relatively increases the fatty contents of the thymus, while the quantity of the microscopical-shaped constituents of the secretion diminishes; and that a continued administration of this aliment induces considerable wasting of the thymus. (3) That an exclusively starchy diet at first appears to increase the secretion of the thymus, as the gland becomes richer in water, swollen, and so appears relatively heavier than normally; but that at a later period the secretion in the gland ceases, so that it even completely atrophies, and approximates in its proportions of weight entirely to the condition which has been shown to exist in fasting animals; the liver and the spleen also behave nearly in the same manner as was then stated. The influence of appropriate food is well shown in a comparison of two children—one of whom, two months old, had thriven splendidly at the mother's breast, and had a thymus of 522 grains weight; while the other, five months old, fed with artificial food, and in a fairly good condition, had a thymus of only 237 grains. Both children died suddenly, both thymuses were rich in secretion, but the latter did not attain nearly the average weight of the gland in the suckling period.

The seventh chapter, respecting the influence of disease on the thymus, we must almost entirely pass over. The author shows that in acute diseases the absolute weight of the thymus, on an average, sinks 2·6 times, and the relative weight 4·1 times below the normal figure; while in chronic diseases its loss in absolute weight amounts in the mean to 6·5-fold, and in relative weight to 8·5-fold. In the chronic enteritis of suckling infants the atrophy of the thymus is most remarkable, its exponent of weight relative to that of the whole body being 2263·80, instead of the normal number, 289·81.

Passing over two short chapters (VIII. and IX.), on the effect of various operations on the thymus, and on the transit of drugs into it, we come to Chapter X. on the Comparative Anatomy of the Gland. The thymus, the author finds, is subjected in all mammalia to the same laws of growth and involution; the case of the hybernators forms no exception. In them, however, there occur a pair of peculiar glands (the so-called fat glands), which have much external resemblance with the thymus, and, like it, have no excretory duct, but are dissimilar in internal structure. They are made up of nucleated fatty cells, lying in a fibroid stroma. These have been confounded with the thymus—a mistake which we ourselves once committed. They are largest at the commencement of the winter sleep, and waste away towards its close. In *all* orders of reptiles, Friedleben believes it to be proved that a thymus is present, and that it is subject to the same laws of growth as the thymus of the higher vertebrata. In fishes he has made no examinations of his own, but on Ecker's and Leydig's authority believes it to be present in some.

Chapter XI. treats of the extirpation of the thymus, and its effects on the general system. The conclusions derivable from twenty experiments were: (1) That no animal died from conditions ascribable to the extirpation of the gland. (2) The removal of the spleen in young dogs has no injurious consequences to the life of the animal. (3) The simultaneous extirpation of the thymus and spleen, on the contrary, occasions a considerable depravation of the blood-making and formative processes, and leads at last to death from exhaustion. The weight of dogs whose thymus has been removed, compared with that of dogs in a normal state, shows a considerable excess of growth in the former; the animals being of the same litter, and the period of time of course being the same for both. A dog whose spleen had been removed also increased greatly in weight; five months after the operation he had gained 174·1 per cent. of his original weight. A dog who had lost both spleen and thymus, on the contrary, at the end of forty-seven days had only gained 22·1 per cent. of weight (while one *minus* his thymus alone had gained in the same time 243·9 per cent.), at the end of three months and a half his weight was 7·8 per cent. less than on the day of the operation. The comparative examination of the *blood* in the above animals gave some interesting results: (*a*) Relative to the blood cells. In a normal dog, the jugular-vein blood showed for every 1000 coloured cells, 7·38 colourless. In a dog deprived of his thymus, the proportion of the latter was 111·02; and in one deprived of his spleen, 151·11. Dr. Friedleben concludes that in the latter animals a part of the processes normally performed by the thymus and spleen, go on directly in the blood. (*b*) Relative to the solids of the blood. After extirpation of the spleen they were diminished 14 per cent.; after extirpation of the thymus, 16 per cent.; after extirpation of both organs, 20 per cent.; and after excision of a piece of the left vagus, as much as 95 per cent. This last result seems almost incredible.

The result of several experiments as to the amount of carbonic acid exhaled by animals deprived of their thymus, was that there occurred a diminution of 14 per cent. as compared with the exhalation of a normal animal of like age and weight. Observations made as to the quantity of urine passed compared with the weight of the animals and that of their food, supply the following conclusions:—After the extirpation of the thymus, the tissue metamorphosis of the animal is altered; a greater amount of nourishment is taken; the change of the same into blood constituents is accelerated; the blood crasis afterwards becomes more albuminous and watery; the excretion of albuminates increased, that of carbonic acid diminished; the excretion of water by perspiration greater, by urine less; the addition by growth absolutely greater, but relative to the amount of aliments taken, less than the normal figure.

Analyses of bones showed that extirpation of the thymus induced a more copious deposition of salts, or of gluten tissue, which varied according to the age of the animal, but corresponded to the existing phase of bone development, and was also proportionate to the existing development phase of the thymus itself; so that the alteration of the bones corresponded accurately to the grade of development and activity of the thymus at the time it was removed from the animal.

Chapter XII., containing ninety pages, is devoted to diseases of the thymus. We can only glance very cursorily indeed at the general conclusions. Inflammation of the thymus the author considers to be exceedingly rare, one single case alone he grants to be well "constatirt." Purulent depôts occasionally occur in the thymus, probably for the most part of syphilitic origin. Tuberculosis is one of the most frequent pathological changes in the thymus; it is, however, very necessary to be on one's guard not to confound common glandular tuberculosis with the much rarer thymic affection. Among 73 cases of general or lung- and bronchial-gland tuberculosis in children, the thymus was found by the author to be involved only three times. Indurations of the thymus have often been described, but only two the author believes to have really concerned the organ. Only 2 cases of carcinoma of the thymus have been recorded, and both are uncertain. The thymus may be compressed in cases of empyema. Of this the author gives three cases which came under his own observation; but as only the corresponding half of the gland was altered, we are rather disposed to think that the change was the result of inflammation, which had spread to and involved the tissue.

With respect to hypertrophy of the thymus, Friedleben can only find 11, or at the most 12 cases recorded, where it is certain this state existed. These he believes to have been instances of congenitally over-large glands, not of mere excessive distension by the secreted product. In the period of childhood, no case has yet been observed where the excessive size of the thymus has co-existed with pathological phenomena referable to this cause. The effects which would be produced by an over-large thymus, would of course be symptoms of pressure on the large venous trunks, or on the bronchi or trachea, abiding breathlessness, swelling and lividity of the face, paroxysms of suffocation. In 7 cases of enlarged thymus, the phenomena of laryngismus were present, and the children sank either in an attack of the same, or in consecutive eclampsia, or in a bronchitic complication. In 3 other cases, laryngismus was certainly absent, 1 died of bronchitis, another of croup, and the cause of death in the third is not mentioned.

The author now proposes the inquiry, whether the laryngismus in the cases in which it has occurred (seven-twelfths of the whole number), can be ascribed to the thymic hypertrophy. After a very full and interesting description of the disorder, and discussion of the nature of the paroxysm, the author considers the question above proposed under a variety of heads, taking into account all the possible influential conditions. This discussion extends to far too great a length for us to attempt to follow him, and we must therefore, as before, content ourselves with giving the ultimate conclusions. (1) That the thymus, neither in its normal non-enlarged state can hinder the respiration; (2) nor can it in either state disturb the circulation; (3) in neither state can it press on the respiratory nerve tracts; (4) consequently, neither in the normal nor hypertrophied condition can it interfere with the circulation of the brain, or with innervation of the muscles of the glottis; (5) neither in its normal nor hypertrophied condition is it capable of a periodic turgescence from blood-stasis. As a corollary from these, the author concludes that the thymus can never cause laryngismus, that there is no asthma thymicum.

The thirteenth and concluding Chapter opens with a series of thirty-three propositions, summing up the author's conclusions; after which he gives his final judgment, *that the thymus is an organ which during the growth of the body ministers to nutrition and blood-development, and therewith to building up of the tissues.* The remaining part of the chapter is occupied with a review of the opinions held by previous writers.

We have thus presented our readers with an analysis of the whole volume, which, indeed, appeared to us the only course that lay open to us. A work of so original a character cannot be fairly criticised in detail, except by those who have had opportunity and a call to labour in the same field. We have derived much pleasure and profit from the perusal of this masterly effort, which is in itself a monument of the author's diligence, patience, and acumen. We commend it most heartily to the notice of British physicians, and cannot doubt that a translation of it will soon make it as generally accessible as it ought to be.

Review XI.

1. *Practical Observations on the Operations for Strangulated Hernia.* By J. H. James, F.R.C.S., Consulting Surgeon to, and late Senior Surgeon of, the Devon and Exeter Hospital; Consulting Surgeon to the Exeter Dispensary.—*London*, 1859. pp. 95.
2. *Practical Observations on the Radical Cure of Inguinal Hernia.* By C. Holthouse, F.R.C.S., Surgeon to the Westminster Hospital, and Lecturer on Surgical Anatomy in its Medical School; Surgeon to the South London Ophthalmic Hospital, &c.—*London*, 1858. pp. 38.

Mr. James's name is a sufficient guarantee that what he thinks worthy of presentation to the public is the result of extended experience and unprejudiced observation, nor will this little work in any way damage the reputation which his former contributions to practical surgery have obtained. The basis of the treatise before us is statistical; it contains a table of all the operations for hernia which Mr. James has had the opportunity of performing, during an experience extending over thirty-seven years, the first operation recorded bearing date October 18th, 1821. The total number of these operations is thirty-six, and they are tabulated on the plan adopted in South's 'Chelius,' the fatal cases, however, being separated from those which recovered, and further, the deaths in private practice from those in hospital. The reason of the latter subdivision is one which applies strictly to country practice only—viz., that the distance of the patient's residence often forms an important feature in the case.

Besides these statistics of his own, Mr. James discusses those which are furnished in the periodical returns from all the metropolitan hospitals in the 'Medical Times and

Gazette.' Let us first turn our attention to this statistical view of the question. It is a startling fact, which, however, we can see no reason to doubt after the data given by Mr. James, that half, if not more, of all the cases of strangulated hernia operated on in London hospitals die. This is proved by the returns collected by the reporters of the 'Medical Times,' from accounts furnished by the officers of the various institutions. These statistics may, indeed, be so far considered imperfect, that cases are left in the reports "under treatment," the result of which is never recorded, but this defect of course operates to diminish the apparent number of deaths. These records for three years (apparently 1854, 1855, 1856, but Mr. James is not very precise upon this point) give a gross total of 391 cases, and 194 deaths from all causes. It is true that this gross total of deaths may be somewhat diminished by excluding cases in which death is produced by other diseases; still, by far the greater number died of causes connected either with the disease or the operation. Nor is the inference to be drawn from Mr. James's own tables a much more encouraging one; for here, notwithstanding the supposed favourable influence of country air, and although the list is composed partly of private patients who are likely to seek relief sooner than the persons who come into hospitals, the mortality is 14 out of 36. We do not see what objection can be urged against these statistics, or the conclusions that they enforce—viz., that the rate of mortality in operations for strangulated hernia is, in the long run, above one-third of the number of cases operated on. Nor will it be said at the present day, that much can be hoped from extended study of the disease, or further improvements in the operation. To the question of the supposed improvement effected in the latter direction, by the more extended introduction of Petit's mode of operating, Mr. James addresses himself with great effect, showing beyond question that, as far as our experience of the latter has gone, it seems to be quite as fatal as the old operation, considering that it is applicable only to the less formidable cases. This conclusion is also that of Mr. Prescott Hewett, from a comparison of 75 cases operated on at St. George's by the old method, with 69 cases reported by Mr. N. Ward from the London Hospital, in about one-half of which the sac was not opened:*

If, then, the pathology of the disease is sufficiently understood, and no great improvement can reasonably be hoped for in the operation, what resource have we left to diminish a mortality which is nothing less than appalling? Will the general resort to immediate operation effect this object? Doubtless, in some measure, but hardly to any great extent; for in the metropolitan hospitals, at any rate, the rule is universal to operate as early as possible. Perhaps, however, as the education both of patients and of their medical advisers progresses, they may be led to apply or to be sent to hospitals earlier, or in private practice operations may be advised sooner than they now are, and so some general improvement in the nature of cases operated on may be attained. Still, with the decisive evidence which has now been accumulated of the very serious risks attending the strangulation of a hernia, it is not a matter of wonder that the attention of surgeons has been very generally directed of late years to the various methods of obviating this risk altogether by the radical cure of the disease. Hitherto this radical cure has been applied only to inguinal herniæ; and it is the object of Mr. Holthouse's pamphlet to give a history and description of these methods, and to discriminate the kinds of inguinal hernia to which each method is applicable. It is in this latter feature that the originality of Mr. Holthouse's pamphlet consists; for in the description and history of the principal operation (Wutzer's), and in the supposed statistics of its results, his paper follows very closely, indeed is little more than a quotation from a similar paper by Mr. Spencer Wells, which appeared in the 'Dublin Quarterly Journal' for May, 1858. We would not be understood as charging Mr. Holthouse with plagiarism or unacknowledged borrowing; in fact, he points out expressly where he is quoting from Mr. Wells, but as this part of his paper is not new, we may fairly suppose that most of our readers are acquainted with the method of operating after Wutzer's plan, and the success which is said to attend it. As to the latter point we may be permitted, however, to say, that the so-called statistics of this operation seem to rest on no better ground than pure guesswork. Mr. Holthouse says:—

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* Medical Times and Gazette, vol. ii. p. 315. 1854.

> "Of Wützer's operation, we are informed by Mr. Spencer Wells, that Wützer himself had repeatedly practised it since the autumn of 1858—let us say, two hundred times. Professor Sigmund, of Vienna, according to the same authority, had done it nineteen times; Rothmund, of Munich, one thousand times; and it has been done by British surgeons at least fifty times. (p. 27.)

Looking to Mr. Wells's pamphlet we find that Wutzer confesses himself to have no idea how often he had operated; all that he says being, in a letter dated 1853: "Since the autumn of 1838, I have repeatedly practised my operation in the clinique every session;" while the incredible number attributed to Rothmund is thus arrived at: "Rothmund has done the operation about four hundred times in the Clinical Hospital at Munich, and he told me that he had done it much more frequently in private practice, so that he felt convinced he must have operated 1000 times."* We should like to have an estimate after this (and it would not be difficult to prepare it if this facile way of collecting facts be admitted) of the number of inguinal herniæ left uncured in Munich and its environs after this terrible onslaught. It is really a burlesque to call this sort of thing statistics; and we trust that in future medical authors will confine that much abused word to collections of facts noted at the time and properly authenticated. Excepting, however, these statistical extravagances, Mr. Holthouse's pamphlet appears to us of considerable practical use. It describes clearly and fully the methods at present in use: viz., Wutzer's, Riggs's, and Wood's.

It may be objected, however, that the modifications of Wutzer's cylinder by Rothmund and Spencer Wells are not sufficiently noticed. The object of all the operations appears to be identical: viz., to produce adhesion of the opposed surfaces of the inguinal canal; for although it would appear at first sight that the principal means of cure in Wutzer's operation is the plug which is invaginated into the canal, acting as a sort of permanent truss, yet it seems, from the experience of the German surgeons, that this plug may be afterwards absorbed, and yet the cure remain perfect; and Mr. Holthouse relates a case on page 22, where the scrotal plug slipped down again by the side of the cylinder, and yet sufficient inflammation was excited by the latter to close the canal by adhesion. Riggs's method consists in introducing a seton along the canal. Wood's is a rather complicated but very ingenious attempt to invaginate the superficial fascia in the inguinal canal, and at the same time to sew together the two pillars of the external abdominal ring. We must refer our readers to the pamphlet itself for Mr. Holthouse's classification of the cases to which each of these methods is applicable; we can only say that the proved risks of strangulated hernia justify any operation, not too dangerous, by which these risks may be averted, especially in patients whose station and habits of life expose them to violent exertion, while they prevent them from obtaining very perfect mechanical appliances. We believe, also, that the experience of these operations for the cure of inguinal hernia which English surgeons have had, justifies a very favourable opinion of them hitherto; but we protest against being asked for a verdict in their favour upon statements obviously exaggerated and inexact, and we are by no means satisfied either that all the mischief which has followed them has been carefully and truly recorded, or that the theoretical dangers of their performance (especially that of wounding the peritoneal sac) are not made too little of by their adherents.

Before leaving this statistical question we would wish to say a word on the subject of medical statistics generally. A great deal of confusion seems to prevail both on the nature of statistics and on the nature of the argument to be derived from them. Let us use Mr. James's and Mr. Holthouse's pamphlets to illustrate these two points:—

> "Statistics," as Mr. James says, "are valuable in proportion as the subject-matter is one capable of being ascertained with more or less certainty. For instance, a hernia being simply intestinal, or omento-intestinal, is a matter of fact, and so is the proportion of deaths

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* Spencer Wells on the Radical Cure of Reducible Inguinal Hernia, p. 18. Dublin, 1858.

in each. Again, the bowels having been moved shortly after the operation, or otherwise, is a matter of fact; so, frequently correct information can be obtained as to the extent and character of such evacuations. On such points as these we have good right to found an argument." (p. 68.)

We are quite willing to accept this definition of the province of statistics, which are indeed only records of facts, and which derive all their value from the truth and authority of the individual facts recorded. Every case, then, which is not recorded at the time, and on a regular plan, should be excluded from statistical tables; and no practice can be more dangerous than that which has lately come into vogue, and of which we gave an illustration above from Mr. Holthouse's paper, of founding statistical arguments upon hypothetical basis. It can never be too often repeated, that the essence of statistics is number, and the very idea of number implies accuracy. But when we have got our statistics, our collections of exact and recorded facts, what is the nature of the argument which, as Mr. James says, we have a right to found on them? Surely it is merely conjectural. The utmost that can be proved by statistics is, that two or more events followed each other in the order of time a certain given number of times. From this, when the number of times is great, a rational conjecture may be derived, that the subsequent event or events stand in the relation of effect to the primary event as cause; but it ought never to be forgotten, that this is a conjecture merely, one which very high numbers indeed may convert into a rational ground of action (as many or most of our grounds of action are conjectural), but which is liable to be overthrown at any time by *à priori* reasoning. The statistical argument, therefore, should always be held in subordination to physical research and experiment, and to the investigation of healthy and morbid processes. It is our duty and our high privilege to spend our lives in following the footsteps of nature, both in physiology and pathology, and we fail in that duty, and give up that privilege, if we consent to become mere collectors of facts and figures. In a word, statistics, like so many other subordinate matters, are good servants, but bad masters; they are admirable accessories when used as Mr. James uses them, to elucidate and fortify conclusions based on the only sound foundation of observation, experiment, and pathology; but they are miserably inadequate to supply the places of those laborious but necessary inductions to all medical conclusions.

These considerations, obvious as they are, may not be out of place at a time when men, both in medicine and in still more recondite sciences, seem to be getting weary of the sure but slow road of scientific advance, which proceeds by discovering and demonstrating the connexion between each fact and the one which preceded it, and are attempting to substitute the easy but most fallacious method of tables and averages. Thus, in Buckle's 'History of Civilization,' the physiological method is said to have vainly striven after the attainment of objects which have been immediately realized by the statistical; and we see frequent attempts made, by comparing the mortality of cases before and after the introduction of certain modes of treatment, to recommend or decry such treatment. Let it never be forgotten that the disease and its event are the extreme points of a chain, of which the intermediate links must be supplied by clinical and pathological observation, and that the innumerable fallacies introduced by omitting these links can but very imperfectly be obviated even by very high numbers.

The discussion of the statistical part of Mr. James's work led us to speak of the operations for radical cure of hernia, for which those statistics are to our mind the best justification; but statistics do not form the whole of that work. Mr. James discusses several of the points connected with strangulated hernia with considerable originality and effect. We would instance more particularly the taxis, of which he speaks more hopefully than it is the present fashion to do, and shows clearly the reason why almost every case of hernia reduced by the taxis recovers, and in what stages or condition of the disease this process may be expected to succeed. We remark, however, with surprise, that Mr. James makes no distinction between the various forms of hernia as to their fitness for the taxis. Now if we can at all trust our experience, we should say, that while there are very few cases of umbilical hernia in which the strangulated bowel may not be reduced by taxis (although most of them will still contain adherent omentum); and while many cases of inguinal hernia may be reduced, even after rather protracted strangulation, by careful manipulation, the proportion of strangulated femoral herniæ which are reducible is very much smaller, and that that proportion diminishes rapidly as the period of strangulation advances. If this fact be so, should it not have been mentioned, as having a very direct bearing on the question of operating at once, or trusting to a renewed attempt at taxis after some of the other usual means? We have a very strong impression that the rule ought to be, in strangulated femoral hernia, always to operate as soon as the first attempt at taxis has failed; while in the other forms a judicious delay may sometimes save the patient the risk of an operation. We think that Mr. James has rather overlooked this distinction, and has in consequence sanctioned, or appeared to sanction, in all cases, an expectant plan of treatment, which can only be occasionally justifiable.

On the operation external to the sac, Mr. James argues with much force, denying altogether its analogy with the taxis; and with unquestionable justice, as far as Petit's operation is concerned; but surely Gay's method, in which only a *subcutaneous* incision is made into Gimbernat's ligament, and the skin over the hernial tumour is not interfered with, presents more points of resemblance, and would appear *à priori* to be a simpler and less dangerous proceeding than the common operation external to the sac; although, perhaps, seldom practicable. Numerous practical points are put, and well put, in this little work, besides those we have alluded to; amongst others we would notice a simple but valuable suggestion in the reduction of herniæ, "which," says Mr. James, "I have often tried with much success. It is simply to turn the patient on the opposite side. It converts enemies into friends, for the bowels within no longer resist the ingress of those protruded, but rather draw them back."* Again, this observation on the deceptive sensation of the partial reduction of the tumour and its cause is important:—

> "The surgeon during the taxis may have reduced a portion of the tumour. He may think that he has reduced intestine or omentum, whereas it is neither the one nor the other; it is fluid. Intestine rarely goes up without a gurgling sound, omentum does; but it appears to me that it goes up in bulk, and rarely comes down again, while fluid is slowly emptied and presently returns. *The tenderness, pain, and hardness at the ring remain the same.* The hernia has not been reduced even in part. Whatever promises the tumour itself may hold out, as regards tenseness or tenderness, the state of the parts at the ring affords the only reliable indication." p. 23.

We could quote many other passages, were it necessary, to justify us in saying that this little book is more important than its size indicates; and that even on so well-worn a subject as hernia Mr. James has found the art of saying something original, and at the same time valuable. We will terminate this notice by quoting the minor points noticed in Mr. James's work.

> "1. That omento-intestinal herniæ are more fatal than any other class.
>
> "2. That as regards the stricture or strictures, analogies with other phenomena, especially those which are observed in paraphimosis, offer grounds for supposing an active constricting power.
>
> "3. That the peritonitis originating from the strangulation in hernia, differs in type from the idiopathic.
>
> "4. That the intestine above the stricture, dilated as it always is, may not unfrequently be the seat of injury, if a wound be inflicted on the bowel in the operation.
>
> "5. That the chronic character attributed to the strangulation when occurring in old people, rests upon very doubtful evidence.
>
> "6. That in addition to the general indications offered by the *fluid*, there may be these further conclusions afforded by it, or determined by the degree of constriction; namely, that when the fluid can be pressed into the abdomen, the degree is not extreme, and *vice versâ;* and again, where no fluid is found in the sac, it affords a probable indication of the degree of stricture being extreme.
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> "7. That inguinal hernia in the female, although sufficiently common, is rarely strangulated.

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* Note, p. 94.

"8. That in very large femoral hernia in the female, it is sometimes impossible to make out Poupart's ligament; and in these the diagnosis may be founded on a different principle." pp. 92–3.

We do not present these conclusions as indisputable—indeed, we had remarked several of them for notice and discussion, had space permitted. The second, for instance, appears to us excessively doubtful; and the analogy drawn between the stricture of a hernia formed by fibrous structures only and that of a paraphimosis formed by skin and its appendages, in which inorganic muscular fibre is known to exist in profusion, to be a false one; nor can we say that the difficulty spoken of in the last paragraph in the diagnosis of femoral hernia (and which we believe to apply equally to both sexes) appears to us satisfactorily explained in Mr. James's treatise. Still we can heartily commend this little book to our readers, and hope that Mr. James's example in turning a long experience to account may be extensively followed by our "practical surgeons."

REVIEW XII.

1. *Diseases of the Urinary Organs: A Compendium of their Diagnosis, Pathology, and Treatment.* By WILLIAM WALLACE MORLAND, M.D., &c. With Illustrations. —*Philadelphia*, 1858. 8vo. pp. 579.
2. *The Urine in Health and Disease; or a Simple Explanation of the Physical Properties, Composition, and Uses of the Urine, of the Kidneys, and of the Treatment of Urinary Diseases.* With Engravings. By ARTHUR HILL HASSALL, M.D., &c.—*London*, 1859. 8vo. pp. 90.

THE first of these works consists mainly of the substance of two essays, to which prizes were awarded by the Boylston Medical Committee in the years 1855 and 1857. It asserts but little claim to originality, professing chiefly to collect and condense the information to be found in the writings of the best authorities, so as to "constitute a convenient handbook for the large class of practitioners whose leisure does not allow them an extended examination of authors." Viewed in this light, it is creditable to the industry and intelligence of the writer; though we beg to be understood as saying this with a distinct reservation in respect to his talents for arrangement, of which more hereafter.

The book is divided into two parts, the first of which treats of "Diagnosis," and the second of "Pathology and Treatment." Any attempt on our part to give an abstract of what is itself a compendium, would be altogether unprofitable; our remarks on Dr. Morland's production will therefore be of a somewhat desultory kind, and if they be directed to what we consider defective rather than to what is fully and satisfactorily set forth, our author will not attribute this to any wish to disparage his labours, but, on the contrary, to a desire of suggesting what may render them still more useful in the future editions to which we hope his work may attain.

The first part does not altogether realize the expectations held out by its title; for although it contains much valuable matter relative to the symptoms of urinary diseases, it affords less than might be wished for in the way of comparison and contrast of symptoms with a view to the discrimination of such diseases—in other words, it is defective in regard to *diagnosis*, the very subject of which it professes to treat. It must also be confessed that there is a great want of distinctive character in the two principal divisions of the work: there is a great deal under the head of diagnosis which would be much more in its place under that of pathology, and *vice versâ*. It is remarkable that one of the most difficult points in the diagnosis of renal diseases is not alluded to at all, either by Dr. Morland, or by the majority of the writers on this subject—we mean, the discrimination between nephritic affections and obstruction of the duodenum, or upper portion of the small intestine, when accompanied, as sometimes happens, with complete suppression of urine. Pyrexia is common to both forms of disease, though generally marked by a harder pulse in the nephritic than in the intestinal affection: frequent and copious vomiting is nearly as characteristic of one as of the other. The seat and nature of the pain, its extension in the course of the ureters, and down the inside of the thigh, with the sensation of numbness in the latter part, and retraction of the testicle, are, indeed, symptoms which, in conjunction, will sufficiently indicate the kidney as the seat of disease; but pain alone, whether as to its position, kind, or degree, is in most cases too equivocal a symptom to be implicitly relied upon, and, in the one under consideration, does not always follow the course just described; neither is retraction of the testicle by any means an invariable occurrence. Again, when the urine, as in most cases, is merely scanty, it will still afford us sufficient ground of discrimination; but where it is entirely suppressed, we are deprived of the most certain, nay, the only certain means of diagnosis. We have met with several cases in which, from this cause, the diagnosis was at the commencement exceedingly obscure, though it soon became manifest in them all, that the alimentary, not the urinary, apparatus was the primary seat of disease.* It would be out of place here to enter further into this subject; we merely indicate it as one which ought not to be omitted in an exposition of the diagnosis of urinary diseases.

When speaking of the use of the *microscope* in the investigation of urinary deposits, Dr. Morland observes that—

> "Unless the physican be an *expert*, [a queer substantive this!] microscopic examination of the sediment in the urine should be entrusted to a professed analyser. The practising physician is at present very fortunate in being able to refer for information upon these essential points to adepts who can give an immediate and reliable reply." (p. 37.)

Here we differ entirely from our author. We should regard that physician as exceedingly *unhappy* who was obliged in such a matter to trust to any other eyes than his own; and we consider a sufficient acquaintance with the use of the microscope as one of the most essential qualifications of the practising physician. Indeed, we can find no sort of excuse for ignorance on this subject, except blindness or some other incapacitating affliction. It is impossible to overrate the importance of microscopic observation, whether to the scientific inquirer or to the actual practitioner in medicine, and as it has the singular advantage of combining a fascinating amusement with an indispensable study, there can be the less apology for neglecting it.

In the second part, on Pathology and Treatment, we have an excellent chapter on those affections of the supra-renal capsules which have lately attracted so much attention. We would willingly have given an extract from it, as a favourable specimen of our author's manner, but to quote it partially would scarcely be doing it justice; we therefore recommend it entire to the attention of the reader.

The chapter on Diseases of the Kidneys is a long and elaborate one. The various subjects it embraces are discussed under the heads of—Nephritis, including acute and chronic desquamative nephritis—Waxy degeneration of the kidney—Non-desquamative disease of the kidney—Fatty degeneration—Suppurative nephritis—Nephritis from retention of urine—Pyelitis—Nephritis from renal calculi—Tubercular or scrofulous disease—Cancer of the kidney—Hæmaturia.

These important topics are, for the most part, judiciously descanted upon. Our author's remarks on the complications and secondary diseases connected with nephritis are, however, far from satisfactory. At p. 42 he tells us that "inflammations of the serous membranes are, if we except, perhaps, the cerebral disorders, the most common and disastrous of the secondary diseases." It is extraordinary that while thus giving to inflammation of the serous membranes a prominence which does not belong to it, he makes no allusion to inflammation of the mucous membranes, nor to those effusions into the submucous areolar tissue which form so peculiar and characteristic a feature of the disease. Of bronchitis, which is so remarkably frequent a complication, he merely says: "Bronchial irritation, or troublesome and long-continued bronchitis, with extreme dyspnœa, are sometimes observed in connexion with chronic nephritis." (p. 194.)

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* Dr. Barlow, who has judiciously noticed this obscure point of diagnosis, says that in the case of obstruction in the duodenum or upper part of the small intestine, "we may have no urine at all for three or four days;" adding, that, "this long-continued suppression should, however, incline us to look for its source rather in the intestines than in the kidneys."—Manual of Practice of Medicine, p. 474.

When treating of *hæmaturia*, Dr. Morland appears to attach too little importance to the prostate gland as a source of hæmorrhage. Thus he says, at p. 77, "In simple or in cancerous ulceration of the bladder, or in fungoid disease of the prostate gland, blood often appears in the urine;" and at p. 271, "Bloody urine is also a frequent accompaniment of enlarged prostate. The bleeding may be simple, or provoked by some instrument." But in point of fact some of the most profuse hæmorrhages which take place from the urinary organs have their source from a diseased prostate. Such hæmorrhage may take place where there is extensive ulceration, or small ulceration, or even where there is no ulceration at all, from simple rupture of the vessels: the introduction of instruments is, of course, one of the most frequent of the immediate exciting causes in such cases.

The chapter on Diseases of the Bladder, which is the longest in the book, contains a very large amount of useful information, and, taken in connexion with the corresponding chapter in the first division, leaves little to be wished for except better arrangement; but the general remark which we have made concerning the want of distinctive character in the two divisions of the work applies here in full force. To enumerate merely the subjects treated of in this chapter would answer no useful purpose, while, on the other hand, our space does not permit, nor does the character of the work under review require that we should enter into them in any detail; we therefore confine ourselves to two or three passing comments.

Our author has introduced some remarks on *varix* of the bladder—a rare affection, of which the reality has been disputed. We extract entire the passage relating to it:

"Vesical varix is very difficult of recognition. Shaw, Civiale and others deny its existence at the vesical neck; there are instances which prove its occasional occurrence there. Its chief signs are hæmaturia, dysuria, and ischuria. These are so frequent in other affections, that the diagnosis is embarrassing. If there be hæmorrhoids or varicose veins elsewhere, the above symptoms would lead us to suspect their existence in the bladder. The following case* illustrates the serious nature of this affection. A patient under the care of Professor Laugier, at L'Hôtel Dieu, and who had periostal exostosis of the femur, after being in the ward a few days, was attacked with profuse hæmaturia. The blood was black and pure, not mixed with urine; the bladder became greatly distended, and was felt very high up in the abdomen. The hæmorrhage evidently took place by regurgitation; no lesion could be detected by catheter or sound; no tumour or fungous growth was found in the prostatic region. There had been previous pyelitis, announced by sharp pain in the situation of the kidneys, and a degree of paraplegia; these symptoms had not wholly ceased. The discharge of blood from the urethra persisted, with short intervals, and at times was very abundant; cold applications and various other means perseveringly employed were of no avail. The patient died gradually worn out. The *post-mortem* examination disclosed *voluminous varices upon the neck of the bladder*; one was ulcerated and largely opened; by this gaping wound the blood had issued. The powerlessness of art in such a case is evident. The reporter remarks: 'Bonet, Morgagni, Chopart, Desault, and others, mention instances. Other lesions, however, were co-existent, which tended to produce or maintain the varicose condition. In most of the cases related by them, vesical calculi or prostatic tumours were conjoined. Desault and Chopart report cases where no foreign body was found. These were observed in persons who returned to France from the Antilles, affected with varicose veins.' The abuse of excitants, so common in many warm countries, and various excesses producing afflux of blood to the genito-urinary apparatus, or its *stasis* therein, contribute to this condition of the vessels. In the case above related, the varices were uncomplicated with foreign bodies, tumours, &c., nor had climatic influences been brought to bear upon the constitution. 'On consevra, aisément, en presence d'une semblable lésion, combien il devait être *difficile de potter un diagnostique précis et rigoreux*, et combien surtout la therapeutique etait désarmée.' (*Loc. sup. cit.*) Dysuria and ischuria are quite constant in vesical varix; a dark-coloured blood is of a certain diagnostic value, although seen in other affections."

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* Gazette des Hôpitaux, July, 1854.

A note in the appendix contains a case of *vesico-intestinal fistula*, which was probably caused by ulcerative inflammation of a vesical varix, which had first contracted adhesions, and had then become the seat of perforation. The case is given on the authority of Dr. Sturm.

We have made the foregoing extract, partly as affording instances of a rare pathological occurrence, partly to give an example of our author's manner of treating a subject; but perhaps as much as either, with a view of justifying our own remarks on the singular want of method which this book exhibits. In the passage just quoted we have diagnostic signs, pathological description, necroscopic observations, etiological conjectures, and a declaration of the powerlessness of art, in relation to treatment: yet all this is in the part of the work which professes to be devoted to *diagnosis!*

Under the head of *Rupture of the Bladder*, Dr. Morland observes that—

> "The accident is infrequent in children and females. In the latter, because the bladder, when full, occupies less of the more capacious pelvis, and they are also somewhat less exposed to the causes of the injury." (pp. 356–7.)

This, as regards females, is a very unadvised remark. Women, *during parturition*, are peculiarly exposed to the causes of the injury, and if it happen but rarely under such circumstances, this is probably because obstetricians use every precaution against it, by attending carefully to the state of the bladder. Many cases of rupture of the bladder during labour are nevertheless upon record, and there is one in which there does not appear to have been any neglect on the part of the medical attendant, nor any particular condition by which the occurrence could be accounted for.*

On the subject of *Worms* in the bladder, ureter, and kidney, our author has brought together the greater part of the information that is to be found in medical writings.

The chapter on "Diseases of the Urethra," when duly collated with the corresponding one in the first division of the work, will be found to yield very full and accurate information; but here, as elsewhere, the task of comparing, disentangling, and combining, which devolves upon the reader, ought to have been performed by the author. In respect to *spasmodic stricture*, Dr. Morland notices the observation of Sir B. Brodie, that an intermittent character amenable to quinine sometimes attaches to it; but he omits to mention, among the symptoms of *permanent stricture*, those paroxysms of intermittent fever which are occasionally met with, and which, it may be added, are not under the control of quinine.

It should be mentioned, in respect to the chapters on diseases of the bladder and urethra, that the strictly surgical departments of these subjects—as lithotomy, lithotrity, and the introduction of instruments in the treatment of stricture, &c.—have received from our author a full and judicious consideration, and afford useful guidance to the practitioner of surgery. It would of course be superfluous for us to enter into these matters on the present occasion.

Notwithstanding the defects which we have pointed out in Dr. Morland's book, we are by no means disposed to deny its merit or its utility. Its great fault is want of method, and this certainly prevails to an extreme, we had almost said a distressing, degree. For our own part, we can see no advantage arising from the entire disjunction of the diagnostic from the pathological department of the work. Since, however, the author has preferred such a distribution, he should at least have kept to it, and not allowed pathology, semeiotics, and treatment to trespass upon each other's ground till the whole field is covered with confusion. It is true that this evil is in some degree counteracted, and the work rendered more available for reference, by an analytical table of contents and a copious index. Still, if the author desire to render his book as useful as it ought to be and might be, he must entirely remodel it for a future edition. There is abundance of good matter, which only wants to be reduced to good order to form an extremely valuable manual. The style in which the book is written is rather cramped, and in many parts laconic to a degree incompatible with elegance; but these are faults easily forgiven in a strictly practical work, intended to convey as much information as possible within a moderate compass. It abounds also in peculiarities of diction which sound strange to Cis-Atlantic ears; but to these, we suppose, we must not take exception, lest we become involved in philological controversy. The book is very free from typographical errors, and the illustrations, though not of first-rate excellence, are sufficient for their purpose. Taken as a whole, we can recommend Dr. Morland's Compendium as a very desirable addition to the library of every medical or surgical practitioner.

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* The case is related by Mr. Bedingfield: Lancet, June, 1837.

Dr. Hassall's publication is in all respects of a very different character from the preceding, though we have associated the two in this article because both have relation to urinary disorders. The author states in his preface that

> "The object of this little work is to afford an explanation, as simple as possible, free from all unnecessary detail and complication, of the physical properties, composition, and uses of the urine; of the functions of the kidneys; and more especially of the principles of treatment of the chief urinary disorders."

And he hopes that, by the assistance here rendered,

> "The student and practitioner will be enabled, with little application or chemical knowledge, to become acquainted with the chief facts, scientific and practical, connected with the urine."

He considers this as an object of some importance, because

> "It is certain that the great majority of medical men neglect to acquaint themselves with this class of diseases, being frequently deterred by the elaborate manner in which the subject is treated, and especially by the extent of the chemical inquiries and reasonings intermixed with it."

In short, Dr. Hassall seems to think that the greater part of the profession, knowing little on this subject, and not aspiring to any profound acquaintance with it, will be glad to take up with just so much information as may carry them through the routine of practice without loss of credit; and this modicum of learning he proposes to impart to them in the small volume now before us. We are of opinion that Dr. Hassall here makes a very erroneous estimate both of the knowledge and of the aspirations of his professional brethren, and we believe there are few of them who do not know more than his book could teach them. Indeed, if it had been put forth merely as an outline, intended to convey to the mind of the young student some general notion of the matter, preliminary to the perusal of more comprehensive works, it must have been pronounced in many respects defective; but when it is gravely presented to a majority of the medical profession as a remedy for their alleged ignorance, we can only wonder at the assurance of the writer.

Let us look a little into this boak, which is to supply all that is needful to those who eschew application and chemistry. At p. 7 we have an analysis of the urine—namely, that of Berzelius, published between twenty and thirty years ago. On this Dr. Hassall remarks, that "the above analysis does not include all the substances contained even in healthy urine, some of the most important being omitted;" and he proceeds to enumerate several of these, as well as some others which are found only in morbid conditions. An analysis which professedly does *not* exhibit all the known constituents of the matter analysed, must be allowed to be rather an original and humorous conception. We might have hoped that the analysis of Berzelius, together with Dr. Hassall's codicil thereto, might have furnished us with a reasonably full enumeration of the constituents of the urine; but such hope would have been vain, for we still miss some eight or nine of the substances generally admitted by chemists of the present day as entering into the composition of this fluid.

Under the head of "Anatomy of the Kidney," we do not meet with enough to satisfy even an inquisitive general reader; yet, in the short space allotted to this jejune description, the writer contrives to find an opportunity of contradicting himself. At p. 13 he says,

> "By the disposition of the parts just described, the blood from which the excretion is formed is brought into close relationship with the true secretory structure of the kidneys—namely, the epithelial cells which line the tubules."

While, in the very next page, he tells us that,

> "It is very questionable whether the kidneys are to be regarded as true secretory organs, or whether they do anything more than separate from the blood certain salts and substances previously existing, ready formed, in that fluid."

At p. 41, Dr. Hassall states his conviction that structural diseases of the kidneys are more prevalent now than formerly, especially in cities; but the circumstances he adduces to account for the alleged fact seem altogether inadequate to that end.

> "The health of persons living in cities is, as a class, much deteriorated; and whatever tends to lower the health predisposes to organic disease, and especially of those organs whose functions, like those of the kidneys, are both important and active. The causes operating to the deterioration of health it is unnecessary to dwell upon at any length: they are, *insufficient air and exercise, impure air, unwholesome and adulterated food, late hours, unhealthy occupations, dissipation, and intemperance.*" (p. 42.)

While these may be admitted as reasons why renal, in common with other diseases, should be more frequent in cities than in the country, they fail entirely to account for their greater prevalence now than at any former period. The inhabitants of our cities in the present day are less intemperate than their forefathers; and many who formerly might have passed their lives almost without seeing the face of Nature, have now abundant opportunities, of which they are not slow to avail themselves, of making excursions by land and sea; add to which, that all the provisions of medical police are much more effectually carried out, and the means of personal cleanliness more liberally supplied. Unhealthy occupations and late hours have ever been a bane of large communities, but are not more so now than in times past—at least, certainly not the former; and with regard to adulterations of food, we suspect they have always been dangerously prevalent, though there has not always been a Hassall to bring them to light. On the whole, we think it probable that there was, in proportion to the population, quite as much renal disease, if not more, formerly than there is at present—though no doubt it was less frequently detected; for example, prior to Bright's discovery, how many cases of chronic disease of the kidney passed under the name of "decline," and "decay of nature," and other equally vague designations?

The following is an interesting case of albuminuria apparently unconnected with structural disease of the kidney:

> "I have now under my care a very remarkable case of this description—a youth, aged about eighteen years. To my own knowledge his urine has contained, for the last two years, such a large amount of albumen that, when boiled, it becomes white, and sometimes almost solid, like white of egg; and yet, with this persistent condition of the urine, and this enormous drain upon the system, in place of being stunted and contracted, he has grown up to be a well-developed and rather fine young man, although not, of course, robust and hardy, yet capable of considerable physical exertion. In this case the urine often smells of the food consumed; and, nearly throughout, it has preserved its normal specific gravity." (p. 61.)

If Dr. Hassall's enumeration of the constituents of the urine in health be, as we have seen, extremely defective, not less so is his description of the morbid deposits and products which are met with in the same fluid. In a work like that under review, omissions might be excused in respect to substances of infrequent occurrence, which had not been found to bear relation to any particular diseased states. But surely we should here have had some mention of *leucine* and *tyrosine*, in their connexion with typhus, the exanthemata, and hepatic disease. The curious discovery of the presence of *acetone* in the urine and the blood in cases of diabetes, should also have found a place. And why should *purpurine* or *urerythrine* have been excluded? It is not only a very common deposit, but has very interesting pathological relations, being never found in connexion with diseased kidney, but almost invariably in acute rheumatism, and sometimes in intermittent fever and affections of the brain, very largely also in pericarditis, and most abundantly of all in some organic affections of the liver.

In the concluding observations on the "Treatment and Diagnosis of Stone in the Bladder," the author expresses himself strongly in favour of attempts to dissolve the calculus, especially by injections thrown into the bladder; but his observations on the subject are far too general to be at all satisfactory, and he does not appear to speak as if from any experience of his own.

He adverts to the use of the microscope as a means of determining the composition of a calculus, while yet contained within the bladder.

> "In general, but few attempts are made by the surgeon to determine, either during the existence of a calculus, or prior to an operation, the chemical composition of the calculus, and yet the microscope affords a ready and satisfactory means by which this object may generally be accomplished. Thus, the composition of the stone may frequently be determined with considerable accuracy by ascertaining by means of a microscope, the ordinary deposit or deposits occurring in several consecutive samples of the same urine." (p. 81.)

This is so obvious a means of diagnosis, that few, we should suppose, omit to avail themselves of it; but Dr. Hassall does well to mention it, because, strange to say, it is not generally alluded to by surgical writers.

The style of this little work is in general rather good (though, by the way, the foregoing extract cannot be cited as an example), and there is a clearness and simplicity in the author's manner of conveying information which make us regret that he has not more satisfactory information to convey.

The work is illustrated with twenty-four plates, affording microscopic views of crystalline and other urinary deposits. These have already appeared in the 'Lancet,' but here present an improved aspect from being on better paper. They are well executed, and give upon the whole very faithful representations.

In conclusion, we do not think that "the great majority of medical men" will feel themselves either much flattered or much enlightened by the perusal of this book. After all, *is* it for medical men that the book is chiefly intended? We confess we have a misgiving on this head. It seems strange that any writer should attempt to commend himself to the attention of a highly informed body of men by an unceremonious announcement of their ignorance; and it seems equally strange that he should endeavour to dissipate such presumed ignorance by so bald and meagre a production, destitute even of some of the commonest points of information. Our doubts are not dispelled by a certain section on "Spermatorrhœa," which looks as if it were addressed quite as much to patients as to doctors. We may be wrong, however, and we sincerely hope that the suspicion which has here forced itself upon us may be without real foundation. The public and the profession are much indebted to Dr. Hassall for his able and useful exertions in one important field of inquiry—the detection of adulterations in food and drugs. We do not think he has been equally successful in his dealings with urinary disease; and assuredly the present *brochure* is directly calculated to diminish any reputation which he may have acquired in this department.

Review XIII.

Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. Vol. XLI., 1858. 8vo. pp. 468.

This volume is nearly double the size of its predecessor; and in the quality of its matter it is inferior to none hitherto published by this Society. It contains twenty-one papers, all of them extremely interesting and original. Seven communications are devoted to Surgery and Surgical Obstetrics; Morbid Anatomy and Pathology are represented by a similar number; Teratology and Development are illustrated by four, and Therapeutics by three. But following, as is our custom, the order in which the papers are arranged in the volume, we proceed to give a short analysis of each.

I. *An Account of a Case of Arterio-venous Aneurism of the Temporal Vessels, which was treated by Ligature of both the Artery and Vein.* By Charles H. Moore, Esq.—The subject of this case was a labourer, sixty years of age, in whom a direct communication had existed for thirty-six years between the temporal artery and vein. The wound was originally made by a surgeon in opening the temporal artery. An abscess ultimately formed in the diseased parts, and the spontaneous bursting of this abscess was followed by such dangerous hæmorrhage that active surgical interference became at once necessary. The operation consisted in tying, first, the vein on the cardiac side of its communication with the temporal artery, behind and a little deeper than the ramus of the jaw, a little below the external meatus of the ear; secondly, the temporal artery, displaced by tortuosity, was tied near the posterior edge of the masseter muscle. The pulsation in the vessels in the temporal region and in the vein previously tied at once ceased. The jugular veins also at once diminished in size, and a pulsation and a thrill previously obvious in them could no longer be detected. With judicious after treatment the case progressed favourably: the discharge from the abscess rapidly diminished, and the ligatures separated on the tenth and thirteenth days; the ulcers assumed a healthy appearance, all œdema of the scalp disappeared; and when the patient left the hospital no pulsation existed in any of the diseased temporal vessels.

II. *Case of Communication with the Stomach through the Abdominal Parietes, produced by Ulceration from External Pressure; with Observations on the Cases of Gastro-Cutaneous Fistulæ already recorded.* By Charles Murchison, M.D.—This is an interesting and valuable paper, written in a clear and pleasant style. Were it nothing more than an account of the vagaries of a hysterical female, it would still be a most important addition to medical literature. We have here a remarkable instance of that morbid mental condition where misdirected volition led the unfortunate sufferer to inflict the most serious and unnatural injuries upon her person. Her purposes of partial self-destruction she patiently accomplished by the most persistent and prolonged methods of injury, designed with peculiar but characteristic cunning, and carried into effect with no less characteristic obstinacy. The case, however, is related and connected with 24 other cases of gastro-cutaneous fistulæ in a tabular statement, embracing the following considerations: 1. The causes of such morbid communications; 2. The situation, size, and character of the external opening; 3. How the food swallowed by the mouth escaped through the unnatural aperture; 4. A statement as to the duration of such fistulæ and the possibility of curing them; 5. An account of the general health of those who have suffered from gastro-cutaneous fistulæ; and lastly, we have a notice of the contributions to our physiological knowledge regarding the stomach and its functions afforded by experiment and observation upon such cases.

III. and IV. *On the Influence of Liquor Potassæ and other Caustic Alkaline Solutions upon the Therapeutic Properties of Henbane, Belladonna, and Stramonium.* By Alfred B. Garrod, M.D., F.R.S.—Two important contributions to clinical therapeutics from the same pen are given under this title. No subject can be more important, and no topic shows more clearly how necessary it is that the physician must be a physiologist and a chemist, while the chemist must be at once a physiologist and a physician, in order that the sciences of chemistry and therapeutics shall continue to make mutual progress, and advance the science of medicine hand in hand together. We cannot too highly estimate the value of such clinical inquiries into the actions of drugs as are recorded in these papers; and we hope Dr. Garrod will continue to communicate such statements of accurate facts, which he may determine or collect regarding the action of individual remedies. The refinements of pharmaceutical operations, the less crude, more refined, more elaborate, and more scientific forms in which medicines are prepared for administration, render a study of the incompatible combinations in which drugs may be—nay, often are—administered, a subject of paramount importance to the physician. But we cannot allow that these observations of Dr. Garrod, however valuable, are perfectly original, for if the reader will turn to page 512 of Dr. Paris's 'Pharmacologia,'* in the middle of a very long tabular synopsis of substances usually considered as incompatible with the different articles of the 'Materia Medica,' he will find the following statements arranged under the following respective headings:

Substances.		Incompatibles.		Results.
Belladonna	. . .	Caustic alkaline	. . .	Decompose atropia
Hyoscyamus?	. . .	solutions.	. . .	and render it
Stramonium?	. . .		. . .	inert.

On the same page we are also directed to a foot-note, which communicates the fact, that

> "Brandes had detected a vegetable alkali in belladonna, which he termed 'atropina.' Succeeding chemists not being able to find this alkali, denied its existence, until Runge explained their failure by showing that a caustic alkali, which they had employed, even though weak, was capable of decomposing atropina. By substituting magnesia, he not only obtained it from belladonna, but from hyoscyamus and stramonium; and he has shown that the properties of these three bodies so resemble each other, as to make it highly probable that they contain one common principle of activity."

In justice to the memory of Dr. Paris, we consider it right to draw attention to these statements; seeing that the quotations from the 'Pharmacologia' of Dr. Paris contain the gist of Dr. Garrod's communication.

We concede to the latter physician, however, the merit, first, of bringing a most important subject prominently before the notice of the medical profession; second, of having made and detailed experiments which show the necessary ratio between the alkali and the various preparations of the plants within or beyond the limits of which the active principles of these preparations retain their physiological and therapeutic properties or become inert; thirdly, he has ascertained and directed attention to the time required for the complete decomposition of the active principles. The main fact—namely, the destructive influence of caustic alkalies on the active principle of henbane, hyoscyamus, and stramonium, must have been within the knowledge of the profession during the past *fifteen* years at least. Some veterans prescribe, and will doubless continue to write prescriptions regardless of the important investigations of chemistry and physiology, which contradict the experience and observation they have been accustomed to regard, and the theories they may have cherished, since their student days. To bring such conflicting classes of prescribers to terms, is the victory we hope to see achieved, and that by such important inquiries as those which Dr. Garrod has proposed for himself at the commencement of these papers. The deductions naturally drawn from Dr. Garrod's investigations are stated as follows in his own words:—

> "Neither liquor potassæ, nor any caustic fixed alkali, should be prescribed with tincture or extract of henbane, as the virtues of the latter drug are thereby completely neutralized.
>
> "But when it is desirable to administer an alkaline remedy with henbane, either a carbonate or bicarbonate should be selected, which would probably be equally efficacious upon the stomach, if such influence be required, and certainly as efficient in altering the condition of the urine, and the mucous membrane of the urinary passages. The same precautions should be observed with regard to belladonna and stramonium, if at any time prescribed in conjunction with alkalies."

In his second communication it is shown that the destructive influence of liquor potassæ is so great in its action on atropine, that less than twenty minims of the former are required to neutralize one grain of the active principle; while a single grain of pure potash will, even in dilute solutions, destroy an equal quantity of the alkaloid. Dr. Garrod then notices the preservative power of ammonia salts, and gives a statement as to the time required for complete decomposition of the active principle; concluding his paper with some interesting clinical illustrations of the influence of liquor potassæ in rendering henbane and belladonna inert.

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* Ninth edition, 1843.

V. *A Contribution to the Science of Teratology*, by HENRY R. SILVESTER, M.D., conveys a minute description of a remarkable instance of congenital malformation of the two upper extremities. The examples of malformations usually preserved in museums convey very imperfect notions of the nature of the various deformities of organized beings. For the most part such museum-preparations represent external form merely. As to the precise nature of the deviation from the normal state we are left altogether in the dark; because, for the most part, no dissection is made of the monstrosity. Descriptive and developmental anatomy, as applied to the science of teratology, is therefore at the present day very defective. The very careful and minutely-described dissection recorded in this paper is a valuable addition to the science of teratology—valuable especially as a case with which similar or analogous cases of deformity may be compared.

The conclusions to which Dr. Silvester arrives regarding the deformity he describes are:

1st. That it appears to have been the result of, first, the original malformation of the germ; second, the subsequent deformation of the embryo and fœtus by causes operating on its development; and, third, certain compensations and vital accommodations having a conservative tendency.

2nd. That the arrest of development reacts on various parts of the body, and particularly on such parts as have either a casual or a natural connexion with the original malformation.

3rd. That a law of compensation prevails during the growth of the body in monsters. This law is expressed by a certain tendency to render the parts as nearly normal as possible, and to make up by excessive formation for the defective development of an adjoining part.

4th. That the inquiry which Dr. Silvester has here instituted, confirms in some measure the opinions held by Vrolik, Müller, Bischoff, Stannius, and others, that the several parts of the body are formed and developed independently of one another.

5th. That the absence, arrest of development or defective condition of the radius (which appears to be the rule in congenital malformation of the human forearm), is not the normal state in beasts; the ulna being the bone which in them is atrophied.

6. "As the transient forms of the human fœtus are for the most part comparable to the persistent forms of the lower animals, the malformations occasioned by impeded development often acquire a brutelike appearance." (Vrolik.)

We here learn how the study of a careful and minute dissection of malformed parts tends to show, that a full-grown monster is no *lusus naturæ;* but, that its form is the result of certain definite laws, the tendency of which is to compel the tissues to rectify as far as possible the original defect in the germ, by means of a principle of vital accommodation and compensation.

VI. *On some Points in the Pathology and Morbid Anatomy of Glaucoma.* By J. W. HULKE, Esq.—In this disease, when all sight is lost, and when a hard and painful condition of the globe of one eye places the other in danger, it has lately become the practice with some ophthalmologists to excise the eyeball, a practice instituted for the first time in the Royal London Ophthalmic Hospital in the winter of 1854–55. Such an operation has afforded frequent opportunities for examining and dissecting eyes affected with glaucoma, directly after extirpation, and while the parts were still fresh. The results of such dissections are given by Mr. Hulke in this valuable communication, which also embraces a short record of the symptoms of the disease (but we regret to say, not in connexion with the dissections he records) of the ophthalmoscopic phenomena witnessed during life within the eye, and of the structural changes observed in the tissues immediately after removal from the orbit of the living patient.

The new observations recorded by Mr. Hulke in this communication, may be stated to consist in the detection, by the ophthalmoscope, of small hæmorrhagic spots upon the retina, with turgidity of the retinal veins, whilst the transparent media of the eye were still clear. The microscope afterwards detected these spots to consist of capillary hæmorrhagies, and proceeding from the capillary vessels in the inner layers of the retina, when the effused blood either spreads laterally among the elementary structures of the retina, or bursts through the hyaloid membrane into the vitreous humour. The retinal capillaries were irregularly dilated, and studded with small fusiform and globular enlargements—miniature aneurismal pouches.

Mr. Hulke has now and then also seen such pouches on small blood-vessels one remove from the size of capillaries, but never on the larger vessels or great trunks. These pouches, and the vessels communicating with them, are usually crammed full of blood-corpuscles; and in the hæmorrhagic spots the tissues are infiltrated with blood discs and stained with blood pigment. Otherwise, the retinal capillaries are healthy. We are not sure if we are warranted in concluding that Mr. Hulke considers such changes in the retinal vessels to be the earliest lesions in glaucoma, followed by those he describes in the vitreous humour; while the changes in the colour of the lens again follow the changes in the vitreous humour and retina. We suppose he believes such to be the order of events, because he writes:—"It is probable that they (the vitreous humour and the lens) derive their colour from the same source, namely, the hæmatine of the blood." If, therefore, the colour of the lens acknowledges such an origin, the hæmatine which colours it can only be derived from the ruptured capillaries of the retina discharging their contents of blood discs, which become entangled among the vitreous humour in a delicate web of coagulated fibrine. Withered blood discs he has also observed in all stages. Moreover, Mr. Hulke has also seen "small filmy blood clots imbedded in the vitreous humour, and tied by a slender point to the retina at the spot where the ruptured *membrana limitans* has allowed the blood to pass from the retina into the *corpus vitreum.*" Now, if the yellowish colour of the vitreous humour is thus derived from the colouring matter of the blood so effused, soaking through it and staining its substance, it is extremely probable that the lens is secondarily affected from the same source or cause, just as we see the nucleus of a microscopic cell coloured in a high degree, by all colouring matters which pass through the fluid contents of the cell to its interior. Indeed the crystalline lens, like the nucleus of the microscopic cell, seems to have a special power of attracting colouring matter when so circumstanced; and thus it is often found deeply coloured when little or no colour can be perceived by the naked eye to pervade the vitreous humour itself. Now, if the crystalline lens may be regarded in the relation of a nucleus imbedded in the hyaloid fossa of the vitreous humour, these observations Mr. Hulke has made afford to our mind a very satisfactory explanation of the cause of alteration in the colour of the lens, and of the period of time at which the change of colour most probably is established; the innermost portion of the lens and the lamellæ immediately posterior to it being sometimes so deeply coloured as to impair its transparency. Mr. Hulke, however, does not state these conclusions; and we also think his observations are consistent with the fact that the amaurotic element is not unfrequently the earliest indication of approaching glaucoma.

VII. *On the Analysis and Immediate Principles of Human Excrements in the Diseased State.* By W. Marcet, M.D., F.R.S.—It is proposed in this communication to show first, that there is an easy and very practical method of analysis applicable to fæces in the diseased condition, capable of affording assistance in the diagnosis of disorders of the digestive system. Secondly some practical and clinical illustrations are given of the results so obtained. Dr. Marcet founds his method of analysis upon the adoption of a process by which the fæcal evacuations are mechanically divided into their immediate principles, instead of being chemically decomposed. Proceeding in this way, Dr. Marcet "offers to the medical profession a method for the analysis of excrements in a state of disease calculated to yield results at least as important as those we have already derived from the analysis of urine."

We recommend our readers to study for themselves the processes and results as detailed in this important paper.

VIII. *On the Membrana Decidua which surrounds the Ovum in Cases of Tubal Gestation.* By Robert Lee, M.D., F.R.S.—The main object of this paper is to bring together the evidence which demonstrates the existence of a membrana decidua surrounding the ovum in cases of tubal gestation; a fact previously demonstrated by M. Chaussier in 1814, by Professor Schroeder Van der Kolk in 1857, visible in preparation No. 14, series xxxiii., in St. Bartholomew's Hospital Museum.

IX. *On the Action of Galvanism upon the Contractile Structure of the Gravid Uterus, and its Remedial Powers in Obstetric Practice.* By F. W. Mackenzie, M.D.—The results of some investigations undertaken for the purpose of determining the exact influence of galvanism upon the contractile structure of the gravid uterus are here recorded; the best method of applying it is also demonstrated; and the results are related which have followed its employment in certain cases attended with difficulty and danger. Hitherto investigators on this subject have not observed a proper distinction between the specific action of galvanism upon the uterus, and the action of other agencies collaterally in operation; so that in deducing their ultimate conclusions, they have undoubtedly in some instances mistaken the *post hoc* for the *propter hoc*.

In illustration of the first of the inquiries instituted by Dr. Mackenzie, he relates experiments performed on the gravid uterus of a pregnant bitch. In that animal he exposed the organ, so as to ascertain by visual and tractile examination the effects of galvanism upon it. He thus found that the structure of the gravid uterus undoubtedly responded to the stimulus of galvanism in a peculiar and remarkable manner; that the action thus excited is a slow and vermicular-like contraction of the organ limited to the portion included in the current when the galvanism is applied locally, and affecting it more generally and more powerfully when it is applied through the medium of the spinal portion of the nervous system. The contraction of the uterus under the stimulus of galvanism is shown to differ most widely from that of the voluntary and other involuntary muscles when acted upon by the same agent; inasmuch as it is more slow in its development, more sustained in its duration, and more gradual in its subsidence, approximating, by these conditions, to the contraction of the uterus during labour.

Dr. Mackenzie shows that the most effectual mode of employing galvanism so as to direct its influence upon the gravid uterus in the human female is to direct the current in a longitudinal direction through the organ from the upper portion of the spinal chord; that is to say, by applying the positive pole of the machine to the upper part of the spine, at the nape of the neck, and the negative pole to the cervix uteri, by means of a vaginal conductor. It is necessary that the current be sustained continuously until the desired result is obtained; individual galvanic shocks exercise little or no specific influence in exciting uterine contraction. It is further stated, that in proportion as the constitutional powers fail, so is the influence of galvanism less powerfully exercised upon the uterus, until at length, with increasing exhaustion, it ceases to exert any influence whatever.

Some interesting clinical evidence is adduced, demonstrating the usefulness of galvanism in obstetric practice, while the author cautiously and prudently states that galvanism must always be regarded as an agent to be used in exceptional cases, and only when the better recognised and more established rules of practice either fail altogether, or offer little prospect of doing good. Still, there are many cases in which it may be usefully resorted to, and some in which it seems to afford the best security to life. Of such cases are especially to be noticed the following: (1.) Cases of placental presentation, in which profuse hæmorrhage continues to recur, notwithstanding the employment of the plug and other means, before the os uteri is sufficiently dilated to admit of manual assistance. (2.) Cases of hæmorrhage in the early months of pregnancy, which resist the usual means employed for their suppression, and which, from the contracted state of the os and cervix uteri, do not admit either of mechanical or manual interference. The cases detailed appear to show that in galvanism we have a powerful and reliable means of moderating and controlling active hæmorrhage, and of simultaneously accelerating the dilatation of the os uteri and the general progress of the labour. It appears also that such a sustained current of electricity may also be continued for a lengthened period without any appreciable pain or inconvenience to the mother, or any danger to the child. Nay, it is said that the galvanism in one case revived the parturient female and gave her strength, while in another casenot only was no appreciable amount of pain felt, but there was no recurrence of those rhythmical pains which are so constantly experienced in ordinary labour.

X. *A Case of Complete Inversion of the Uterus, of nearly Twelve Years' Duration, Successfully Treated.* By W. Tyler Smith, M.D.—This case is brought forward to illustrate a new method of treating this formidable affection. In the vast majority of such cases, where the uterus remains unrelieved, the result is undoubtedly a fatal one; death following at periods varying from eight months to five years after the occurrence of inversion. Dr. Smith proposes to effect reduction by continuous pressure. In the case related he was successful, and he believes that by the use of air or fluid, pressure may be made of general application in the reduction of all chronic cases of inverted uterus.

In the case detailed, nothing could have been more rigid and unpromising than the os and cervix uteri; but in obedience to the force and stimulus exerted, the body of the uterus was converted into a wedge or tent, which by constant pressure dilated and developed the narrow rings which had so long fixed the uterus in its inverted position. Continuous pressure, directed upwards, was applied chiefly by the use of the air-pessary, which kept the vagina distended, and the hand was passed into the vagina night and morning to manipulate the uterus, by squeezing and moulding it for about ten minutes at a time. This combination of continuous pressure with alternate manipulation ultimately diminished the size of the body and fundus, and in this way promoted re-inversion and the arrest of hæmorrhage. Two years and a half after reduction Dr. Smith had the satisfaction to learn that his patient had become pregnant, and was delivered in due time of a living child. Some amount of flooding occurred after the labour, but no tendency to inversion was manifested.

XI. *On Excision of the Knee.* By George H. Humphry, Esq.—A brief account is here given of thirteen cases, in which Mr. Humphry excised the knee-joint; some remarks are also made upon the selection of those cases which are the best suited for the operation, and suggestions are offered upon the mode of performing it. As the operation of excising the knee-joint may still be said to be on its trial, this contribution is a very valuable one to the profession. It is very carefully written, and full of such practical and useful information which will not bear further condensation, that in place of giving an abstract of it, we heartily recommend it to the study of our surgical brethren. In doing so, we also express the hope that we shall see, ere long, similarly carefully-written statements emanating from other hospital surgeons. Thus only can the exact utility of the operation be determined, and its legitimate position in operative surgery clearly defined.

XII. *Contributions to the Etiology of Continued Fever; or an Investigation of various Causes which Influence the Prevalence and Mortality of its Different Forms.* By Charles Murchison, M.D.—This is perhaps the most elaborate paper contained in the present volume. It attempts to investigate the causes which influence the prevalence and mortality of continued fever, regarded as a single disease. It also examines the ascribed causes in reference to the different forms of fever, and the facts established, although they are not novel, nor sufficient to decide the disputed point as to the identity or non-identity of typhus and typhoid fever, yet they illuminate the subject with such a flood of light, that this contribution must ever be regarded as one of great value in recording the pathology of continued fever.

Dr. Murchison adopts the division of continued fever into four forms, namely: typhus, typhoid, relapsing fever, and febricula; and proceeds to state his results under the following heads: 1. The prevalence of continued fever in Great Britain and Ireland during the present century; attempting to ascertain which of the forms of fever mentioned have mainly composed the great epidemics of fever in this country. 2. The annual prevalence of the different forms of continued fever in London as compared with other towns. 3. The prevalence of the different forms of continued fever, according to the months and seasons. 4. The influence of sex upon the prevalence of fever. 5. The influence of age. 6. The predisposition to fever from occupation and station in life. 7. The localities in London in which each form of fever is most prevalent. 8. The influence by over-crowding in generating fever, with deficient ventilation, and destitution. 9. The influence of putrid emanations in generating fever from decomposing organic matter in drains, cesspools, churchyards, &c., and organic impurities in drinking water. 10. The influence of contagion in propagating fever. 11. The influence of recent residence and birthplace in determining attacks of fever.

Dr. Murchison next considers the various circumstances which influence the mortality from continued fever; and concludes his paper with the formal enunciation of twenty-five distinct propositions or conclusions, which he supposes are legitimately deduced from his previous inquiry.

We commend the paper to our readers.

XIII. *Case of Hydatids of the Tibia.* By WILLIAM COULSON, Esq.—This communication is interesting on account of the rarity of the affection. The disease occurred in a female twenty-five years of age, and was supposed to be the result of a kick from a horse. No inconvenience attended the gradual development of the swelling during a period of four years, but after that time the pain became very severe. The use of blisters had the effect of relieving it, but the swelling remained, and at last gave way spontaneously, when matter containing acephalocysts was discharged. The operative treatment then consisted in laying open the cavity in the tibia containing the hydatids. The whole of this cavity was found lined by a white glistening membrane; and it extended within half an inch of the knee-joint and three inches down the shaft. After the removal of all the hydatids the whole lining membrane was rubbed over with solid nitrate of silver, and the cavity filled with cotton wool. Two days afterwards the wool was removed, and the cavity washed out with a solution of chloride of soda, when several hydatids came away. The wound was thus daily dressed: and again, in four days, two other hydatids are reported to have come away. It was not till eleven days after the operation that healthy granulations were seen springing up from a great portion of the cavity; but at the upper part a piece of necrosed bone was detected, which ultimately became loose, and was removed with the forceps. It was found to be thickly covered on both sides with small hydatids. After the removal of this sequestrum the patient rapidly recovered.

XIV. *A Case of Fibrous Polypus of the Urinary Bladder, with Observations and a Table of the Recorded Cases.* By JOHN BIRKETT, Esq.—The disease here described by Mr. Birkett resembles in every anatomical particular the succulent fibrous growth, so well known by the name of the "nasal polypus," and it is principally on account of the rareness of this form of polypus in the urinary bladder, that the information given us by Mr. Birkett is most valuable. He briefly but clearly records the literature of the subject, and proceeds to relate his case. The subject of the disease was a girl five years of age, of a strumous habit, and in a most cachectic condition. Eight weeks before Mr. Birkett saw the girl, she complained of pain in the hypogastric region, followed by a diminution of the quantity of urine passed. Four weeks afterwards, muco-purulent discharge was noticed in the urine, and when seen by Mr. Birkett, the child was unable to pass urine voluntarily. After remaining in the hospital for some time, a dark red growth was seen to protrude between the vulva. She was placed under chloroform, and having emptied the urinary bladder, Mr. Birkett ascertained that the growth projected through the meatus urinarius, and was attached to the superior boundary of the neck of the bladder. The growth was found to be composed of lobes and lobules, was soft, but strong enough to resist ordinary manipulation; it was not very vascular, and resembled closely the firmer varieties of nasal polypus. The child died twenty-five days after admission. A full account of the necropsy is furnished by Dr. Wilks, with a minute account of the polypus, illustrated by an excellent drawing.

Mr. Birkett then sums up the differential diagnosis of such cases, and suggests methods of treatment.

XV. *Researches on Gout.* By Alfred B. Garrod, M.D., F.R.S.—This paper is divided into two parts; the first part is on the urine in the different forms of gout; the second part is on the influence of colchicum upon the urine. Of these in their order. The urine was examined in *three* cases of gouty patients.

In the first class of cases, thirty-two examinations of urine were made in seven different individuals suffering from acute gout, in whom the general health had been pretty good during the intervals of the paroxysms. In these cases of acute gout, Dr. Garrod shows that the daily excretion of uric acid is by no means necessarily increased —nay, often notably diminished, the average of the analyses in all his cases being 3·62 grains in twenty-four hours.* Carrying his inquiries a stage further, he examined the blood as well as the urine in these cases, and in each of them he found that a considerable amount of uric acid existed in the serum of that fluid. He thus inferred that the renal organs were unable to excrete the whole of the uric acid formed in the system, and the circulating fluid in gout is thus always contaminated by the presence of a large quantity of uric acid, whatever may be the amount thrown out by the kidneys. In reviewing the results of each case, it is also seen that in acute gout the quantity of uric acid eliminated in different days is liable to much variation. In the only case examined, the elimination of urea was found not to be affected in the same degree as that of uric acid, although the blood usually contains a slight abnormal amount of this latter principle in the acute form of the disease.

In the second class of cases, those of chronic gout, "the majority of the patients were not suffering from any very urgent symptoms at the time the analyses of their urine were made; but many were afflicted with some of the sequelæ of the affection, as shown by the concretions of urate of soda on different parts of the body, and the stiffened condition of the joints." On such patients, fourteen (seventeen?) in number, Dr. Garrod gives the results of sixty analyses, which show a remarkable *diminution* of uric acid in the urine. In no instance did it amount to more than 5·78 grains; and the total average of all the analyses was far under a single grain per day. Several *urea* determinations being also made, the general result became evident, that the function of the kidney for excreting *urea* from the blood often remains intact in cases of chronic gout, where the power of the same organ for eliminating *uric acid* has become most seriously impaired.

In the third class of cases, the urine of those who had suffered more or less frequently from attacks of gout, of varying degrees of intensity, was examined at the time of complete freedom from any symptoms of the disease. Six cases were examined, and in none of them did the amount of uric acid excreted in the twenty-four hours reach the quantity usually considered to be the average in health.

Regarding the action of colchicum upon the urine, Dr. Garrod believes that "there exist in the medical profession at the present day, opinions which are extremely erroneous and without the slightest foundation." He admits the fact of colchicum having a powerful influence upon the progress of gouty inflammation, and to ascertain in what manner the effects are produced becomes an interesting and important question. The result of Dr. Garrod's observations leads him to draw the following conclusions:—

> "First. There is no evidence that colchicum produces any of its effects upon the system by causing the kidneys to eliminate more uric acid; in fact, when continued any length of time the contrary generally holds good.
>
> "Secondly. We cannot assert that colchicum has any influence upon the excretion of the urea or other solids of the urine.
>
> "Thirdly. Colchicum is by no means a diuretic in all cases. On the contrary, it often diminishes the quantity of urine, especially when it produces a marked effect upon the secretions from the alimentary canal.
>
> "No experiments are yet made sufficiently decisive as to whether or not colchicum produces a diminished formation of uric acid in the system."

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* The normal amount being estimated at *eight* grains.

XVI. *Contributions to the Pathology of the Glandular Structures of the Stomach.* By Wilson Fox, M.D. Following up the investigations of Dr. Handfield Jones, originally published in the Transactions of this Society, we have here an account of morbid and pseudo-morbid appearances observed in the glandular structure of the stomach, based on the notes of observations made upon one hundred stomachs taken indiscriminately from the bodies of patients brought for post-mortem examination to the Pathological Institute attached to the Charité Hospital at Berlin. The principal morbid conditions illustrated are those supposed to be associated with the affection known as recent or acute catarrh of the stomach. It is greatly to be regretted, however, that no accurate comparison had been made between the post-mortem appearances recorded and the symptoms of the patients during life. The facts, therefore, can only be regarded as records of morbid anatomy, the value of which must be determined by future research and comparison.

We want, also, a more clear account of the exact anatomical structure of the stomach and intestinal mucous membrane in health, with microscopic drawings, and the nature of the histolytic changes these parts are liable to undergo. As yet, the best and most practical monograph on the subject is that by Dr. Allen Thomson, Professor of Anatomy in the University of Glasgow, published by him in Goodsir's 'Annals of Anatomy.'

XVII. *On the Influence of the Cervical Portions of the Sympathetic Nerve and Spinal Cord upon the Eye and its Appendages, illustrated by Clinical Cases, with Observations.* By John W. Ogle, M.D.—The main points in this valuable paper may be summed up as follows:

1st. A short historical review of the most important results attained as regards the eye and its appendages, by experiments which have from time to time been performed upon the cervical parts of the sympathetic, and the spinal cord with which it is connected. The histories of clinical cases, old and new, are then detailed, in which a contracted state of the pupil had been observed, apparently induced by the presence of thoracic aneurisms, equivalent in degree to that which results from injury to the sympathetic in animals experimented on for that purpose.

2nd. Clinical cases are related in which a contracted state of the pupil had been observed in connexion with aneurism of other arterial branches than those within the thorax.

3rd. Instances are quoted in which pressure upon the sympathetic, from other causes than aneurism, had produced a like effect.

4th. Certain cases are noticed in which a contracted pupil had been observed in injuries of the spinal cord itself.

5th. Clinical cases are brought forward in which a *dilated* state of the pupil was apparently produced by pressure from various causes upon the sympathetic, the pressure being so slight and transient as merely to act as a stimulus to the dilated fibres of the pupil, enabling them to overbalance the existing contractors.

6th. Cases are related in which symptoms arose from pressure upon the sympathetic, or certain nervous structures with which it is connected—such as might, in addition to those effects upon the iris already alluded to, have been anticipated from anatomical considerations. These symptoms are strictly analogous to the various phenomena produced in the lower animals by section of, or extreme pressure upon, the sympathetic.

XVIII. *An Account of a Case of Calculus in the Bladder, removed by Lithotrity, in which a Communication existed between the Bladder and Intestine.* By Charles Hawkins, Esq.—The peculiarity of the case and the successful result of the operation performed for its relief are the features of interest in this paper. The communication must be read in detail.

XIX. *A Case of Dislocation of the Humerus upwards and forwards, with Fracture of the Coracoid Process of the Scapula; accompanied by a Dissection of the Parts* *involved in the Injury.* By T. Holmes, Esq.—This case is related because it is believed that no account has ever been given of a dissection of this form of injury, and because the numerous preparations in our London museums do not show anything like it.

XX. *On a Case of Premature Puberty.* By Robert Bath Smart, Esq.—This case is here put on record together with a brief reference to the examples of this condition already published in this country. The catemenia made their first appearance when the child was three years and six months of age; and a few weeks previous to the first menstrual flow a slight down on the pubes was the earliest phenomenon observed of this premature sexual development. The first menstrual discharge continued for a week, and it has continued to return regularly every month, the child being now (21st June, 1858) five years and four months old. The catemenia are natural in quality, but not much in amount, and generally continue to flow during four days.

PART SECOND.

[Bibliographical Record.]

Art. I.—*The Principles and Practice of Operative Surgery.* By Frederick C. Skey, F.R.S., Surgeon to St. Bartholomew's Hospital, &c. &c. Second Edition.—*London*, 1858. pp. 732.

Few English surgeons have ventured to write a systematic work on surgery. However large the experience of any individual may be, it cannot be sufficient to enable him to write originally upon all the subjects which are usually included in a complete treatise on any branch of the healing art. The nearest approaches to good systems of surgery are those works in which the labours of many authors are compiled, but even here, from the limited space allowed to each subject, the meaning of the original author is often obscured, and much that will not bear a critical examination becomes mixed up with that which is true. Mr. Skey's work is evidently the result of his own experience, and as such we must not be disappointed if we find in it the views of other surgeons imperfectly represented. Mr. Skey's work is, in fact, a series of practical dissertations upon those points which in the practice of a large hospital, have fallen under his own notice. From the merit of these essays we would in no way detract, if we venture to point out instances in which Mr. Skey's experience is at variance with that of other surgeons, or where subjects deemed generally of importance are inadequately described.

After some introductory observations of an ethical character, the author proceeds to discuss the various circumstances and phenomena attending the performance of an operation. He observes that—

> "At a period when the functions of the animal economy were less understood, the value of blood was less highly estimated than at present. It has now become a recognised rule in practice, to exercise the utmost caution in preventing unnecessary loss. . . . . Just in proportion to the quantity of blood which escapes from the circulation during an operation on a healthy person, always supposing the quantity to be considerable, may we anticipate difficulties and obstructions in our after treatment of the case. . . . . In children, no precaution tending to the prevention of loss of blood should be neglected. (p. 24.)"

The importance of this subject is becoming more and more recognised by English surgeons, and we believe that Mr. Skey has done good service by directing the attention of the profession particularly to it.

In the treatment after operations, Mr. Skey dwells upon the propriety of administering nutritious food, and, where the appetite allows of it, he prefers solid food to beef-tea and soups. When thirst is present, slightly nutritious drinks should be given, such as whey, or milk and water. Constipation Mr. Skey regards as a general consequence of loss of blood, and therefore no demand should be made upon the constitution by purgatives, until the deficiency is in some measure supplied.

The subject of dislocations is treated at length, and with much clearness. Mr. Skey here gives some valuable suggestions as the results of his own extended experience—

> "In dislocations of the femur on the *dorsum ilii*, we do not attempt to draw the bone in a direct line with the acetabulum, but we carry it below round its back and elevated margin, and no sooner does it reach the lower part of the rim, which is much less prominent than the upper and back part, than the muscles immediately restore it to its socket. The same rule holds in dislocation of the ulna and radius backward, at the elbow-joint. I believe the exact line of extension to be much more readily determined, and in truth, a less important subject of consideration, than it is generally deemed. I believe that, if we bring the bone sufficiently downwards, and place it in the *neighbourhood* of the articulation, the muscles alone will replace it without difficulty." (p. 81.)

With regard to dislocation of the head of the humerus, Mr. Skey observes—

> "With the heel in the axilla, we make a simple extension of the arm from the wrist; we leave the scapula unfettered to assume its own direction; and by the extending power slightly, as it is applied by one person only, the *scapula is made to revolve on its centre*, through the leverage exercised on the acromion by the deltoid. . . . . I have seen the protracted efforts of pulleys, drawing outward, fail again and again with the utmost effort, when the head of the bone has been restored to the glenoid cavity by the single agency of one person subsequently placing his heel in the axilla. There is no reason why, in very muscular subjects or in old dislocations, the same principle may not be applied conjointly with the use of pulleys. For the purpose of retaining this admirable, because most efficient principle, I employ a well padded iron knob which may represent the heel, from which there extend laterally two strong branches of the same metal, each ending in a bulb or ring of about four inches in length, the office of which is designed to keep the margins of the axilla as free from pressure as possible. . . . . It is of the utmost importance, if we wish for success, that we have the margins of the axilla untrammelled, in order that the bone (the scapula) be allowed to descend unchecked, at least by our agency." (pp. 106, 107.)

Mr. Skey throughout this chapter dwells upon the importance of the principle that the general extension of the muscles in the axis of the affected bone ought to be adopted, for the purpose of bringing a bone downwards, within the range of their general, and not of their individual action, and that we gain nothing by the resort to any direct line of traction, by which we give relief to one or more muscles at the expense of others.

Chapter VI. of Mr. Skey's work treats at length on the subject of fractures, and in the course of his observations many excellent remarks occur. Here again, however, the author confines himself to his own experience, and he has failed to avail himself of the improvements which have been going on elsewhere. In the section on fractures of the leg, for instance, Mr. Skey gives a woodcut of a swing cradle, which no doubt is a great improvement upon some cradles formerly used. It admits of the leg moving from side to side without any disturbance of the fracture, but it is essentially defective in not allowing the corresponding motions as the patient gets higher or lower in the bed. Mr. Salter's swing, now in general use, contrasts in this respect most favourably with that which is represented in Mr. Skey's work.

In the chapter on Wounded Arteries, Mr. Skey gives a very clear account of the two great theories by which surgeons have usually accounted for the obliteration of an artery, by ligature—

> "The principle laid down by Dr. Jones, and deduced from his interesting experiments, is that the union of the opposite sides of an artery is effected by the lymph thrown out from the divided coats, both within the tube of the artery and between the coats themselves, and that this lymph becomes accessory to the consolidation and to the final obliteration of the vessel." (p. 205.)

Scarpa, on the other hand, asserts that apposition alone will ensure perfect adhesion of the opposite surfaces—

> "That the artery does not become obliterated by the organization of lymph, thrown out by the divided tissues along the circle of the ligature, and in its immediate neighbourhood, by a process of adhesive inflammation, uniting the opposing surfaces of the serous coat."

Mr. Skey does not give his own opinion upon the subject, nor does he intimate to which of the above theories he inclines; we believe neither of them to be strictly correct. It has of late years been demonstrated fully, that the lining membrane of an artery or of a vein will not, as long as it maintains its integrity, be made to secrete lymph by any process of mechanical irritation; but we all know how readily the fibrin of the blood is deposited upon any foreign substance, or upon any irregular or abraded surface within the vascular system. All analogy would thus lead us, even were direct experiment wanting, to say, that the first bond of union in a ligatured artery is the fibrin deposited from its contained blood, and not the lymph effused from its inflamed coats. With regard to the second supposition, that of the adhesion of the opposed surfaces of the serous coats, it might be sufficient to remark that in no part of the body is the simple apposition of two serous surfaces ever followed by union without some uniting medium; but if direct experiment were wanting we have it supplied in the length of time that both arteries and veins will remain with their cavities closed by external pressure without there being the least indication of the adhesion of their opposed surfaces.

The principle of the obliteration of an artery by the fibrin deposited from its contained blood, is allowed by everybody, including Mr. Skey, in other cases, as for instance in aneurism. Why not, then, allow the principle of union by fibrin, as well where the internal coat of an artery has been divided by ligature, as where it has been destroyed by disease?

> "The process of union of the opposite sides of an artery and its consequent obliteration is, however, not to be determined by the period of the separation of the ligature, which may be protracted by other causes. . . . . In his experiments upon this subject, Mr. Travers has proved the removal of a ligature, within even a few hours of the operation for applying it, may be effected with safety, and the entire obliteration of the vessel secured. But as a general rule, it is better, because safer, to leave the ligature to come away by itself." (p. 287.)

With this opinion we cordially agree. One remarkable case has been published in which, after a temporary ligature applied in the way above mentioned, gangrene of the foot speedily followed its removal. This curious phenomenon can probably only be accounted for on the supposition that the period during which the artery was compressed was not sufficient to allow the coagula fully to form, and that when the current was re-established, the semi-coagulated blood was carried into the distal arteries and stagnated in their capillaries. It is not unimportant also to notice in connexion with this subject, that in several cases where compression for the cure of aneurism has been used, and subsequently relaxed, mortification, commencing in the distal extremity of the limb, has followed. These have been, however, altogether exceptional cases, and in no way militate against the value of compression as an ordinary mode of treating aneurism.

For the purpose of obliterating varicose veins, Mr. Skey still adheres to the old plan of destroying a portion of the skin over the vein with caustic. It is painful and tedious, and is now very generally supplanted by the simpler plan of the prolonged compression of the vein, or the still quicker plan of subcutaneous section.

Chapter XII. treats of amputations. Three forms of amputation are here described—the circular, the flap, and that which is commenced by an incision through the integuments, only forming a double flap of skin. Mr. Teale's operation is not included. Most of the observations which we should have to make on this head have already been anticipated in a review of Mr. Teale's work in a former number. We are still inclined to regard Mr. Teale's mode of amputating as superior to any of those described by Mr. Skey, but experience has proved that even this is not free (as we were at first led to suppose) from the common consequences of amputation, namely, phlebitis, secondary deposits, erysipelas, &c. While alluding to this subject, we may remark how very scanty and imperfect the descriptions of these diseases are in Mr. Skey's work. In fact, Mr. Skey professes not to have seen a case of pyæmia in his wards for the last three years. This assertion contrasts strangely with the experience of other surgeons. In a little work recently published by Mr. A. G. Sansom,[*] tables of mortality are given derived from various sources, showing the percentage of those who die after different operations. In one of these tables, No. VI., no less than one-fourth of the cases of primary amputation of the leg are reported to have died from pyæmia alone. We are, then, at a loss to account for the fact of Mr. Skey's not having witnessed this disease for so long a period.

We are unable to follow Mr. Skey through the other chapters of his work, every part of which contains original observations and interesting remarks. Although the work does not, as a whole, represent English surgery of the present day, we are of opinion that the independence of thought and research which characterize it, give it a peculiar merit, and justify our recommending it to every earnest student of his profession.

ART. II.—*Selections from favourite Prescriptions of Living American Practitioners.* By HORACE GREEN, M.D., LL.D., President of the Faculty, and Emeritus Professor of the Theory and Practice of Medicine in the New York Medical College, &c.—*New York*, 1858. pp. 206.

OUR readers are familiar with the name of Dr. Horace Green as the great promoter of the local treatment of morbid conditions of the fauces and air-passages. In the above work he comes before us in a new capacity, and offers to present us with a picture of American practice, so far as it is capable of being illustrated by prescriptions, and the remarks that they call for. The contents of the work scarcely correspond to the title; the latter leads the reader to expect that the bulk of the prescriptions at least will be by other American practitioners than the author, whereas they form but a small portion of the volume, Dr. Green's own formulæ being those that are put prominently forward; while in the majority of cases the prescriptions of others are quoted as being "highly recommended by an intelligent and experienced practitioner"—as being employed by a "physician of great experience"—by an "eminent physician of New Orleans"—by a "physician of this State of large experience," and the like impersonal individualities which excite no interest and leave no impression. Of the first hundred formulæ, sixty-five appear to have emanated from the pen of Dr. Green, while to eight only of the remaining thirty-five the authors' names are appended. The prescriptions are classified according to their assumed therapeutic effect, and are distributed in fourteen chapters, as narcotics, tonics, excitants, cathartics, emetics and expectorants, astringents, diaphoretics, diuretics, antispasmodics, anthelmintics, emmenagogues, gargles and lotions, antacids and antilithics, and derivatives. The majority of the prescriptions are closely analogous to what we habitually see in this country; more so than what those might have anticipated who are acquainted with the universal Formulary by Doctors Griffith and Thomas.[†] There are, however, two drugs which the author specially recommends, and regarding which we would therefore say a word or two, as they are not in common use in this country. One is the root of the *Podophyllum peltatum*, or May apple, which is regarded as a vegetable mercurial in its operation; it is prescribed in the form of an extract, or the active principle is isolated as *podophyllin.*[‡] The former is administered in grain doses; of the latter half a grain is given at a time. We subjoin two of the formulæ:

℞ Extracti podyphylli, ʒj.
  Extr. aloes hepat., ʒiij.
M. fiant pilulæ lx.

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[*] The Mortality after Operations of Amputation of the Extremities. By A. E. Sansom. Prize Essay. London, 1859.

[†] See British and Foreign Medico-Chirurgical Review, Oct. 1855.

[‡] The Record of Pharmacy and Therapeutics, edited by Dr. Medlock, informs us that Podophyllin may be obtained at the General Apothecaries' Company, in Berners-street.

℞ Podophyllin, ʒj.
Sacchari albi, ʒxix. M.

The dose of the latter is from five to ten grains, and "may be given in all cases where mercurials are indicated."

The other remedy alluded to is the *Sanguinaria canadensis*, which Dr. Green strongly recommends as an efficient excitant and alterative. It is ordinarily administered in doses of ten to twenty drops of a tincture ("four ounces of the root to two pints of diluted alcohol"), and is employed in catarrhal and febrile affections; an "old and experienced physician" strongly recommends the following formula in the treatment of habitual constipation:

℞ Sanguinariæ pulveris,
Rhei pulveris, ana ʒj.
Saponis, ℈ij.
Misce, et cum aquæ fiat massa in pilulas xxxij. dividenda, quarum capiat una mane ac nocte. (sic.)

The *Cimicifuga racemosa* is spoken of very favourably as a remedy for chorea, and is given in the form of decoction or tincture, *e.g*:

℞ Cimicifugæ racemosæ, ℥j.
Aquæ ferventis, Oj.
M. Ut fiat decoction. cujus sumant coch. mag. ij. ter in die. (sic.)

We would willingly believe that the errors in the directions of the two last prescriptions that we have quoted are mere misprints, did not similar mistakes occur frequently throughout the work, and did they not present a contrast with the remaining letterpress, which is well got up.

We have little doubt that the book is one of those which will soon come to a second edition; we trust that the hints we have ventured to give the author will not be thrown away, and especially that the contents of the work will then more closely agree with the promise held out on the title-page, and that the responsibility which attaches to a prescription will be thrown upon the physician who has communicated it to Dr. Green, a proceeding which we are sure will be more satisfactory to those who buy the book, as well as to the "Living American Practitioners."

Art. III.—*Johannes Müller: Eine Gedächtniss Rede gehalten bei der Todtenfeier am 24 Juli*, 1858, *in der Aula der Universität zu Berlin.* Von Rudolf Virchow.—*Berlin*, 1858. pp. 48.

Johann Müller: an Eloge pronounced in the Hall of the University of Berlin. By Professor Rudolf Virchow. Translated and edited by A. Mercer Adam, M.D.—*Edinburgh*, 1859. pp. 30.

Among the great men of the present century, Johannes Müller, the physiologist, deserves a prominent position. Possessed of the vigour and energy that prompted him to undertake important works, he was endowed with the brilliancy of conception and the power of persevering and laborious research, which lead to success. Probably no foreign physiologist has exercised so large an influence upon the growth and development of physiological studies in this country, directly by his intercourse with some of our own teachers, and indirectly by his well-known contributions to literature. His 'Hand-book of Physiology,' which is perhaps most generally known of his many works, shows best how entirely he mastered the whole range of natural science, and with what giant industry he brought together hitherto unknown facts and solved new problems. To employ Professor Virchow's words:

"He may be regarded as the representative of modern science, and in spite of the almost monastic seclusion of men of science, he has exercised an influence upon a wider sphere than upon physiology alone. He overcame the mystical and fantastic tendencies prevailing in the organic sciences; he opposed his clear judgment to all dangerous proceedings, whether sheltered under the mantle of metaphysics or of dogmatism, or whether resulting from mere caprice. He has not indeed invented, but he has established on a firm basis the exact, the true philosophical method. The school of natural science which he has called into existence recognises no uniformity of doctrine, but a uniformity of facts, and, still more, of method."

In the Speech delivered by Professor Virchow in the great Hall of the University of Berlin, we find a short summary of the various contributions to science which we owe to Müller. It is not to them individually that we would now direct attention—this would indeed be a work of supererogation. But it behoves us to echo the feelings of deep respect and admiration for the earnest man of science, to which Professor Virchow gives utterance. The seed that he has scattered has already brought fruit largely, so that he being dead yet speaketh. He speaks, too, by his example, and encourages all zealous searchers into the secret of Nature not to be dismayed by the difficulties which she opposes to them, but to preserve unflinchingly until she yields to their solicitations. Gifted with high talents, it was still by labour and endurance that Müller achieved his victories. Neither birth, nor fortune, nor position favoured his early strivings.

Art. IV.—*Medicines, their Uses and Mode of Administration, including a complete Conspectus of the British Pharmacopœias, an Account of New Remedies, and an Appendix of Formulæ.* By J. Moore Neligan, M.D., Honorary Doctor of Medicine, Trinity College, Dublin; Fellow of the King and Queen's College of Physicians in Ireland, &c. &c. Fifth edition.—*Dublin*, 1858.

It is devoutly to be hoped that when the sixth edition of Dr. Neligan's well-known and well-appreciated work is called for, one part of the title which is given above will have to be altered, and that the learned author will be permitted to allude to the British Pharmacopœia instead of the British Pharmacopœias. The revision of the work rendered necessary thereby will entail a greater amount of labour than otherwise would be requisite; but we are sure that Dr. Neligan will not grudge it.

In the present edition we find numerous additions, as the valerianate of ammonia under the head of antispasmodics, the anhydrous sulphate of zinc among caustics, carbonic acid and amylene in the chapter on sedatives, pepsine among the special stimulants, the arseniates of iron and of soda under the head of tonics.

It will thus be seen that Dr. Neligan has not been satisfied with a reprint of his previous edition, but that by incorporating most of the remedies recently introduced into practice, he has further enhanced the value of a work which the profession have unanimously placed in the first rank of medical literature.

Art. V.—*The History of Prostitution; its Extent, Causes, and Effects throughout the World.* (Being an Official Report to the Board of Almshouse Governors of the City of New York.) By William W. Sanger, M.D., Resident Physician, Blackwell's Island, New York City; Member of the American Association for the Advancement of Science; late one of the Physicians to the Marine Hospital, Quarantine, New York, &c. &c. *London* and *New York*, 1858. pp. 685.

If all Official Reports in the United States handle the subjects to which they refer in the manner in which, to judge by the sample before us, reports to the Board of Almshouse Governors of New York deal with any given question, Brother Jonathan must have abandoned his go-ahead system altogether; or, among other accomplishments, have acquired the secret of making time even more elastic than it has become by the aid of steam and electricity. We are accustomed to the length of the President's Message, as compared with the scanty speeches delivered by European monarchs to their assembled Houses, Chambers, or Landtags; but the variety of topics, national and international, which the President is called upon to consider, and to present to his countrymen, is an ample justification for a deviation from the routine of ancient governments. The author of the Report before us may quote Parliamentary Bluebooks as a precedent for the ample manner in which he discourses upon the subject he has chosen; but though we have often in those ponderous tomes found copious food for consideration and digestion, we have never met with that succulence and artistic display which meets us here.

It is a new feature, too, to see works like the one before us published in another country than that for which it is specially intended; until at least the public press and public opinion shall have put a stamp upon the work which removes it from the category of mere reports, and shows that the arguments and facts that it contains have a wider bearing than the title alone might imply. In the volume before us, London is put very prominently and in large capitals upon the title-page; while New York, the real place of publication, and the place which is supposed to be specially concerned in the Report, vanishes into an insignificance of type which may be complimentary to Englishmen, but is not unlikely to rouse the bile of our irritable cousins. We are great advocates for the freest interchange of opinions, and wish to see a community of thought and feeling fostered and perpetuated on both sides of the Atlantic, and shall therefore be glad to find a more real reciprocity established between the literary men and publishers of the United States and Great Britain than we have hitherto enjoyed; but we protest at the same time against having American or any other works foisted upon us as literary and scientific productions, which, under the guise of an official report or of a philanthropic scheme, pander to prurient and morbid imaginations. We know full well that it is necessary at times to purify the moral atmosphere by painful and disgusting proceedings; we have not shrunk from handling subjects in these pages that we would willingly have refrained from, but for a feeling of duty. Moral disease may no more be ignored in a Review like this than physical disease, even when they are not as intimately associated as they are in the subject under consideration; but as the sons of Noah walked backwards as they covered the nakedness of their father, so, too, when we have recognised a foul spot in our country, though seeking to prevent its contagion either to ourselves or to our contemporaries, by neutralizing its effluvia, or by burning out the unhealthy tissues, we would not unnecessarily and profanely expose it to public gaze. Much less is it requisite to bring forward prominently the evidence that such plague-spots have existed in all nations, and to show how mankind have fostered and perpetuated them at all times, unless by so doing some moral purpose is gained. In the work before us there is not a tittle of evidence to prove that the author has pursued such a purpose; and, whatever his intentions may have been, we maintain that his details of the forms and varieties in which prostitution has existed at all times and in all nations, can serve no other end than that of stimulating a depraved appetite.

We all know, even the Governors of the Almshouse of the city and county of New York must know, that there is, and ever has been, no more prevalent vice than prostitution; what the history of prostitution, the details of the manner in which it was or is carried on in Palestine, in Egypt, Greece, Rome, France, Italy, Spain, Portugal, Algeria, England, and other countries, have to do with the measures to be employed for its prevention, or the cure of its results, in New York, Dr. Sanger does not inform us. But we do see that it is the intention of the parties connected with the work that this "History of Prostitution" should be sold in England, if possible; and it is because we desire, as far as in us lies, to prevent the circulation of such a work, that we devote as much attention and as much space to it as we do.

The chapters that are specially devoted to the consideration of the subject as it affects New York can have no peculiar interest for our readers, and the suggestions offered by the author may be summed up in this, that they are a recommendation to adopt, as far as possible, a system of control similar to that existing in Paris. The moral aspects of the question, the real causes of the extent and spread of the vice, the influence of education in promoting or checking it, are not even glanced at. Our own views have too recently been put before our readers to render it necessary to go fully into the subject again; but we cannot refrain from placing once more on record the concluding remarks which we then made, and which we recommend to the Governors of the Almshouse of the city and county of New York, no less than to all governments who have the real welfare of their subjects at heart.

> "The grand battle, however, with prostitution must, we are convinced, be fought in the heart of man himself; there alone can the labour of regeneration be worked out. Instruct the young better to regulate their passions. Bring men to a true knowledge of what the thing called prostitution really is. Teach them to judge it as a crime, and to shun it as a dishonour. Strive to assuage all those many miseries of society which drive the wretched into crime. Struggle against the unrighteous fallacies forced by fashion upon the world around us. Thrust the seducer from decent life, as you would expel the slave-dealer from your home. Set the brand of society's scorn upon him. Here is the field of labour, and herein every one of us may work successfully; and these and such like are the lessons, in the teaching of which our profession can act well the instructor's part; and so lead society to struggle successfully against this pernicious enemy of the souls and bodies of mankind."*

Art. VI.—*Brief Exposition of Rational Medicine; to which is prefixed, the Paradise of Doctors: a Fable.* By Jacob Bigelow, M.D., late President of the Massachusetts Medical Society, Physician of the Massachusetts General Hospital, &c.—*Boston*, 1858. pp. 69.

The title of this little book, and its dedication to Sir John Forbes, as the author of 'Nature and Art in the Cure of Disease,' sufficiently indicate its scope. It is a succinct and modest statement of the medical creed which directs the scientific and practical views entertained by Dr. Bigelow, and though not likely to be extensively read in this country, we may express our satisfaction at seeing such excellent doctrine so well and clearly expounded in a country where the extreme of medical interference is more likely to be favoured than the extreme of *laissez aller*.

The following extract contains the quintessence, the *crême de la crême* of our author's views:—

> "Anatomy, physiology, and to a certain extent pathology, may be considered, so far as our discoveries have advanced, to be entitled to rank with the exact sciences. But therapeutics, or the art of treating diseases, like ethics and political economy, is still a conjectural study, incapable of demonstration in many of its great processes, and subject to various and even opposite opinions in regard to the laws and means which govern its results.
>
> "The methods which at the present day are most prevalent in civilized countries in the treatment of disease, may be denominated the following:
>
> "1. The *Artificial* method, which, when carried to excess, is commonly termed heroic, and which consists in reliance on artificial remedies, usually of an active character, in the expectation that they will of themselves remove diseases.
>
> "2. The *Expectant* method. This consists simply in non-interference, leaving the chance of recovery to the powers of nature, uninfluenced by interpositions of art.
>
> "3. The *Homœopathic* method. This is a counterfeit of the last, and consists in leaving the case to nature, while the patient is amused with nominal and nugatory remedies.
>
> "4. The *Exclusive* method, which applies one remedy to all disease, or to a majority of diseases. This head includes hydropathy, also the use of various mineral waters, electrical establishments, &c. Drugs newly introduced, and especially secret medicines, frequently boast this universality of application.
>
> "5. The *Rational* method. This recognises nature as the great agent in the cure of diseases, and employs art as an auxiliary, to be resorted to when useful or necessary, and avoided when prejudicial."

While we claim to be rational physicians, and, as far as we gather from Dr. Bigelow's volume, entirely agree with his estimate of medicines, of regimen, of diet, and of the

* British and Foreign Medico-Chirurgical Review, April, 1858.

powers of nature, we could wish some more definite term than the latter selected to convey the author's meaning. Rousseau and his followers put as forced an interpretation upon nature as a homœopathist or a mesmerist of recent days does; and we fear that it would be difficult to define nature in a medical sense more satisfactorily than the French philosopher. We attach a more definite meaning to the word "physiology," and its derivative, "physiological," and as technical language is unavoidable, it is well to use such terms only as shall leave room for as little ambiguity as possible. Physiological pathology, physiological medicine, the physiological laws which govern the body, are terms derived from what Dr. Bigelow himself admits to be one of the exact sciences. If Dr. Bigelow will kindly accept our suggestion, and modify his definition of the rational method accordingly, he will remove the only objection which we have to raise to his little volume, which we sincerely trust will be extensively circulated in the United States.

ART. VII.—*Veterinary Medicines, their Actions and Uses; with a copious Appendix on the Diseases of the Domesticated Animals.* By FINLAY DUN, V. S., formerly Lecturer on Materia Medica and Dietetics at the Edinburgh Veterinary College. Second Edition.—*Edinburgh*, 1859. pp. 520.

TRULY Edinburgh bids fair to outstrip all competitors in the race of veterinary science, as it has long rivalled the most important schools of the world in human medicine. Only in our last number we introduced to our readers Mr. Gamgee's 'Veterinarian's Vade-Mecum,' and in the previous number we had occasion to notice the 'Quarterly Veterinary Review,' both works of high merit, and issuing from the northern metropolis. We now bring before them the second edition of a valuable work on veterinary materia medica, which not six years ago was first presented to the public. By far the greater part of the volume (408 pages) is devoted to the consideration of medicines and their properties, the remainder of the work is an appendix, in which the author gives a brief account of the diseases to which domestic animals are subject, and their appropriate treatment.

The medicinal substances employed in veterinary surgery are arranged alphabetically, according to their English names; under each head, after giving the history and mode of preparation of the drug, with such chemical information as may be necessary to the veterinarian, the author analyses the physiological and pathological effects; the mode of exhibition and the dose constitute the concluding section of each chapter.

In the introductory part, the theory of the action of medicines is discussed, and here, as throughout the work, Mr. Finlay Dun proves himself to be well acquainted with the researches of modern science, while his own practical experience, of which evidence is frequently afforded, raises the work above the rank of a mere compilation. In the field of therapeutics, where so much is yet uncertain, the veterinarian may often be able to enlighten the physician, and we doubt not that many of our readers would gather useful suggestions as to the theory and practice of the healing art in the diseases of man, from Mr. Finlay Dun's well-digested and well-written work.

ART. VIII.—1. *The Pathology and Treatment of Stricture of the Urethra, and Urinary Fistula.* By HENRY THOMPSON, F.R.C.S., M.B., London, Assistant-Surgeon to University College Hospital, &c. Second Edition.—*London*, 1858. pp. 417.

2. *The Pathology and Treatment of Stricture of the Urethra.* By JOHN HARRISON, F.R.C.S. Second Edition.—*London*, 1858. pp. 108.

3. *On Stricture of the Urethra, including an Account of Perineal Abscess, Urinary Fistula, and Infiltration of Urine.* By SAMUEL G. WILMOT, M.D., Fellow and Member of Council of the Royal College of Surgeons, Ireland, &c. &c.—*Dublin*, 1858. pp. 219.

NOTWITHSTANDING the number of works that continue to appear on stricture, a practical work, embodying in a small compass all that is known on the subject, is still, perhaps, a desideratum for the student. This want Dr. Wilmot has endeavoured to supply. In the first chapter he treats of the division of stricture, and its pathology. The muscularity of the urethra is fully considered, and the part it plays in certain morbid phenomena. The situation of stricture, its causes and symptoms, form the subject of the second chapter, and here the different kinds of urinary fever are considered, together with the differential diagnosis of stricture. The third chapter is devoted to the results of stricture. The remaining chapters have to do with the treatment of different forms of stricture, of the impediments and complications met with in the treatment. We are very glad to see Dr. Wilmot's work in its present form, and cordially recommend it to the notice of students of medicine.

Mr. Harrison's work we have already had occasion to notice. It is clearly and distinctly written, is practical throughout, and well adapted for the use of the surgeon engaged in general practice who wishes to be informed in a few words what he is to do in any particular case.

Mr. Thompson's work, the first edition of which we have also formerly noticed at length, contains a much more circumstantial and detailed account of the diseases of the urethra than either of the other works which we have mentioned. It is illustrated by thirty-nine well-executed wood engravings, and contains a considerable amount of new and original matter. We would particularly notice in this respect a careful collection of cases in which Mr. Syme's operation by external division of the stricture has been performed by various surgeons. The mortality from this operation Mr. Thompson makes about six per cent.; but upon this subject it is important to notice, that in none of the works above mentioned, nor, in fact, in any of the surgical works at present published in London, is a clear distinction drawn between those cases in which Mr. Syme's operation is admissible and those in which it is not.

According to Mr. Syme's own description of his operation, it appears evident that he makes his incision into the urethra in front of the membranous portion, and as he then cuts forward, it is equally clear that he leaves both the membranous portion of the urethra and the deep perineal fasciæ uninjured. Now, this mode of operating is obviously adapted only for cases in which the stricture is in front of the membranous portion. To attempt to apply this operation to any other kind of stricture is obviously to confound together two essentially different operations, and we strongly suspect that the greater mortality that has occurred in the practice of some surgeons than in that of others has depended upon this want of discrimination.

No one is better aware than Mr. Thompson of the different positions in which stricture occurs, for, as he himself informs us (p. 82), he has himself submitted to close and careful inspection not less than three hundred preparations of stricture of the urethra. Mr. Thompson's first class includes strictures which occur in the canal an inch before and three-quarters of an inch behind the junction of the spongy and membranous portions (p. 83): a more practical division would be into strictures which involve the membranous portion of the urethra and those which do not. The latter kind are alone proper cases for Mr. Syme's operation, and if restricted to these we believe the mortality resulting from the operations which have been performed under this name would be much less than it has hitherto appeared to be.

In conclusion, we have only to express our conviction that the thanks of the profession are due to Mr. Thompson for the great care and assiduity he has bestowed in collecting evidence with regard to disputed points, and to state that the second edition of his work is well calculated to sustain the favourable impression produced by the first.

Art. IX.—*Observations and Notes on the Arteries of the Limbs.* By Thomas William Nunn, F.R.C.S., Demonstrator of Anatomy and Lecturer on Pathology at the Middlesex Hospital.—*London*, 1858. pp. 27.

Descriptive Anatomy is such weary work, both for teacher and pupil, as it is usually followed—the former grinding over, time after time, a tune which has no meaning or music in his ears, the latter floundering about in a chaos of hard words which he can hardly repeat, and tries in vain to remember, that we cannot refuse an expression of gratitude to any author who will strive to connect the description of the parts of the body at fresh points with the rest, to which they are subordinate; strive, in one word, to teach anatomy physiologically. It is the merit of Mr. Nunn's pamphlet, and a very great one, to endeavour after this end in a rather remote part of anatomy—that which treats of the course and distribution of the arteries of the limbs.

Mr. Nunn uses as the base of his theory a fact which is stated implicitly rather than expressly in most anatomical works—viz., that there is in most segments of each limb a main artery, which runs through that segment without giving off any important branches, and another which breaks up into numerous branches for the supply of the parts contained in that segment. Thus in the pelvis, the external iliac belongs to the first category, and the internal iliac to the second; in the thigh, the superficial femoral to the former, and the profunda femoris to the latter; in the leg, the posterior tibial runs through without giving off any important branches, while the femoral breaks up to supply the various parts, &c.

From this arrangement, which, however, is much less obvious in the upper extremity than the lower, Mr. Nunn derives a scheme which divides all the arteries of the limbs into four classes:—I. *Segmental,* those, namely, which supply the segment of the limb in which they arise. II. *Transegmental,* which run through the segment and carry blood to the parts below. III. *Anastomotic,* or communicating, which provide for the collateral circulation; and IV. *Composite,* which partake of the nature of the others, and which Mr. Nunn appears to regard as deviations from the archetypal structure, necessitated by the requirements of the individual organism.

The classes above indicated differ in their course as well as in their function; the segmental arteries branching frequently and at large angles; the transegmental pursuing a nearly straight course; the anastomosing having no special direction. The physiological reason or import of this fact Mr. Nunn thus explains:

> "The straight tubes transmitting the blood to distal parts, with the least loss of original velocity, thereby provide against the loss of heat by the blood, and consequently maintain the parts most distant from, at a temperature little below that of those nearest to, the heart. The segmental branching and diminishing tube, by delaying the blood, allows it to impart its heat to the tissues traversed; and besides, renders the supply to the capillary system more steady and uniform, and less liable to be interfered with by temporary disturbing causes." (p. 24.)

The glaring exception to his scheme of classification furnished by the axillary brachial trunk, Mr. Nunn endeavours to dispose of, by classing it as an anomaly; and by very ingeniously dwelling on the tendency that that trunk has, in so-called abnormal distributions, to recur to the type normal to the arteries of the lower limb, which he regards as the typical condition of the vascular supply of limbs generally. He also endeavours to show how the arteries of the forearm conform to his scheme; but we cannot say that this part of the paper is to our minds very satisfactory; nor can we see why Mr. Nunn should have limited himself to the arteries of the limbs (including those of the pelvis), and have declined the consideration of those of the neck, where the relation of the common carotid to the branches of the thyroid axis, and of the internal to the external carotid, appear at first sight so analogous to those on which he dwells.

The paper is illustrated with appropriate diagrams, and will be found interesting and suggestive.

ART. X.—*On the Vesico-Vaginal Fistula.* By I. BAKER BROWN, F.R.C.S., &c.—*London,* 1858. Pamphlet.

THE pamphlet of Mr. Baker Brown contributes eleven cases illustrating the value of Dr. Bozeman's operation for the cure of vesico-vaginal fistula. The peculiarities of this method have been on a former occasion described in this Journal; and an analysis of Dr. Bozeman's pamphlet on the same subject has also appeared. The interest of Mr.

Brown's cases, therefore, depends upon the amount of confirmatory evidence they bring to show the curability of this long intractable complaint.

Six of the reported cases were treated in St. Mary's Hospital. We select these for analysis. The first case presented an opening the size of an ordinary director close up to the os uteri. It was the result of an instrumental delivery after forty-eight hours' labour. Bozeman's operation was performed on the 15th of October, 1856, a silver button with three holes being used. The patient was reported perfectly cured on the 8th of November.

The next case was admitted in February, 1855. The patient had been delivered by instruments after forty-eight hours' labour in November, 1854. The urine escaped into the vagina twelve days afterwards. There was a large transverse opening across the centre of the bladder, admitting two fingers. Closure appearing impossible, the plan recommended by Jobert was adopted. The neck of the bladder was dissected from the pubis and descending rami, thus allowing the anterior half of the bladder to fall back backwards and relax the fistula. In April the edges were pared and brought together by Sims' method. This did not succeed. The patient left, had another child, and returned in December, 1856. Bozeman's operation was now performed; eight-tenths of the opening were closed. Several successive operations, nine in all, were performed, the last in March, 1858, before a complete closure was effected.

The third case was admitted six weeks after an instrumental delivery. The escape of urine was noticed a fortnight after labour. When admitted a large slough was removed from the vagina. The fistula was just anterior to the os uteri, transverse, half an inch long, having a white appearance, showing that a slough had yet to fall. A month later Bozeman's operation, using a leaden button, was performed. On the tenth day, when the shot and button were removed, the fistulous opening had entirely healed. To facilitate the operation in this case, an incision was made downwards and obliquely to the right side through the posterior wall of the vagina, to enlarge the vulval opening.

The fourth patient was delivered after three days' labour, without instruments. The urine dribbled away two days afterwards. There was a transverse opening about an inch long at the neck of the bladder. The operation was performed on the 19th of May, 1858, using three silver sutures and a leaden button. A second operation was necessary, after which she was discharged, cured, on the 7th of July.

The fifth patient had been delivered after a very protracted labour by craniotomy. The water dribbled away eight days afterwards. The actual cautery had been applied. An opening existed at the upper part of the vagina, close to the right side of the os uteri, admitting the tip of the little finger. The edges were tense and callous. The edges were pared in a longitudinal, instead of a transverse direction. Four silver sutures were passed, one going through the anterior lip of the os uteri. The sutures were removed on the eleventh day, when the opening was found quite closed.

The last case was admitted on the 14th of October, 1857. The patient had been delivered in March by instruments after a protracted labour. She had a fistulous opening into the bladder large enough to admit three fingers; the edges were puckered up, very tense, and the opening was drawn quite behind the arch of the pubis. The urethra and neck of the bladder were first detached from the rami of the pubis, in order to close the opening. On the 17th of December, Bozeman's operation was performed, but unsuccessfully. Re-admitted in January, 1858. The fistula was now an inch and a quarter long, extending in an oblique direction close to the os uteri, which was closed. She had not menstruated since the accident. One of the silver wires used in the former operation was lying bright and firm in the edges of the fistula, causing no inconvenience. The operation now repeated, using seven sutures, was this time successful.

It is clear that no one is likely to have much success in these operations who does not bring as much perseverance as skill to the work.

ART. XI.—*On Wounds and Injuries of the Eye.* By WILLIAM WHITE COOPER, Fellow of the Royal College of Surgeons of England, Ophthalmic Surgeon to St. Mary's Hospital, Senior Surgeon to the North London Eye Infirmary, Consulting Surgeon to the School for teaching the Blind, &c.—*London*, 1859. pp. 330.

WE have received this work at too late a period to allow of our doing more at present than directing the attention of the Profession to it. Without pledging our opinion as to details, we have no hesitation in stating it to be a book of a very practical character, and one that contains a large amount of information on a subject upon which hitherto we have possessed no English monograph. The volume is profusely and elegantly illustrated, and bids fair to become a favourite with the Profession.

ART. XII.—*Summary of New Publications.*

THE doctrines of the causation and prevention of disease continue to be subjected to scrutiny and illustration by various inquirers, who appear in force during the present quarter, both officially and as volunteers. Drs. Conway Evans and R. D. Thomson, respectively, are authors of sanitary reports; the former dealing with the Strand District of London, the latter with the parish of Marylebone. Among the volunteers we must put prominently Dr. Parkin, because he boldly attacks many of the prevailing theories regarding 'The Causation and Prevention of Disease,' in a work bearing that title. Mr. Smee, in a little book which contains the substance of an oration delivered before the Hunterian Society of London, and is entitled, 'General Debility and Defective Nutrition,' discusses the various influences of civilized life in promoting disease; with it we may mention an elaborate and interesting report of Dr. Whitehead's of the Clinical Hospital, Manchester, which we recommend especially, on account of the suggestive nature of its contents, to those dispensary physicians who wish to utilize their labours for science. Mental Disorders are investigated by Dr. George Robinson, especially with a view to tracing their connexion with the various influences that result from our present state of civilization. In an able review, entitled, 'On the Relation of Psychology and Physiology,' Dr. Harvey warns against the exaggerated importance which he thinks is being attributed to the reflex functions, and asserts the prerogatives of mind as lord paramount of the body, in opposition to those psychologists who would appear to reverse this relation.

General Physiology finds a new exponent in Dr. Dalton, jun., of Philadelphia; Dr. Schiff, whose labours we adverted to in our last number (p. 185), has issued the third number of his 'Lehrbuch der Physiologie;' in Anatomy, we have to mention a valuable work by Dr. Robert Lee, 'Engravings of the Ganglia and Nerves of the Uterus and Heart,' in which we recognise our old friends which illustrated Dr. Lee's contributions to the 'Philosophical Transactions.' We may here advert to an interesting pamphlet by Dr. Harvey, on the Inoculation of the Maternal Organism through the Fœtus in Utero. In the sciences ancillary to Medicine, we have to mention Mr. Conington's Handbook and Tables of Chemical Analysis.

In Medicine proper, we have received 'A Handbook of Hospital Practice from Dr. Lyons, which is to serve as an introduction to the 'Practical Study of Medicine at the Bedside.' A fourth edition of Dr. Horace Green's work, 'On Diseases of the Air Passages,' intended, as our readers probably all know by this time, mainly to advocate the local treatment of these affections; and a fifth edition of Dr. Condie's work, 'On Diseases of Children,' come to us from the United States, together with the eleventh volume of the 'Transactions of the American Medical Association.' We have before us a comprehensive and elaborate work by Dr. Brinton, 'On Diseases of the Stomach,' and a second edition of Dr. Taylor's well-known work 'On Poisons in relation to Medical Jurisprudence and to Medicine,' containing numerous alterations and additions. Dr. Hunt supplies a volume entitled 'A Manual of the Philosophy of Voice and Speech,' intended to establish the treatment of stammering on a scientific basis; Dr. Mayne progresses laboriously and satisfactorily with his 'Expository Lexicon of Terms in Medical and General Science,' which now attains its eighth number, concluding with *strobiliferus.* 'The Healing Art the Right Hand of the Church' by Therapeutes; 'Man and his Dwelling Place,' by an anonymous author; and 'The Soul and Future Life,' by Cromwell, may be mentioned in this place.

Midwifery supplies us with no novelty, unless we reckon as obstetric, Dr. Skinner's 'Chloroform as an Anæsthetic in Natural Labour Defended.'

Surgery is represented by an American edition, by Dr. Packard, of Malgaigne's 'Treatise on Fractures;' by a prize essay by Mr. Sansom, on 'The Mortality after Operations of Amputation,' to which we have already had occasion to advert in speaking of Mr. Skey's recent work; by the seventh fasciculus of Mr. Maclise's work on 'Dislocations;' by a second edition of Mr. Brodhurst's work, 'On the Treatment of Anchylosis, or the Restoration of Motion in Stiff Joints,' in which the author advocates forcible rupture of the uniting medium in partial anchylosis. We may here also advert to a translation, by Mr. Maunder, of Ricord's 'Lectures on Chancre,' to which, with many of the other works enumerated, we shall refer on a future occasion. We may not, however, close this enumeration without adding Mr. Wilde's 'Medico-legal Observations upon the case of Amos Greenwood, tried at the Liverpool Assizes, December, 1857, for the Murder of Mary Johnson;' the second edition of Mr. Beazley's 'Book of Prescriptions,' the continuation of Dr. Beale's 'Archives of Medicine,' of the 'Ophthalmic Hospital Reports,' of the 'Liverpool Medico-Chirurgical Journal,' and the 'Memorial de Sanidad del Ejercito y Armada,' which we have received from Madrid.

PART THIRD.

Original Communications.

Art. I.

Entoptics. By James Jago, A.B. Cantab., M.B. Oxon., Physician to the Royal Cornwall Infirmary.

§ *Entoptical Methods and Preliminary Principles.*

1. Our visual organs are not only capable, by an adjusting lenticular system, of painting, under varying conditions, images of luminous objects upon a membrane in peculiar relation with the brain, but are furnished with or involve many adjuvant structures. Thus it happens that they reveal to us a number of adventitious phenomena—spectres, as we may call them—whether caused by light at the parts that cover the cornea, or by any stimulus whatever affecting the special nervous tract. Besides the ordinary interest that we feel in tracing subjective illusions to their sources, the accurate elimination of these is a physiological necessity, if we would avoid the risk of ascribing effects begotten by subordinate to more integral portions of the apparatus, and thus of forming wrong conceptions of the laws which regulate the conduct of the latter. Finally, a diligent study of these accidental appearances may be made serviceable for the solution of certain important points of ocular structure of too delicate a nature for the microscope or other usual means of investigation; as also for determining important questions of function.

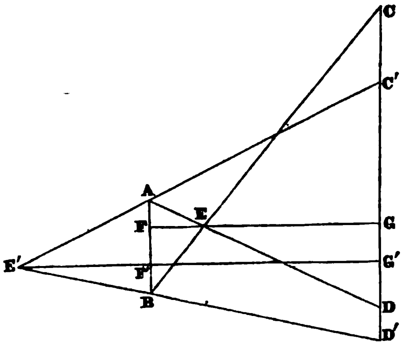
A methodical proceeding to show, as I have elsewhere expressed it, how "ocular spectres, structures, and functions are mutual exponents," I have here called Entoptics. Or rather, it would be better to regard so much of this essay as deals with phenomena independent of objective light as supplementary, that the word entoptics may accord, as it ordinarily does, with other words from the same root.*

When light is an agent in their production, such apparitions as we allude to arise generally from certain rays being absolutely or comparatively blocked from the course they would otherwise pursue, or else turned aside by refraction, reflection, or inflection at some object they encounter. But since the pupil is occupied in common by the pencils of rays from *all* visible points, whilst they again separate as they near the retina, it is only when the phenomena originate far back in the eye that they are discernible in the diffused light of day. Or to view them in other cases, we must regard a bright space of limited extent.

And to make *precise* observations we should resort to pencils of rays which do not return to foci upon the retina, so that effects once evolved may not afterwards blend with and obliterate one another; as by using rays diverging from some point external to the eye, and within its least focal distance, or rays converging to, and then diverging from some point within the globe.

When a portion of the rays of such a pencil find some body in their path, if a simple shadow is not the consequence, some appearance sufficiently indicative of the body's contour to warrant our currently mentioning it as an image of it, will be projected in the line of the shadow. So that, for the geometrical purpose of discovering the seat of the intervening body—a prominent problem in this essay—we may regard the image as a shadow; as also, with certain precautions, for ascertaining the size of the body. Whilst our ideas of the nature of the body must be derived from a more particular examination of the phenomena it yields under different physical conditions.

2. Draw A B and C D (Fig. 1), any two straight lines parallel to each other; join A D and B C, and through the point E, where the two straight lines thus made intersect each other, draw F E G perpendicular to A B or C D, meeting them in F and G respectively.



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* In 1841, whilst in statu pupillari, I located *muscæ volitantes* entoptically, and in and out of these realms freely got others to explore their eyes for me, ere, in 1845, more leisure led my thoughts into "Points in the Physiology and Disease of the Eye," as in London Medical Gazette, May 9th, subscribed March 12th; geometrically done, much as herein, as far as two divergent pencils, from radiants near the cornea or one in motion, go, measuring inwards from the cornea, as well as outwards from the retina. The only history of entoptics I know of is by H. Helmholtz (Allgemeine Encyklopädie der Physik, s. 163. Leipsig, March, 1856). From a summary of casual remarks of previous writers he proceeds: "A stricter (strengere) theory of the appearances, and the methods of judging of the place of the corpuscles in the eye, were first established much later by Listing and Brewster." (Beitrag zur physiologischen Optik. Göttingen, 1845, Trans. Roy. Soc., Edinburgh, vol. xv. 1845.") Both these methods use only divergent pencils. The latter does not embrace the thought of searching the eye from known anterior objects backwards. The former is virtually the same as mine, as I find from Helmholtz's sketch of it. This paper of Brewster's on *muscæ volitantes* I have only had access to in Phil. Mag., p. 1, 1848, which introduces it as "from Trans. . . . vol. xv.; read March 6th, 1843." I am not within reach of further information about the several dates mentioned. I believe the double method by convergent and divergent pencils to be entirely mine. It may be found in general language in "Ocular Spectres and Structures as Mutual Exponents," London, January 1, 1856, though it was not then reduced to a geometrical form.

Again, take some point, E', on the other side of A B, to that where E is situated, join E' A and E' B, and let the straight lines thus made be produced to meet C D, or C D produced, in C' and D' respectively. Lastly, draw E' F' G' at right angles to A B and C D meeting them respectively in F' and G'.

From similar triangles contained in the figure we have C D : A B :: E C : E B, or :: E G : E F; that is, since E G = F G − E F,

$$\mathrm{CD} = \mathrm{AB}\,\left(\frac{\mathrm{FG}}{\mathrm{EF}} - 1\right) \;.\;.\;.\;.\;.\; (a)$$

In like manner, C' D' : A B :: E' C' : E' A :: E' G' : E' F', and therefore,

$$\mathrm{C'D'} = \mathrm{AB}\,\left(\frac{\mathrm{F'G'}}{\mathrm{E'F'}} + 1\right) \;.\;.\;.\;.\;.\; (b)$$

3. But, firstly, if we view the figure as representing everything as falling in the plane of the paper, we may take A B to be an object lying in the course of a pencil of rays, which converge to the point E, and C D to be its shadow received, under the conditions implied above, after the rays have passed on divergently from E. In the same way C' D' will be a shadow of A B thrown by a pencil of rays diverging from E'.

And we perceive at a glance, that in the former case the shadow is an inverted image, whilst in the latter it is an erect one; that in either case the length of the shadow varies directly as that of the image. And for a given object the length of the shadow is to its own as their respective distances from the focal point. When the object moves to the focus (E F = 0, or, E' F' = 0), the shadow is infinite in length; and when to its shadow (F' G' = 0), both their lengths are equal. But if we assume the object and shadow to have fixed places, whilst the position of the focus alters (that is, F E or F' E' alone to vary), the equations (a) and (b) show how the approach of the focus to the objection their respective sides of it augments the shadow. That, for instance, (E F = E' F') at equal distances from it, the shadow in the divergent pencil is longer than the other by twice the length of the object. If the focus comes up to the screen on which the shadow is received (E F = F G), there will be no shadow.

Finally, if we know all the terms in either equation, except A B, the length of the object, this may be determined.

4. We may, secondly, regard the same figure as indicating yet other matters. If we imagine a pencil of rays proceeding from left to right to decussate, or pass through a focus at A, and another pencil to do the like at B, C D will represent the distance between the two shadows of any object, E, lying in the divergent portions of the pencils, and C' D' that between the two shadows of any object, E', lying in the convergent portions—that is, if received upon a screen, as implied in the diagram.

Wherefore, in the first case the objects are inverted in position with respect to the foci; in the other, erect. In either case the distance between the two shadows varies as that between the two foci; and that between the two former is to that between the two latter as their respective distances from the object. Hence, too, if the foci, preserving their distance A B, move, whilst the positions of the object and screen remain the same, the separation of the two shadows is greater when the foci approach the object; when A B arrives at the object, the two shadows are infinitely apart. Should the foci be brought up to the screen, the two shadows would be at their distance (A B) from each other. But if we assume the foci to be fixed as well as the screen, whilst the object changes its place (F E and F' E' alone to vary), the equations (a) and (b) show how the approach of the object to the foci, and their respective sides, augments the separation of the pair of the shadows; and that if E F = E' F', the distance between the pair in the convergent pencil is greater than that between the pair in the divergent by twice that between the foci. If the object touch the screen (E F = F G), the two shadows coalesce. But if E' F' = F' G', the two shadows are twice as widely sundered as the foci.

Finally, if we know all the terms in equations (a) and (b), except E F or E' F', the place of the object, this may be determined.

5. It being proposed to make the principles above explained the fundamental method

of exploring our own eyes, we must not forget that the screen, our own retina, is curved, nor that there may be in eccentrical parts of it a certain amount of parallax from the law of vision by normals to it, nor that the rays of light that reach it are bent by ordinary ocular refractions. Insomuch that, in obtaining absolute values from the foregoing proportions, we must not reckon beyond upon more or less approximate results. These circumstances, however, cannot be said to occasion embarrassment; because we shall find no great difficulty in evading or correcting such aberrations. And it will transpire, as we proceed, that there are no physiological problems of any prominence, open to this mode of treatment, in which comparative results alone are not efficacious.

Let us seek for pencils of rays of the two kinds, which may be conveniently thrown upon our retina.

The diffused light of day, from the sky, clouds, smooth sheet of water, white road, or wall of a house, admitted into the eye through a fine puncture in a card, gives a good divergent pencil; though the convergent one, in this instance lying in front of the puncture, is not available.

Nevertheless with light issuing from small luminous discs we can readily command all such pencils as we need. We may, by means of the head of a pin, or the surface of a convex lens, reflect into the eye, from the sun or a small candle-flame, very fine divergent pencils. Or use the image of the disc formed in the focus of a lens of short focal length for this purpose. If we look through a convex lens of an inch focal length towards a gas, or candle-flame, remotely (as at the length of a long room) situated, so that the image may be formed at or near the principal focus of the lens, that there may be yielded a rapidly convergent and therefore divergent pencil, the eye may receive, to more than half its depth, far enough for all practical purposes, a divergent or convergent pencil at pleasure. For nice observations we can reduce the flame to a small round disc, by fixing before it a card with a small circular aperture. By approaching the disc we can introduce the focus deeper into the eye. By placing the disc at our back at one end of the room, and viewing its image in a mirror at the other, we can double the distance of the radiant. In a word, we may consider ourselves equipped for the major part of our researches, if we are provided with an object-glass of a microscope of an inch focus. By fitting the tube in which the glass is fixed into another of card-paper, we can slide one within the other, so as to introduce the focus any *measured* distance into the eye.

In commencing, however, the actual application of the geometrical deductions of 3 and 4, we must take into account that all projections upon the retina will appear inverted; and that thus what has been described as being inverted will *seem* to be erect, and what as erect, inverted.

6. If a single pencil, then, diverge (3) from a point a little in advance of the eye, this point will be the apex, as it were, of a cone of light, the size and shape of whose retinal base will depend upon that of the pupil, of which indeed it will be an erect, and therefore apparently an inverted, luminous image; comprising apparently inverted images of all the objects that intrude upon the cone, in apparently inverted positions with respect to its own centre, and with movements seemingly diametrically opposite to those they really have, if any of them move independently of the eye itself.

If we use the lens, and carry the focal point towards, and then into the ocular media, it will arrive at a depth where the divergent pencil will no longer fill the pupil, and therefore no longer project a complete image of the margin of the iris. Still, so much of this margin, and of whatever else falls in the divergent pencil, will display the inverted effects just adverted to: while whatever has been embraced by the advancing cone of convergent rays will *appear* erect, in their *true* positions with respect to one another, and with the *actual* movements independent of the eye they may happen to have; presenting up to this point a complete inversion of the picture we had of the same things in the divergent pencil.

By referring to 3, and interpreting the word *infinite*, as applied to an image, as meaning filling the retinal field, or occupying the whole pencil of rays, we may see how the retinal shadows or images of interrupting bodies vary in size, as the focal point falls before, upon, or behind them. Thus, by this means alone, we may immediately observe the order in distance from the retina, iris, or tear, or any known ocular site, in which such bodies occur.

7. There is a point in the axis of the eye which may be called its optic or lenticular centre, as nearly that where the rays from external visible points that hold a *straight* course to their destination, all cross each other. Hence, if the focus of the lens we apply to the eye be made to coincide with this point, its rays will likewise proceed undiverted by ocular refractions.

In conceiving in the diagram the screen as a straight line, we virtually represent the retina by its tangent; and we may do this for *small* angles, because in them the lengths of the arc and tangent do not *appreciably* differ from each other. Wherefore, when we introduce as far as the optic centre, a pair of foci nigh enough together to cast the corresponding points of any pair of shadows we may wish to view near each other, we adopt the best expedients for ensuring accuracy in numerical results deduced from the foregoing equations.

In attempting from 3 and 4 calculations as to size and distance, in this manner, we use an instrument which prevents our seeing with the eye which we are exploring, objects of the external world, but we can note apparent sizes and distances of the images against any surface at an ascertained distance from the eye's optic centre, by aid of the other eye.

8. We may take the eye's optic radius (making the optic centre $\frac{7}{36}$ from eye's surface) at $\frac{2}{3}$, and its optic axis at $\frac{5}{6}$ of an inch. Whilst the depth of the vitreous humour is $\frac{2}{3}$, of the crystalline lens $\frac{1}{6}$, and of the aqueous humour and cornea together also $\frac{1}{6}$ of the length of the optic axis.* Then an actual retinal length is to the apparent as $\frac{2}{3}$ to the remoteness in inches of the surface we measure upon from the optic centre. We may mark experimentally in a given case the distance of the focal images of the two discs from the centre of the lens we use, and calculate the separation of these images, as in the instance of the eye; or employ the usual formula for such a purpose.

9. If we now have recourse to two crossing pencils (4 and 6), of the pairs of like images projected upon the retina, those images contributed by the right-hand source of divergent rays will be *seen* on the right of their fellows. But when we derive our divergent pencils from the images of a couple of bright discs (6) by means of a lens, these images have respectively changed sides in regard of their discs, so that the projections by the right-hand disc *appear* on the left of their fellows, whilst those rendered by the convergent pencil of the right-hand disc fall, in appearance also, on the right of their fellows.

As the principal transparent structures of the eye are an alternation of solid and fluid media, a very rough application of our rules would enable us to observe in which of these media any shadow-throwing body is situated, because those of one kind will be fixed in the eye, whilst the others will float in it; those occurring anteriorly to the conjunctiva, and the iris within the globe being moveable at our pleasure, without the globe itself being disturbed. Thus, an eyelash, tear, iris, an object in the crystalline, and one in the vitreous, with a couple of divergent pencils in front of the eye, would cast pairs of shadows, subtending angles less and less in the order in which their causes are here cited. But when the same bodies are immerged in a couple of convergent pencils, the pair of shadows of the eyelash subtend the least angle, and, in order, the others greater and greater angles. . This sort of reversal is a striking event, so that the mere passing of the imaginary line that joins the two foci from before backwards, tells us at once which of two ocular bodies is deeper.

10. For the clearer understanding of a modification of the mode of investigation by two pencils, of practical moment, we may remark, that could we look with the same eye, and at the same instant, towards a distant, small luminous disc, through two side by side convex lenses with parallel axes, the right hand lens would fling its image of the disc to the right of that of the other, and all the fellow shadows thrown in the two divergent pencils would be seen in just such mutual relation, as happens in the

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* These *mean* values are gathered from 'Physiologische Optic,' von H. Helmholtz, § 10, Encyk. d. Phys.

case of light from two punctures or two lucid points; wherefore the convergent pencils from the lenses would so project the objects lying in them that the images caused by the right-hand lens would appear on the left of their fellows.

Hence, if we gaze through a *single* puncture in a card at the apex of some terrestrial object visible against the sky, or a fixed spot of any luminous surface, or gaze straight forward towards a lucid disc through a lens, and move the card or lens about across the optic axis, perpendicularly to it—*taking care with the lens to keep its axis always parallel to itself or the optic*—we may affirm that a dark spot, owing to a blind place in the retina, or some thin object resting flat upon the visual sentient points, will not appear to move, and that the shadow of objects will seem to travel *in the same direction* as the card or lens with a perpetually increasing velocity, as they are situated nearer to the point of divergence. Whilst whatever objects are encountered by the convergent pencil will seem to make excursions in the *adverse* direction to what the lens does, with a swiftness continually decreasing as the object is further in advance of the point of convergence. We have merely to turn the lens upon its axis to rotate its contents so as to distinguish them from retinal objects.

True it is that we cannot see the remote disc through the lens. But in moving it *as advised*, the nearly parallel rays from the disc must keep passing every point in the substance of the lens in the same style, or all such rays for a given point in the glass would fall upon the cornea parallel to one another, and thus upon the same retinal point. So that the extremity of the short tube in which the lens is set, seen through or beyond the latter, or any of the numerous obstacles to the free passage of light, so common in glass lenses, will seem to abide *quite still* in their original sites, however widely the lens be borne across the optic axis, yielding *fixed points* for our regard to rest upon, of the most commodious sort. In other words, in these cross excursions, images due to the lens and those due to the retina suffer no displacement, whilst the rate of movement continually accelerates as the object imaged approaches the focal point, the movement being evinced in a retrograde manner with respect to that of the lens, for objects between it and the focal point, and with that of the lens for those between the focal point and the retina. Thus, in a case in which divergent pencils, especially if from points anterior to the eye, as in the usual entoptical methods, yielding parallaxes in the same direction, would produce no distinguishable difference, when there is room enough between the two bodies situated a little deeper one than the other in the eye, for a focal point to be carried transversly between them, their images appear to *fly asunder*. Just as when the line joining two foci derived from two lights is placed between the objects, this great opposite deflection may be witnessed by noticing on which side that image of each pair lies that vanishes when we shut off one light by the hand.

11. Yet, when we have thus provided ourselves with devices for determining the localities, sectional shapes and sizes of the bodies connected with the visual organs, which obtrude themselves upon our sight, we have not exhausted our means of research. Because the form and refractive power of a transparent body may appreciably influence the features of its image; and inflection of light occurs both at the edges of these and of opaque bodies; whilst all the bodies may reflect light. These different phenomena may happen more or less separately or mixed, may be distinct in their character, or may so far simulate another sort of phenomena as to make it difficult to distinguish them. But here, too, it will transpire that the use of the two kinds of pencils is of much greater efficacy than that of the divergent only. However, it will be more convenient to take the effects here alluded to into consideration in our actual analysis of the accidental phenomena, since we shall then be aided in our interpretations by comparing one with another. Nevertheless, it may not be amiss to premise a few words on a special topic or two claiming our attention in this study.

12. We notice, then, that the shadow of the orifice of the tube in which the lens is screwed, as seen on the distal side of the glass towards a round lucid disc, shows a series of concentric circles, or alternations of bright and dark *external* fringes from inflexion of light at the edge of the tube; that like external fringes surround the shadows of all the foreign particles that ostensibly stud the glass. In addition to which,

if the body be narrow, the very middle of its shadow is illuminated from inflection at its sides, even though there may be no other *internal* fringes strong enough to be thus visible. The central luminosity in the shadow of a small round particle being a round area of about the same brightness as the average light yielded by the lens—facts in accordance with ascertained laws of light. If we use an elongated disc, as a candle-flame, these effects must necessarily be best seen in objects that are parallel to the flame's length. Inflective phenomena in the convergent and divergent pencils resemble each other.

13. Phenomena of interference, by inflection of white light, are attended with manifestation of colour, and perhaps, as Sir D. Brewster affirms, by careful scrutiny we may discern inflective colours from ocular objects. But the appearances we are to investigate are so immersed in a pervading colouration, the offspring of ocular chromatic dispersion, that no other kind merits attention. Thus the image of the pupil obtained in white light from a lucid point, within the least focal distance of the eye, is a perfect *spectrum* of the point, whose middle is occupied by the most, and circumference by the least refrangible colour.

14. If we look through a puncture in a card, a straight narrow object, as a pin, passed across its axis a little without the card, only appears straight when it coincides with the diameter of the pupil's image, seeming to curve continually with a more marked concavity towards the centre, as it retires from it. An effect ascribed by Dr. T. Young to ocular refraction.* It may be appended that, from the same cause, the pin passed between the eye and puncture seems straight only at the centre of the pupil, becoming more *convex* towards it as it leaves it. These remarks may be applied (6) to our *other* convergent and divergent pencils respectively.

The ocular refractions gather the rays that enter the pupil from a radiant point just before the cornea, to a less image of it than it would otherwise have, and in like manner diminish the images of all ocular bodies; and as the eye is withdrawn from the radiant this diminution goes on, until, when it is able to bring the rays to a focus upon the retina, all the images vanish. There is a point about half of an inch from the cornea, where the rays from the radiant fall upon the retina parallel to one another, and which has been named the "anterior focus of the eye." And assuming that the rays also traverse the vitreous humor parallel to each other (in § IV. I shall demur to this supposition), the images of all bodies residing in it would equal in diameter the bodies themselves. Duncan measured these bodies in this way.

Listing looked through a puncture from one fixed external point to another, and observed how the images of ocular objects shifted their places in that of the pupil, and thus judged of their depths in the eye relative to that of the iris. Brewster,† by means of a lens of "very short focus," gets from two sources of light two radiant points, at an ascertainable interval near the cornea, so as to throw two images of a filament near the retina, "just in contact," in order to calculate the distance of the filament from the retina. It does not appear that he proposed to correct for ocular refractions. Donders afterwards modified the plan of these radiants just in advance of the cornea, to calculate the distances of objects more remote from the retina. He assumed the parallaxes of the iris and other objects to be respectively to each other as their retinal distances. This, however, could only really happen if the rays from each pencil passed from the iris to the retina parallel to each other.‡

§ II. *Eyelashes, Eyelids, and Conjunctival Fluids.*

15. The eyelashes, the most advanced of these, when immersed in either kind of pencil, exhibit diffractive effects like bodies spoken of in 11 and 12. The hair also reflects light into the eye, and when it does this from a small luminous disc, yields a divergent pencil, limited in one direction at least by the iris, according to the place of the disc, and more or less perfectly displaying the contents of the eye.

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* Bakerian Lecture, 1800. Phil. Trans., 1801, p. 68.

† Paper cit., p. 7.

‡ Allg. Encykl. d. Phys. s. 161-2.

16. The lubricating fluids are frequently so equably diffused over the conjunctiva, that our pencils fail to disclose them; but with fitting examples present we may see with divergent rays both opaque and illuminated pictures of drops of fluid, a contrariety announcing a difference in form, both drops being transparent, to the effect that the former are elevated and the latter depressed in the middle. Thus the convex tear (the more frequent kind) brightens its image and gives shade to its areola, by abstracting from the divergency of the rays that penetrate it, whilst in the other case the areola is brightened at the expense of the image. When these drops are immersed in a convergent pencil, the illuminated image becomes dark, and the darkened image bright, the convex tear, by increasing the convergency of the rays that pass through it, causes them to diverge sooner than the mass of rays, whilst in the other case they do not meet so early.

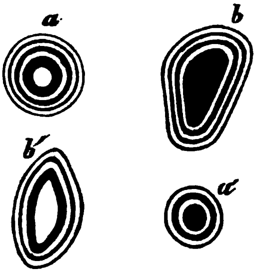

FIG. 2.—*a* and *b* are two drops of conjunctival fluid in a divergent, *a′* and *b′* the same in a convergent, pencil.

These little lenses, if we look at a candle-flame through a lens, tend to form erect or inverted images of it, according to their own form. They average from $\frac{1}{165}$ to $\frac{1}{200}$ of an inch in diameter (7).

The rays passing by and through these tears interfere with each other, so that a series of fringes surround the light and dark images; four, five, or more alternations may be counted, or in a flattish tear in a fine divergent pencil, perhaps twenty exquisitely fine ones. The illuminated middles of the images of the tears are broader and brighter for the size of the object than in the example of an opaque body, whose image preserves a like middle both in divergent and convergent rays (12).

These tears on the otherwise naked eye display round a candle-flame a series of wide brighter and darker rings, and in this common way obtrude upon us phenomena like those described above. A convex tear by the divergent pencil which proceeds from the image of the flame formed by it a little within the cornea, manifests beautifully the contents of the vitreous humour. The chromatic dispersion of the eye, combined with that of the tear, is usually obvious in the pencil from a tear. An eyelash seen through it against a flame often seems curved, if it falls laterally to the axis of the pencil, owing to the refractions of the tear and eye (14).

When the upper eyelid is lifted, we observe to follow it (upwards in a convergent and downwards in a divergent pencil), often, a series of drops. Helmholtz notes this upward movement, adding, "that when the upper eyelid is lifted, it draws after it the viscous mucosities, ("die zähen Schleimtheile nachzeiht.")* The conjunctiva must tend to drag off some of a limited quantity of any sort of fluid carried over it by attraction into the angle between it and the upper lid.

If the eyelids be kept closed by even a slight pressure from the finger for a while, the marks of the Meibomian glands and of the inequalities of the finger, will persist in the conjunctiva for some time; the true seat of the "curdled" appearance is easily detected by 3 and 4, and the nature of its inequalities analysed, if we wish.

17. Furthermore, whilst employing the divergent pencil, let an eyelid encroach upon it, and we observe along its shadow, which is disposed to bend its middle towards the

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* Allg. Encyk. d. Phys. s. 151. His description of these surface-phenomena from divergent light agrees well with the above; but I had anticipated him, except in the instance quoted.

centre of the pupil's image (14), and approaches from the opposite direction, a series of fringes; but if we quickly withdraw a lid which had rested awhile in one place, a luminous band bearing several fringes remains behind, longer than a luminous impression would be extant upon the retina, from a linear accumulation of fluid left by the retreating lid. The convergent pencil modifies these appearances as in other cases. Also, we are aware that a little bar of fluid, as it were a little prism, vertically concave in front from the attractions of the eye and lid, must gather along the edge of each lid, so that in fact the image of this bar in divergent light is projected upon the retina coincidently with the bright space which inflection at the lid would place next its shadow. Thus, by the intervention of the fluid bar, the rays that pass near the lid escape the usual refraction of the cornea, which would bend them towards the pupil's centre, and are either bent less or not at all, or deviate in the opposite direction, whilst light is moreover inflected both at the lid and fluid; besides which, there is a comparatively very weak beam of the pencil *reflected* from the fluid and lid, which is *really* thrown across the axis of the pencil towards that margin of the pupil which is on the side of the other lid, from the refracted beam, and may be easily seen as the lid just arrives at the margin of the pupil.

To the refractive powers of the fluid bar are due the upward and downward streams of light that issue from flames when the lower and upper lids respectively advance upon the cornea; to these, with the said inflections, are owing the supernumerary crescents when the lids are winked together nearly parallel to a new moon. If we carry from the lid the back of a knife over, whilst the blade is perpendicular to, the cornea, with a candle so placed that the knife reflects a beam of light into the eye, when we let the back rest on the cornea, the fluid will instantly form the refracting prism described, and corresponding phenomena result.*

The streams are limited pencils, diverging from the eye's surface. By nearly closing the lids we may get such a pencil through the small aperture thus produced. On account of the eyelashes, more than one such orifice may result, or there may be at the same time, if there be a disc of light before us, a pencil from a tear or from reflection at one of the hairs. All these manifest the contents of the deep parts of the eye with its chromatic aberration.

§ III. *Iris and Crystalline Lens.*

20. Neither the conjunctiva nor the cornea seems to contain any bodies to transmit shadows distinguishable from irregularities on the surface and the fluids thereon, nor can anything be seen in the aqueous humour.

But at a due depth from the tears (8) we encounter the iris, and in a pencil of rays whose focal point is not too near to it, we observe a series of inflective fringes; in light from a brilliant point in advance of the eye, even closing in rings upon the very centre of the pupil's image. With me as with others, this image is not circular. I find it of an oval figure, rather, whose axis is vertical,—and somewhat differing for my two eyes.

In my left eye, apparently commencing at the same depth as the iris, and ranging through a depth fully equal to that of the crystalline lens, I meet with many small opaque bodies of irregular outline, fixed in the globe. They are evidently scattered through the substance of the lens. The lens of my right eye is much freer from such objects, but, on the other hand, it is more remarkable for displaying several larger objects which are near its posterior face. One or two of these seem semi-opaque, whilst there are five or six discs of a higher refracting power than the general substance of the lens, and situated more away from its axis. If (speaking in terms of an inch) I introduce (7 and 8) a pair of foci $\frac{1}{150}$ apart as far as the optic centre, so that in the convergent pencils the shadows of a tear ($\frac{1}{35}$ from the said centre) are *retinally* $\frac{1}{50}$ apart, and those of the iris ($\frac{17}{150}$ from said centre) $\frac{1}{25}$, these large objects appear in the divergent pencils with shadows showing not more than a retinal separation of their centres of $\frac{1}{15}$, which places them as far back from the iris as the posterior vertex of the lens, which should be $\frac{2}{45}$ from the optic centre. Two of these objects have real diameters of $\frac{1}{350}$ and $\frac{1}{400}$. They may be masses of cholesterine or fat like the microscope has encountered in the crystalline, and may have some connexion with the capsule of the lens. The smaller opaque bodies may be earthy particles. They seem to undergo no change. I have been acquainted with two of the refracting specks for eighteen years.

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* In London Medical Gazette, April 21, 1848, I show (in detail how each bright stream is due to a "prism" so to speak. or fragment of a concave lens of fluid" on a lid. Helmholtz, *op. cit.* s. 146, ascribes the explanation to Meyer, 1858, Pogg. Ann. lxxxix. s. 429. The pale reflected beam, as far as I am aware, has not been described by any one but myself. It was formerly supposed that the bright beam itself was from reflection at the lid.

With a fine divergent pencil a tuft of bright lines, of a straighter character in my right eye than in the left, radiate from about the middle of the pupil, whilst finer ones radiate between them or arise from them. The largest of these lines plainly carry diffractive fringes, and in a convergent pencil form shady dark lines. They are found on either surface of the lens, and (as Dr. T. Young suggested) are clearly caused by the stellate structure of that body, as presented at its two surfaces. Whilst the phenomena mentioned seem to show that the modified substance of which the star is composed must project in its middle line a little above the general face of the lens, so as to constitute a refracting ridge, if we may argue from the strongest (whose ridge may be $\frac{1}{2000}$ of an inch in breadth) lines to the weakest ones, for the effects are so delicate, that they vanish away in most of the examples, on the nigh approach of the focus of our exploring lens. From intervening between the anterior face and the retina, the vertex and limbs of the posterior star are the more visible, the limbs of the other seeming to peep between the hinder ones. The image of the predominant vertex usually deviates from the middle of the pupil In my case, for a lucid point at my reading distance, it *appears* as if lying at about $\frac{1}{3}$ of the vertical axis of the pupil from its *apparent* lower edge, and in my left eye slightly towards the *apparent* left edge. Besides, there is a pervading dotted faint cloudiness of the lens or capsule, which it would hardly be possible to analyse.

If we introduce a pair of foci to the posterior surface of the crystalline lens, and in equation (b) from 2 in the sense of 4, make $F'\,G' = \frac{2}{3}$, and $E'\,F' = \frac{1}{6}$ of the optic axis, we find that an object falling in the pair of convergent pencils at the anterior surface of the lens, would have $C'\,D'$, the separation of its retinal shadows, equal to $5\ A\,B$. If we now suppose the retina to retreat from the lens by $\frac{1}{7}$ of the optic axis, or $F'\,G' = \frac{17}{21}$, $C'\,D' = \frac{123}{21} = 6\ A\,B$ nearly, giving a separation of the two shadows $\frac{1}{5}$th further asunder. By comparing the separation of the shadows with that between two of the globules that are observed to be attached to the hyaloid by short threads, we might readily detect such a deviation as the one imagined, did it take place. It is deduced by Dr. Young that the accommodation of the eye from vision for infinite distance to that of nearest distinct vision, through an alteration in the length of the optic axis, would require the axis to be lengthened by one seventh of itself.* It thus appears that did all this extension occur between the lens and the retina, which the properties of the latter forbid us to accept, we could entoptically detect such an alteration. But if the axis lengthen much in its anterior portion, so that objects in or upon the eye move much, where we can easily bring the foci into their vicinity, it will be found by substituting the proper numbers in equations (a) or (b), that *very* considerable deviations in the separations of the pair of shadows result. In a word, any change in distance between a tear and the iris, between the latter and a speck in the lens, between any two of these specks themselves, at all approaching one-seventh of the optic axis, could not fail to be observed by the methods adduced. Hence the fact of our not being able to *readily ascertain* (I am not sure that we may not detect *some* movement between the tears and iris or bodies in the lens) that the pair of shadows after the accommodation, do vary their apparent distance from each other, in a negative fashion, supports the opinion of Young, Helmholtz, &c., that focal adaptation takes place by a change in the *form* of the lens, as that supposition is the only one which requires a very minute ($\frac{1}{50}$th of an inch in the lens, says Young) alteration in length along the optic axis.

23. Young, and all subsequent observers, notice a want of symmetry in ocular refractions, so that the focal adaptation which would bring the rays from a lucid point which fall in the vertical plane most accurately to a point, is not the same as that which brings

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* Phil. Trans., 1801, p. 53.

those of a transverse plane most so. Young ascribed this to the influence of the crystalline lens.

24. The inflective fringes of the edge of the iris involved in ocular chromatic dispersion, and the stellate figure of the crystalline lens, wherever any portions happen to be parallel to a linear bright space, tend to arrange themselves along it, so as to form supplementary images of it, to do this, through the boundary lines, to letters in a book. And wherever a series of alternations of dark and white lines occur of a certain breadth, especially if the lines be curved, to cause such effects to cross one another. Thus, whenever we stare at such a series until the pupil dilates and the eye loses its focal adjustment for their distance from it, a very singular exhibition of broken, or intercurrent white and coloured lines results.*

§ IV. *The Vitreous Body.*

25. Perhaps it would help my readers to a readier apprehension of the chief points to be kept before the mind in investigating the nature of the vitreous body, if I place before them, as preliminary to entering upon details, something like a definition descriptive of my views of the visible texture floating within it, by which entoptical researches are guided, and the structural inferences to which I have been led after a diligent study of its movements.

From the walls of the cavity behind the crystalline lens, as far as it is lined by hyaloid membrane, there springs into view a lax network of beaded fibre, which is the frame of an invisible membrane that divides the peripheric portion of the vitreous into a certain number of little chambers, separating them from each other, and from a larger middle one.

These compartments are filled with fluids of graduated density, in such order, that the densest lies next the capsule of the lens, and the rarest next the retina, so that the vitreous body is a compound optical instrument, whose anterior constituents excel the posterior in refractive power.

26. Before we enter upon the exploration of our visual organs for the verification of this statement, let us consider for a moment what forces are likely to act upon a reticulation thus attached in a cavity filled with fluid, so as to occasion its parts to migrate.

First, it must be remembered that when the globe rotates, the fluids within will relatively rotate. No matter about what axis the rotation ensues, whether about one through the globe's centre by turning in its orbit, or about any other by the movement of the head, or of our whole person, the vitreous fluid will strive to abide absolutely at rest; or, translation of the orbit apart, rotate within its cavity, through the same angle. But in consequence of the obstructions from the web, if not from some friction between the fluid and the parietes of the cavity, the latter must ultimately concur in the rotation. And as to the objects visible in the fluid, their connexions must exercise control over their movements. Thus we have a modified result, or upon the whole, the objects in the vitreous will travel in the direction of the rotation, but will start from their places at later respective instants than the beginning of that act. Then go equally with the rate of rotation. And when the ocular rotation is arrested, they will continue theirs awhile longer, through the *inertia* of the now rotating fluid, until the fluid ceases to move, or until they reach the ends of their tethers; when, after a momentary pause, they tend to regain their original sites by retraction, or the web's elasticity. Independently of these connexions, rotation of the fluid in its vessel would cause the objects nearest the wall to journey through the greatest linear spaces, and the others gradually through less, unless at the centre of the entire chamber they would remain at rest.

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* As I gather from Helmholtz's reproduction (Encyk. cit., s. 152), Listing (Beit. z. phys. Optik. 1845) pictures the entoptical characteristics of the crystalline lens just as I have done. But the larger bodies which he describes corresponding to those that I have above given, he connects with the *anterior* portion of the capsule, and apparently also the smaller ones which I regard as dispersed throughout the lens. He speaks of the stellate figure as owing to "dark radial lines." He also sketches a sort of "irregular star" of bright stripes (*Streifen*) issuing from near the middle of the pupil, which he conjectures to have been occasioned by the separation of the capsule from the cornea in the fœtal state. These nice distinctions with the entoptical method he used cannot, I am persuaded, be safely made, and I am rather disposed to suspect that even the irregular star was an aspect of the true stellate figure on one of the faces of the lens. Helmholtz seems to imply that the manifold images of lines, &c., through the stellate figure was explained by Gut (Über Diplopia monophthalmica. Dissert. Zürich, 1854). I do not know if Gut, or any other observer, had noticed like effects from the iris.

Though the principles here sketched are found on trial to be generally agreeable with the movements of the bodies visible in the vitreous, yet we discover that their operation is disturbed by the influence of a force causing movements solely in the vertical direction, *at the instant*, whatever point of the eye's circumference happens to be then uppermost; and which must therefore proceed from a difference in specific gravity of some of the contents of the vessel.

If the axis of the eye be horizontal, whether we stand erect or lie on either side, or let the head depend, and we now look to a higher point, the objects in the back part of the vitreous will *actually* (6) rise, as far as their connexions permit, exceeding the angle of rotation far beyond what happens in horizontal rotations, through a considerably greater angle than the axis has turned through; whilst those in the front part of the vitreous will *actually* descend through angles equal to those risen through by the former objects, which are severally at corresponding distances from the wall of the cavity. But if from a horizontal direction of rest we turn the regard to some lower point, both the anterior and posterior objects so accompany the movement as not to rotate at all in the eye. And similar effects respectively take place either in looking from a lower to a higher point, or *vice versâ*, no matter through how great an angle.

Hence the posterior objects are, relatively, so strongly buoyed up, and the anterior ones pulled down by a difference in specific gravity in some of the contents of the entire chamber, as to more than counterbalance the effects of friction in the rotatory act.

Nevertheless, with the optic axis vertical, whether we look upwards or downwards, neither the objects nearest the foramen centrale retinæ, nor those nearest the lens, approach appreciably, or depart from the wall of the vessel respectively nearest them. And it is as much as ever we can detect a very slight vertical movement in objects rather further removed from the wall than either of the examples mentioned. As, therefore, the difference in specific gravity which we have found to exist does not manifest itself decidedly under circumstances when it must have a great tendency to do so, the objects we regard must, immediately or mediately, in some fashion be tied to the parietes of the cavity.

When we wish a deliberate inspection of an intra-vitreous body, we had better look downwards, because this is a direction agreeable to the ocular muscles, and the body will remain steady whilst we survey it.

In setting about the scrutiny in question, the first glance conveys to the mind the impression that the floating bodies we behold are a confused mixture of globules and fragmentary filaments. The most cursory methodical exploration apprises us that all these behind the crystalline lens, and by 3 and 4 we resolve that whatever be the breadths of the shadows, under various conditions, all, from front to back of the vitreous, whether filaments or globules, have a general agreement in breadth, so that both in a divergent and convergent pencil increased magnitude in the image shows a less distance from the focal point we employ; though in any given region globules or filaments, side by side, at the same focal or retinal distance may differ *appreciably* in size.

If, to get more accurate information (speaking in terms of an inch), I place a pair of foci $\frac{1}{180}$ apart at the optice centre (7 and 20), the formost fibre in the vitreous, casts in the divergent pencils shadows whose middle lines are *retinally* $\frac{1}{25}$ apart, and $\frac{1}{160}$ broad. It is thus $\frac{1}{14}$ from said centre, or $\frac{1}{37}$ from the lens, and has a real diameter $\frac{1}{840}$. The anterior current of fibres seems to extend to fully $\frac{1}{8}$ from the lens, and the breadths of the main ones to be about that of the example chosen, or a little less. With foci $\frac{1}{90}$ apart, one of the most advanced fibres of the posterior current, retinally shows shadows $\frac{1}{373}$ apart and $\frac{1}{750}$ broad; another, behind this one, shadows $\frac{1}{1500}$ apart and $\frac{1}{1600}$ broad. These are therefore $\frac{1}{8}$ and $\frac{1}{60}$ from the retina, and have real breadths of $\frac{1}{930}$ and $\frac{1}{1000}$. I may see a fibre lying over the *punctum aureum retinæ* within $\frac{1}{60}$ of it, having a parallax little more than the central vessels of the retina, which are about $\frac{1}{100}$ from the *sentient surface*, from which we are, in fact, measuring, on movements of a pencil parallel to the retina (10). We must guard ourselves against taking for globules little vascular dots or specks in the hyaloid or tissues between it and the sentients which are brought into view by such a movement as will be explained in the next section.

The image of the globule in a divergent pencil is a bright circular area surrounded by diffractive fringes; in a convergent one, a dark circular area, within a fringed bright areola. The globule, therefore, is transparent, and of greater refractive power than the fluid in which it floats; but it cannot be of much greater, for were it so globules residing near the retina would evince shady centres when we receive into the eye rays from a lucid point placed as far, or nearly as far, away from it as its anterior focus (14); for they should then be able to bring the rays that pass through them to a focus nearer to themselves than to the retinal seat of their image, and thus to project them away from the image's middle portion. However, in such a case, we may observe globules near the lens with obscure middles; and what has been said of the globule may be considered to apply to the filaments.

The objects near the crystalline lens are easy to see, and therefore could not be attached to its capsule without its being noticed. Whereas, on the contrary, every bead or filament thereabouts sweeps across the pupil, however broad this be, without any check.

If we now single out, with a divergent pencil, one of the smallest images, or a bead nearest the sentient points of the retina, and move the eye about gently, we can note that it accompanies the movement; whilst others near it, with broader shadows, pass over it and swim about. If the eye rotate through a very small angle, it does the same, though the others referred to remain at rest, not *yet* having been set in motion. And when the eyeball rotates sufficiently to cause the mass of globules, less near the retina, to appear to move, they will proceed even after the eye has ceased to move, and will regain their places by apparent retraction; whereas, the object we are especially watching moves, if not exactly, *all but exactly*, with the eye, and stops with the eye. If we reflect that the smallest instance of images ever lessening for more posterior objects scarcely expresses the lucid and shady contrasts which reveal it, we can well understand that a like object behind the one that yields such an image would not be discernible at all. So that if all the forthcoming observations shall be found to harmonize with the notion, we may conjecture that a chain of one or more invisible beads is the mode of connexion with the parietal membrane, ending in the "*decumbent nuclei*" that histologists find thereon. Especially if there is reason for believing that none of these threads spring from that portion of the membrane that covers the *punctum aureum retinæ.*

If we now turn in succession to objects at different depths in the humour, we observe that their several excursions from positions of rest, consequent upon ocular movement, are in accordance with what we should expect from objects tied together as imagined, under the dynamical influences actually in play. In so far that, though we might never be able to divine how the seemingly solitary globules are retained in their relative places, we might be sure that they are parts of a *system.*

But if we repeat the examinations of these objects in various postures of the head; if we rotate the eye in all the positions it can take, gently, quickly, and strenuously, seizing every advantage for getting as extensive and diversified a sight as possible of the contents of the vitreous, then we shall readily come to *see* that a host of fibres start from the hyaloid, from place to place, as far over it as the super-imposed retina has a moderate sensibility, and congregate into fibres that stretch out into the fluid. Thus, reversing the description for a given instance, by the gradual tapering of the image through variation in retinal distance, in a divergent pencil, a fibre coming from the depths of the vitreous when the eye is prone, to attach itself above, wears the aspect of a trunk of a tree throwing out branches and twigs, holding by the ultimate subdivisions to the retina, so that in a vehement rotation of the eye the tree seems to fly along space, revolving on its twig-tips. And further, it requires but the bestowal of a little pains to make ourselves conversant with the skeleton of a fibrous network stretching all along the walls of the vessel.

Nor can we have omitted to notice that many of the filaments present to us uninterrupted ranks of beads of much length. If closely observant, and our pencils of the best, we perceive that every filament in a plane parallel to the back of the eye, or crossing the axis of either kind of pencil at right angles to it—that is, *at least approximately intersecting the rays of the pencil in this style*, is resolved into a thread of beads. If the filament bends in such a plane, the consequent crowding together of the beads on the concave side is manifest.

Fig. 3.

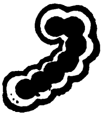

Fig. 4.

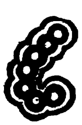

FIG. 3.—A portion of a filament in a convergent pencil.
FIG. 4.—The same in a divergent pencil. In the latter case we may often see three or four dark inflective fringes, instead of one only, as here indicated.

Now, if a filament at any part quits such a plane, the images of two or more portions of it, which may have afforded each other reflected and refracted rays, must be projected upon the same retinal spot; and considering that the filaments may cut the vertical transverse plane at every angle, that they may have every degree of tortuosity, are exceedingly plentiful, and the beads innumerable, the interference of their projections with one another must be of inconceivable frequency, and the resulting phenomena in all possible ways diversified. In such a case, it may be observed, it is neither the object which first intercepts a pencil of rays, nor that which last falls in their path, which is revealed by preference; as a rule, it is that whose image has the most forcible contrasts in bright and dark parts; in a divergent pencil, more commonly, though not necessarily, the hindmost object. Ordinarily neither image extinguishes the other, and even all the bright and dark spots of several such images may be distinguished at a retinal spot. Yet events of another character may accrue: a couple of images may own a common centre, and coincide in their concentric fringes, generating an image of intensified contrasts. This effect is often produced if a filament bends sharply forwards. Again, the reverse of this happens by several images of beads dropping upon one another in such a fashion as to slur all the contrasts together, to their entire obliteration. Thus we have a continuous perfectly beaded thread severed into pieces, comprising many beads, but a few, or even single beads. By causing a fibre to twist in the eye, or by bringing the images of anterior beads of a fibre inclining forwards from the retina, over those of posterior ones, by aid of their different parallaxes when we move the focus of our exploring pencil from side to side, up and down, and as occasion requires, we may produce at pleasure phenomena of all the sorts that are here sketched.

Keeping, then, this hint in mind, and perseveringly disentangling the fibres, we may soon perceive that they are all mere strings of beads, which coalesce by contracted surfaces. Here and there we may note some stouter than the rest, with perhaps their beads more broadly united, constituting, as it were, a stronger framework ramifying through the general web.

But it may be better to speak directly of what I find in my own eyes. I have, then, quite familiarized myself with the web—having a systemic but no further similarity to each other in my two eyes. I can at once locate in the tissue any thread that comes into view, and any globule that tumbles upon the sight I can restore to its place among the meshes of the web with facility. Again and again I have succeeded in stretching a host of seemingly disseminated beads into a tuft of neat fibres. And a number of them scattered separately among the meshes I have been able to distinctly assign to one or more crumpled fibres twined in among the rest. And if I have occasionally been unable to get a continuous inspection of certain fibres confined by others, I have dotted out their course satisfactorily by the line which the projected beads indicate. Whenever I have been able to cast a swarm of plain circular images tolerably free from coincident projections, I have not failed to discover that they are constituents of fibres. In all the examples of the anterior portion of the vitreous that fall within the reach of our convergent pencil, it is quite obvious that there is not a stray bead, and that all the fibres are series of beads.

Nevertheless, in the immense multitude I do fall in with some beads so involved among fibres, or so widely away from the axis of vision, as scarcely to afford an opportunity for the establishment of their being items in a series; and one or two beads, the nearest to the most sensible portion of the retina, where no contiguous ones are apparent, but which may be presumed to be merely displaying unusually strong images in a position where ordinary ones are not discernible, and these are plainly held in their places in the fluid by some means or other. So that, from all these observations I feel all but absolutely certain that every bead forms a link in a fibre, and certain that there are no loose beads in the vitreous.

The net work, I see, is not a mere medley of fibres in a space of three dimensions. Arising thickly, but not indifferently, from over the surface of the hyaloid, they unite in detached groups, and the junction of these groups in the body of the fluid is not effected by fibres running in indiscriminate directions, but by fibres interlacing and anastomosing in such fashion as to form trellised *sheets* of tissue, some of the sheets being so replete with threads as to be almost a cloth of them, whilst others show them with considerable intervals.

Insomuch that these sheets, or fibrous lamellæ, are arranged like rudely latticed dissepiments, traversing in a more or less continuous expansion the vitreous, with more freedom of movement as they reach further from its wall, along which they chiefly prevail, only extending into the interior obliquely, Bowman's account of the appearance of the vitreous body when prepared in chromic acid, involves a fair notion of what I am striving to convey. "A few concentric lamellæ externally, to which succeed very irregular radiating septa, and lastly, an irregular central cavity." Only the word concentric must only import that from the extension of the lamellæ near the wall, much of it takes a direction nearly parallel to it; the fibres run as if in the septa of a number of cells, whose mean shape is expanded parallel to the parietes of the vessel, and compressed in a direction perpendicular to that, just as the elastic fibre of histologists is known to do in serous membranes, or homogeneous connective tissue, of which sort of fibre they would seem to be. These reticulated septa are woven, as it were, from the hyaloid, but span the orifice of the pupil without springing from the capsule of the crystalline. Nevertheless, no membrane is visible in the meshes, nor could any homogeneous material of this sort be seen, if existing. Nor is there any granular deposit along or near the fibres to be seen, for what may wear a *primâ facie* aspect of such, it is easy to detect, only proceeds from a peculiar overlying of the images of ordinary beads, which is often but transitory.

Such seeming, then, to be all the results obtainable by detailed inspection of the web, let us bring forward the observations on difference of specific gravity, which were left imperfectly interpreted, and try whether the unknown in the two cases may be arrived at by considering them together.

We shall hardly be inclined to conjecture that a fibre that looks precisely alike in all regions should be lighter than the vitreous fluid posteriorly, and heavier anteriorly, or that it, together with membrane in which it runs, is so. If thus, and the fluid freely communicates with itself throughout the vessel, when we look downwards the fibres in the vicinity of the lens would fall down upon it, and those near the retinal centre mount up against it. If the fibre runs in septa which completely divide the vitreous into shut cells, it might not be possible to refute the supposition before us, but its intrinsic improbability will prevent us from entertaining it.

In a former attempt to account for the see-saw motion from specific gravity, I suggested it might be attributable to an excess of light fibre in the back of the vitreous, overcoming the buoyancy of that in the front, pulling down the less quantity, by a series of attachments of the expanded web to the hyaloid, as if over a line of pulleys. But a renewed examination leaves me no room to think that any posterior excess exists, whilst in a freely communicating fluid the objection urged as fatal to the foregoing hypothesis applies equally well to this; and if the fluid be shut into separate bags, then, should we lie on the back, and incline the head but a little to one side, the strain of a posterior excess of buoyancy should fail to exert material force upon the anterior fibre; yet never can I witness any movement from gravity in one set of fibres without an equal opposite one in the other set. In a word, the endeavour to explain the see-saw phenomenon by imagining a difference in the specific gravities of the fibre and fluid, I feel compelled to abandon as a hopeless task.

However, it is left us to assume that the vitreous body is an aggregate of little bags which are filled with fluids of graduated density, the heavier in any two examples, always lying nearer the lens. The sole hypothesis which, as far as I can see into the matter, meets the requirements before us.

Fluids shut in lax bags admit a contortion of these sufficient to yield all the motions the web exhibits to us. With the optic axis in the horizon or inclined to it, the sinking of the heavier bags and rising of the lighter, would render the see saw phenomena with precision.

If we bend our gaze downwards, the heavier and lighter bags will tend, by their relative densities, to keep their places in the eye; and there does occur a certain dropping of the fibres running laterally from the centre of the retina, as if a bubble of fluid bounded by them were striving to reach the surface.

Should we look upwards, the heavier bags cannot descend, because they are bound down on either side round the lens, and the lighter ones cannot ascend for a like reason; yet I think a certain direction of the lateral fibres indicates an arching upwards of the reticulated tissue near the middle of the retina in this case, whilst nice observation in this instance or the former may enable us to discern equivalent displacements of the tissues spanning the lens.

It seems immaterial for this hypothesis whether we regard those portions of the vitreous fluid removed from our easy inspection, most distant from the optic axis, to be kept in cells apart from that in the middle portion, or not. But a lighter fluid may lie over *all* parts of the retina, if rarer layers *embrace* each other *from* the lens *towards* the retina.

In this manner, then, I have been led to the conclusion that the vitreous body is a compound concavo-convex lens, so constructed as in one respect to be a fluid imitation of the crystalline, a prolongation, as it were, of its series of posterior layers; which are formed of material of gradually diminishing refractive power, whilst the decreasing series is here inferred to be carried through the vitreous to the very retina, thus the middle of the crystalline is the centre of an easy succession of refractions, for the pencils of rays that are destined to impinge in foci upon the retina.*

(*To be concluded in our next.*)

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* Though my earliest entoptical attempt in treating of the vitreous was burdened with the defect of overlooking the anterior cross-current of muscæ, yet I will venture to quote the words, in which it sums up, for comparison with the conclusions of other inquiries (London Med. Gaz., vol. xxxvi. p. 101): "The more I consider their connected chain-like character, their branches thrown out in so many different directions, and to such great lengths, the different parts of these figures changing their relative positions but very little, so that each *musca* has, so to speak, a home in the vitreous, to which after each disturbance it finally returns, the more I feel disposed to regard them as fringes or processes of the hyaloid membrane, or as deposits in it"—that is, in the septa of the cell-divided fluid." I owned myself puzzled with these views to reconcile the obvious effects of the "*vis inertia* of the vitreous itself" with the apparent "buoyancy" of the *muscæ*. Brewster (paper cited) described both currents of *muscæ*, termed the images diffractive, and *not* refractive, offered a numerical retinal distance of a filament with "the diameter of its shadow," called them specifically lighter than the fluid, ignoring or overlooking all the anterior cross movements, except what arise from rotation,—took the beaded fibres for hollow tubes with scattered globules inside, perhaps "the remains of vessels," as existing in detached and floating portions."—In a word, these *muscæ* as fragmentary knotted filaments subject to increase and decrease within his experience, floating freely within a few cells with invisible membranous walls, into which he divided the vitreous. As I learn form Helmholtz (Encyk. cit., s. 158), who quotes them and adds his own observations. Donders (Nederlandsch Lancet 1846) and Doncan (De Corp. Vitr. Struct. Dissert. Trajecti ad Rhenum, 1854) next investigate the vitreous, the latter of whom compares what he thus sees with what he observes under the microscope. The fine globules, (Donders) graduated threads (*mit Körnern besetzt*), aggregated granules (*Körnerhaufen*), and membranous-like folds "isolated," or else attached to other forms or to the walls of the cavity, all of which are lighter than the fluid.—register numerically the sizes and places of the objects. Finally, they like Brewster, regard what they see as remnants of fœtal structure, but of the very internal framework of the vitreous, (*dies Gebilde sind Reste des embryonalen Baues des Glaskörpers*). Afterwards I described all the objects of the vitreous as belonging to beaded fibres forming one system, and recognised the fact that it is a *relative* movement from gravity of the anterior and posterior fibres that called for explanation. However it is quite enough to glance into the accounts of different observers to perceive that the contents of the vitreous in all the individuals are alike.

Art. II.

On the Treatment of Tetanus. By Campbell de Morgan, Surgeon to the Middlesex Hospital.

In a valuable report by Mr. Poland of "seventy-two cases of tetanus occurring in Guy's Hospital," published in the 'Guy's Hospital Reports,' the author adopts the view generally entertained with regard to the efficacy of remedies in this disease.

> "We can hardly," he observes, "call the medicines administered in the recoveries 'remedies;' they appear merely to have rendered the patient more able to fight against the battle of spasmodic action. Tetanus runs a certain course; it has its period of accession, its height and intense activity, and its gradual decline; it often kills before it reaches its active point; and often kills by exhaustion during its decline. Nothing seems to check its regular course; there is no control in its unvarying and undaunted career; it will have its sway. All we can do is to enable our patient to weather out the storm, by giving him as much strength as possible, and not adding fuel to the fire by all sorts of applications and internal remedies, which have over and over again signally failed."

This, which was the opinion of Hunter, and which is probably the opinion of most who have tried to combat this terrible disease, has been but too well justified by experience. But it is one which should not be pressed so far as to drive us to the conclusion that, because as yet there has been only failure, we must not look forward to success. On the contrary, should we not rather endeavour, by repeated experiment, to find some remedies from which positive results may be obtained? With a view to leading the profession to a more efficient trial of remedies, which, judging from the reported cases, have hardly as yet been fairly tested, the following case, with the observations upon it, has been drawn up:

Henry Blackwin, aged fifteen, employed at a coal shed, was admitted into the Middlesex Hospital on the 16th of September, 1858, with symptoms of trismus. He is short, but well formed and strong, and has had, by all accounts, very good health.

On the 30th of August, while walking, he trod on a large rusty nail, and the point of it pierced through the thin boot he had on, and ran into the right foot just at the base of the middle toe. There was little bleeding from the wound.

On the following day he came to the hospital, walking on the heel of the wounded foot in consequence of the pain, and aiding himself with a stick. A poultice was ordered; a gathering formed in the part, which broke on the 4th of September, and the relief was so great that he was able to walk about. On the 7th the place was quite healed, and he discontinued his attendance. On the 8th he felt a stiffness about the jaw, which got so much worse that on the 12th he was unable to open his mouth. He felt some stiffness at the back of the neck, and pain down the back to such an extent as to interfere with his walking. He had some difficulty in swallowing. On the 13th he took to his bed. On the 15th he was unable to open his mouth or move his head, but he had no twitchings in his limbs. For two or three nights he had not had any sleep, but dozed off occasionally in the daytime. The bowels had beeen opened daily.

On the morning of the 16th he was brought to the Hospital; the trismus was very severe, and he was unable to move his head in any direction. He perspired freely, and complained of great pain down the back; but there was no opisthotonos. The abdominal muscles were very tense; pulse, eighty. He was ordered a castor-oil injection, and linseed-meal poultice down the back to which was added a lotion of chloroform, aconite, and opium. A draught containing paregoric and the liquor opii sedativus was given every six hours; broth diet and strong beef tea.

The injection only brought away a few scybala. At about ten P.M., on waking from a sleep of two hours, he had a spasm, which caused slight opisthotonos and great difficulty of breathing. The spasm lasted only a short time, and was followed by a copious perspiration. He slept afterwards for two hours.

17th. Feels more comfortable, is in less pain, and the expression of the face more natural.

Ordered: Olei tiglii, ♏j statim; tinct. aconiti, ♏v 3tiis horis; mist. vini gallici, ℥ij 4tis horis; essence of beef tea, two eggs, and milk Oj.

18th. Has had a fair night; but the trismus is more marked, and there is more pain down the back; the abdominal muscles are again very rigid. There is no pain in the foot.

He was in this condition when I first saw him on my return to town. Hitherto he had been under the care of my colleague, Mr. Flower. The symptoms since the morning had been more marked, and were gaining ground. For reasons hereafter mentioned, I ordered him at once the one-tenth of a grain of strychnine every two hours, the symptoms to be carefully watched, and the medicine to be omitted so soon as any effects from it were observed. The diet to remain as before.

In the evening the muscles of the back were very rigid; he was unable to bend his knees, and there was from time to time slight opisthotonos. He complained of twitchings in the thighs, which prevented his sleeping. These symptoms becoming more marked, the medicine was discontinued after the second dose.

19th, 1 P.M. The symptoms are more marked, the spasms at times being very severe, though during the night he had an hour or two of sleep. Ordered to resume the medicine.

10 P.M. The spasms have been increasing in severity. He has had six or seven violent paroxysms during the day, and has had continued suffering; pulse, one hundred and six; face and shoulders perspiring freely. The medicine to be taken during the night in half doses (one-twentieth grain of strychnine).

20th. Has had no continued sleep; on dozing off has been startled by violent spasms of the hands and arms; the body rigid throughout. This morning at about five had a paroxysm so severe that he was near death from asphyxia. He has great difficulty in swallowing. The pains down the thighs and in the abdominal muscles very severe. A turpentine enema to be used, and the strychnine to be given in its former dose, one-tenth of a grain.

The enema acted well, but the spasms were increasing in frequency and violence. After the second dose the medicine was stopped; the pulse, one hundred and thirty-five; the catchings in the hands constant.—Eleven P.M. The symptoms were now becoming so urgent that the strychnine treatment could not be longer tried. It was evident that though it was producing its own specific effect, the paroxysms of the disease were in no way relieved, nor were the chronic spasms at all diminished.

That the symptoms were due in great measure to the disease was evident from the fact that the paroxysms did not correspond in time or severity with the administration of the strychnine. The symptoms increased in severity for fourteen hours during which no medicine was given; and at the time when the paroxysms were most severe, they became milder during periods when the medicine was still being given; whereas, when strychnine is the cause of similar symptoms, the fits correspond in time and severity with the reception of new portions of the poison. During the whole time, moreover, the chronic rigidity of the trunk and lower limbs was becoming progressively more intense.

The strychnine, therefore, was left off, and the patient was again put upon aconite, of which five minims of the pharmacopœia tincture were given every two hours, and as the pains in the thighs were very severe, a liniment composed of equal parts of tinct. aconite and camphor liniment was directed to be rubbed into them.

21st. Has had some pretty severe attacks of spasm during the night, but says he feels more comfortable; the pains down the thighs are very acute. The whole body is still quite rigid. Has taken seven doses of the tincture, but feels no effect from it. It was ordered to be continued in eight minim doses; pulse, ninety. In the evening he complained of a feeling of soreness in the throat; otherwise, during the day, he had been freer from spasms. Ordered to take only half-doses (four minims) during the night.

22nd. Has had a somewhat better night, without any severe paroxysm; but the body is still perfectly rigid, and the pain is as severe in the thighs; less perspiration; pulse, sixty. Tinct. aconiti, ♏viij 2ndis horis.

23rd. Remains much the same, but his appearance is better; to have a turpentine enema, and to take Fleming's tincture of aconite, ♏v every two hours, as before.

24th. The pain in thighs continues so severe that an enema containing ♏x of tinct. aconiti was ordered, but it did not relieve him. In other respects he is doing well. He has lost the active spasms, but the chronic rigidity is as great; pulse, sixty; has no sensations of pricking in the hands, or any unusual feelings in the body generally. An ointment of one part of ext. belladonna and two of opium was ordered to be rubbed into the thighs, and this gave him some relief.

From this time the improvement was progressive, the countenance becoming more natural, and the pain and anxiety diminishing. The pulse remained steadily at from sixty to sixty-five, but he had no symptom indicative of the large quantity of aconite he was taking. On the 27th the medicine was given every four hours only; on the 28th he could open the mouth a little; the aconite was given every six hours, and on the 29th three times a-day. He continued to take the medicine to this extent till the 4th of October, when it was left off altogether. At this time he could feed himself and move his limbs freely; the pain had entirely left him; the abdominal muscles were still tense; the countenance natural. A little hardness about the abdomen remained for some time longer, and it was not till about the 10th that he could open his mouth freely. He was kept in the hospital till the 2nd of November, and was discharged in perfect health.

There are some points connected with the treatment in this case which seem to me worthy of special remark. The disease did not show itself in a severe form. The symptoms set in gradually, and some days elapsed, after the stiffness in the neck was first felt, before any active spasm appeared. The severity of some of the paroxysms may possibly be attributable to the strychnine. Altogether the case may perhaps be regarded as a favourable one from the beginning, though experience teaches us that even the mild and protracted cases are too frequently fatal. It must be remembered, also, that the past year has been remarkable for the number of recorded cases of recovery from traumatic tetanus. Within ten weeks previous to the occurrence of the attack now recorded, no less than three cases of recovery were published in the 'Medical Times and Gazette;' one under the care of Mr. Cock and Dr. Wilks at Guy's Hospital, in which the cannabis indica was given; one at St. Thomas's under Mr. Simon, in which nicotine was largely used; and one at the London, which Mr. Curling treated with ether inhalation, and opium.

Admitting that the greatest care must in every instance be taken to guard against that common and natural error of attributing to treatment the credit of a favourable termination which nature alone might have achieved, and that there are few modes of practice which have not, at one time or another, been followed by recovery, there appear to me sufficient grounds for believing that in the present instance aconite did exercise a control over the disease. Before considering this question, however, it may be well to advert to the effect of the strychnine. In the American journals there are records of several cases of traumatic tetanus which had been apparently cured by this powerful medicine, and I had determined on the first opportunity to test its efficacy. The dose in which the strychnine was given was sufficient to induce speedily the muscular contractions characteristic of the poison. The spasmodic twitchings of the hands and arms, which are rarely present save in the severest forms of tetanus were frequent, especially on the patient's awakening from the short dozes into which he fell from time to time. Possibly, too, the severer and nearly fatal paroxysms which occurred at this period might be partly due to the strychnine. But, so far as could be observed, the effects upon the disease, which are stated in the American cases to have taken place upon the establishment of the symptoms of strychnine poisoning, were not present. There certainly was no remission of the tetanic symptoms after those produced by the medicine had passed off. On the contrary, the disease appeared to be increasing in violence, and I therefore gave up this mode of treatment and resumed the use of the aconite. My colleague, Dr. Goodfellow, called my attention to a case which he had seen, and which is related in the 'Lancet' for 1846, where the effects of aconite were very marked. The patient was under the care of Mr. Page in the Carlisle Infirmary. The disease was caused by the irritation of a gun-shot wound of the fore-arm, and the symptoms, which increased rapidly after their first appearance, were severe. On the third day after the stiffness in the jaw had been noticed, Mr. Page began the use of Fleming's tincture of aconite, and it was continued for thirty days in greater or less quantity according to the recurrence of the symptoms, which for the last fortnight were of a very mild character, and were only severe for the first six days after the medicine was given. But it is remarkable that on all occasions the symptoms were subdued after the aconite had been fairly given. For example, on the day on which the tincture was first given "the tetanic spasms were constant and severe; the muscles of the abdomen were rigid and unyielding, and those of the inferior extremity were so stiff as to render it very difficult to flex the limbs. At noon, a turpentine injection having been first administered, three minims of the tincture of aconite (Fleming's) were given, the effect of which became very speedily manifest, and in half an hour there was an almost total remission of the muscular spasm. Three P.M.—The pain and spasms having again returned with increased intensity, producing a slight degree of opisthotonos, four minims of the tincture were administered, which produced the same speedy and marked effects as the former dose. Six P.M.—The patient has been comparatively easy since the last dose, but there is now an evident disposition to relapse. Four minims to be given immediately, and to be repeated every hour until some decided effect is produced, the patient, of course, being carefully watched, in order that the remedial measures may be adopted should symptoms of poisoning become apparent. Ten P.M.—Three doses have been given—in all nineteen minims between twelve and eight o'clock; at present there is a complete cessation of the pain as spasms, which, however, did not yield until after the third dose had been taken. The system is now evidently under the influence of the aconite." And so during the continuance of the disease, though at times he was alarmingly affected by the medicine, the severe tetanic symptoms were constantly subdued.

In the present case, as in that related by Mr. Page, the questions may fairly be raised, whether the disease really yielded to the aconite, or whether a recovery would not have taken place under any other recognised plan, or even without treatment at all? Of course no answer can be given to the latter suggestions; but there are, I think, strong grounds for believing that the aconite was in this case the efficient agent. Dr. Fleming has described accurately and minutely the physiological effects of this poison on the system, and his statements have been corroborated by all subsequent observers. I frequently use the ordinary tincture in doses of from four to eight minims three times a day, and with this quantity the peculiar effects of the aconite usually manifest themselves speedily. Dr. Fleming says, that after a five minim dose* has been given there is a feeling of warmth, first in the stomach, then over the body, with numbness, tingling, and a sense of distention of the lips and tongue. There is a sense of tingling also at the tips of the fingers; slight muscular weakness, with indisposition for exertion, mental or bodily; weakness of pulse; and diminished frequency of pulse and respiration are also observed. If a second dose of five minims be given two hours after, the tingling extends along the arms, and the sensibility of the surface is impaired; the pulse becomes still lower and less frequent. There is great muscular debility, with giddiness and confusion of sight, and the person sinks into a lethargic condition, evinces great disinclination to be disturbed, although he rarely falls asleep, and complains much of chilliness, particularly in the extremities, which are cold to the touch. On the administration of five minims more, two hours after the last dose, these symptoms are greatly increased—lancinating pains in the joints are felt occasionally, headache, vertigo, and dimness of vision more marked. The pulse sometimes falls very low, but more frequently rises to seventy or eighty, and becomes small, weak, and probably irregular. The surface is moist and still more reduced in temperature. Sickness may now come on. Beyond this the experiment is attended with great risk. Many other symptoms are at times produced; and Dr. Christison has mentioned griping and diarrhœa as occasionally present. In some cases, too, Dr. Fleming has noticed that great insensibility to the influence of the remedy is manifested.

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* Dr. Fleming's tincture is more than twice as strong as that of the London Pharmacopœia. (See Fleming on Aconite. London, 1843.)

Now there is one fact which must always be borne in mind in administering narcotic and antispasmodic, as well as some other remedies. So long as their effect on the system is the arrest of some morbid action, so long, as a general rule, will their ordinary physiological influence on the system be suspended. I know no better illustration of this than in the action of opium on phagedæna. I have for years past relied exclusively on this remedy; and the only guide which I have followed in respect to the amount of the opium to be given has been the effect upon the disease, its arrest, and the return of healthy action. Thus, from a scruple to forty grains of opium have not unfrequently been daily given to patients unaccustomed to its use, without the manifestation of any one symptom characteristic of the influence of the medicine upon the system. There has been neither drowsiness nor torpidity of the bowels, nor loss of appetite, nor contraction of pupil; in short, excepting that the local disease has been subdued and healthy action restored, there has been nothing to indicate that the patients were taking what under ordinary circumstances would have proved dangerous quantities of the drug. Sir Henry Holland has remarked, in relation to the effect of opium when given for the relief of acute pain or spasmodic action, that "It would seem, however vague the expression, that the medicine, expending all its specific power in quieting those disorders of the nervous system, loses at the time every other influence on the body. Even the sleep peculiar to opium appears in such instances to be wanting." That this is the case in tetanus itself, as in other painful nervous affections, is too well known to require comment; and such instances are familiar to all practitioners. And so with other medicines perhaps, as in the case of calomel in pericarditis, in which disease, as Dr. Latham and Dr. Seymour have pointed out, enormous amounts of the drug will be borne without the system becoming affected; or in that of iron in erysipelas, where the remedy exerts its almost specific influence only when given in quantities far greater than are necessary to produce an ordinary alterative or tonic effect.

On the conviction, then, that our limit to the use of medicines in some forms of disease is not to be fixed by the boundaries within which they must for safety be restricted in healthy states of the body, the aconite was in this case given in larger quantity daily than has perhaps ever before been tolerated. One drachm of Fleming's tincture, equal in strength to more than double the quantity of the best pharmacopœal tincture, was taken continuously for three days; and we find that in the course of ten days, three drachms of the pharmacopœal tincture and five drachms of Fleming's tincture were taken without the manifestation of any effect on the system, except the rapid lowering of the pulse from 135, and its steady maintenance at 60 in the minute, the patient's natural pulse being from 75 to 80. But from the time when the pulse began to fall, the boy had no convulsion, and there was a progressive diminution of the chronic rigidity. In Mr. Page's case, although the effects of the aconite upon the system were after a time very marked, yet it was seen that very large quantities were tolerated before they were induced. Thus, "on the 15th December, this patient took in eight hours nineteen minims of Fleming's tincture; on the 16th, thirty-two minims in fourteen hours; on the 17th, twenty-five minims in seven hours; and on the 20th, twenty minims in two hours.

It seems to me that in these cases we must adopt one of two conclusions—either that the patients, as is sometimes the case even in man, were not susceptible of the influence of aconite, or that the energy of the poison was held in check by the disease, which physiologically implies generally that the disease was controlled by the medicine. It is quite contrary to all that is known of the action of aconite, to suppose that the system had in so short a time become tolerant of the poison, nor can the notion be entertained that the medicine was not absorbed. The potency of the tincture employed in the present case was unquestionable; the tip of the finger moistened with it and applied to the tongue, produced at once tingling and numbness, with slight sensation of giddiness.

To determine whether in the present instance there existed any idiosyncrasy which gave the lad immunity from the effects of this agent, some trials were made on him about three weeks after all the symptoms of the disease had disappeared. On the 28th of October, five minims of Fleming's tincture were given and repeated in two hours. No effect followed the first dose. Shortly after the second dose, he complained of headache and pain in the bowels. He went to bed and said he felt chilly, but the skin was found to be hot and moist. The pulse was quickened. Next morning, he had a little diarrhœa, and all the disorder disappeared. No satisfactory conclusion was arrived at, for all these symptoms might have arisen from an accidental attack of indigestion. On the 30th, the tincture was again given in the same way. No effect was produced, and he was not conscious that he had taken anything. On November 1st, a trial was again made, with directions that if no effect followed the second dose, a third should be given two hours after it. After the third dose, the symptoms of aconite poisoning were very marked. The forehead was cold and clammy; he complained of great chilliness, though the body under the bedclothes was warm and perspiring profusely; the pulse was quickened and irregular. The headache and pains in the bowels were again present, and he complained of tingling sensations in the arms and hand, and in other parts of the body. On the following morning he was as well as usual. It was clear, then, that larger quantities than ordinary were required to produce the usual effects; but those effects were produced actively enough when a certain quantity was taken. While the tetanus was present, he took the same doses every two hours for days together, and no one can doubt that a far less amount must have proved fatal had the disease not been present. Still, the slowness with which the aconite acted will explain why so much more was borne in this case than in that recorded by Mr. Page.

The question naturally arises, why should stress be laid on the fact of such large quantities of aconite being tolerated, when, as is so well known, opium and other narcotics are taken in enormous quantities without effect in this disease? But have we not really been seeking from opium results which it is not calculated to effect? All the foregoing remarks apply undoubtedly to opium and other narcotics. In most cases they will be borne to an extraordinary extent before their effects are manifested. But experience has proved what our knowledge of the action of opium would lead us to expect, that but little effect is produced by it upon the essential character of the disease. If the opium relieve pain and induce sleep, it tends to prolong life by taking away some secondary causes of exhaustion, and therefore in chronic cases has no doubt often saved the patient by helping to sustain the system until the disease has worn itself out. In no instance, however, that I am aware of, is there any evidence that it has directly arrested the muscular spasm. The action of opium and that of aconite are quite distinct from one another. Opium is primarily a stimulant, and although if applied directly to muscular tissue it paralyses it, yet its effect on the muscular system, when acting through general absorption, is principally the result of coma, the exercise of the will over the muscles being more or less removed. Not unfrequently, and especially from morphia, convulsions are present similar to those of tetanus. Its influence is exerted on the brain especially, little, if at all, on the spinal cord. But the primary and peculiar effect of aconite, conium, &c., is muscular paralysis, the brain being only secondarily affected, the influence of the poison being exerted mainly on the cord.

As, then, all the phenomena of tetanus indicate an affection of the cord, an irritation giving rise to muscular spasm, the pain being merely a consequence of it, we should naturally look for relief to those agents which diminish the irritability of that part of the nervous centres which controls the reflex muscular actions, not to those which act on the brain or diminish sensibility. In Mr. Page's case, we have distinct evidence that the muscular spasm was controlled by the aconite, and inferentially I should assume that the same must have been the case in the present instance.

In the following case, my colleague, Dr. Stewart, employed conium, which the investigations of Dr. Christison have shown to act, like aconite, primarily on the spinal cord. The form used was the extract prepared by Taylor. Many of the extracts in common use are, as is well known, nearly inert.

A man was admitted into the hospital on the 7th August, 1858, with severe and frequent paroxysms of tetanus, and with permanent locked jaw. The symptoms had set in eight days before. On the 10th, he began to take Taylor's extract of conium, and continued its use in five grain doses every two hours, and afterwards every hour, until the 26th. During this period, he took no less than two ounces and a half of the strongest form of extract, without any indication whatever of the physiological effect of conium, but with simply a gradual diminution of the tetanic symptoms. This of course was not an instance of the poison remaining unabsorbed in the stomach, as has been seen at times with opium. I should consider that here the agency of the conium was directed towards the counteraction of the morbid condition, and that hence its normal effects were not manifested.

The object of the foregoing remarks is to induce those who have the opportunity to give a full and complete trial to aconite and other medicines which have a like effect on the nervous system. In looking over the records of cases in which such agents have been given, it appears that generally the ordinary doses have been administered at long intervals. It has been by some assumed that because no benefit has been derived from them when so employed, that therefore no good was to be expected from them at all. There is a feeling, too, very prevalent, that though cases of tetanus sometimes recover, yet that the remedies employed have little to do with such a result. Nor is this surprising, considering the disappointment which has so often followed the more extended use of medicines which at first seemed to promise favourably. The same disappointment will perhaps follow the future employment of aconite. But in a disease so terrible and hopeless as tetanus, any encouragement, however slight, will be acceptable.

There is one other point in the treatment of tetanus to which allusion may be made. It has been generally taught that free action of the bowels should be kept up by means of powerful purgatives. From cases recently reported, indeed, this course does not appear to be now so generally the rule as formerly. My own belief is that excepting as a preliminary step to remove any matters which may be lodging in the bowels at the time of the attack, no benefit attends the practice, and that often harm is done by keeping up irritation in the alimentary canal, when our object should be to ensure as quiescent a state of the whole system as possible. In a case under my care last year, in which the symptoms were very mild, although it terminated fatally after nearly a month's duration, the bowels were allowed to remain quiet for the first nine days, purgative medicine was then given, and a free action of the bowels obtained, but the general symptoms were aggravated, and this was the case whenever the purgatives were used. Great relief is found from the use of enemata, however, especially those with turpentine. In the present case, the bowels were emptied by this means two or three times only during sixteen days.

Art. III.

On Syphilitic Inoculation. By Henry Lee, Surgeon to King's College Hospital and to the Lock Hospital.

On the 10th of October, 1858, I had the honour of reading before the London Medical Society a paper on the mode of action of morbid poisons, and of the syphilitic poison in particular. I then attempted to demonstrate that the absorbents were not the means by which poisons were ordinarily received into the blood, and that in those cases where the evidence was most conclusive of great excitement in the absorbent system, there was comparatively little danger of the poison being received, as such, into the general system. I endeavoured to show that the absorbent glands were, in fact, placed as sentinels in different parts of the lymphatic vessels, and that they had the power of refusing admittance to certain injurious agents; or of retaining them until those agents had undergone a change, such as would render them comparatively innocent when admitted into the general circulation. These principles were illustrated particularly with reference to the morbid actions induced by the inoculation of the syphilitic virus. The primary forms of syphilitic disease I then ventured to divide into four principal varieties—namely: 1st, those in which the application of the poison, or of some irritant applied either with the poison, or shortly afterwards, produced mortification of the infected part; 2ndly, those in which the contact of syphilitic matter produced true ulceration—that is, an action in which the absorbents played a part and took up portions of the infected tissue impregnated with syphilitic matter; 3rdly, those in which a free suppuration was produced, and in which the secretion from the sores consisted principally throughout their course of well-formed pus globules; and 4thly, those in which the morbid action induced consisted of a specific adhesive inflammation. From tables which I read before the Society, it appeared that the first form of disease was not necessarily accompanied by either swelling or inflammation of the absorbents; nor was it followed by any constitutional symptoms: that the second was as a rule accompanied by violent inflammation of the absorbent glands, but was not followed by secondary symptoms; that the third was not ordinarily accompanied by swelling or inflammation of the glands, and was not followed by secondary symptoms: that the fourth was accompanied by an indolent swelling (but not inflammation) of the glands, and was, as a rule, followed by some form of constitutional disease, unless this was prevented by mercurial treatment.

Soon after the period to which I have referred, I had reason to believe that these different forms of disease were not all equally communicable in the same way. It appeared to me that the adhesive form of inflammation was not capable of being communicated by inoculation as readily as the suppurative form. More than one occasion offered itself for publicly discussing this point, and I was told in friendly criticism that I had not selected my cases for inoculation rightly—that, in fact, I had inoculated some sores during their period of repair, and some previously to that period, and that this had given rise to the differences I had observed. As the time advanced, fresh opportunities presented themselves of testing the truth of the doctrine, and in 1856 I published in the 'British Foreign Medical and Chirurgical Review' several cases where inoculation with the lancet in the ordinary way had failed, and yet when every doubt seemed to be removed as to the cases being genuine primary syphilitic sores in a period of progress. The importance of such observations could not be doubted if the experiments had been rightly performed; for in that case, what became of the inoculation test as the sole means of distinguishing primary syphilitic affections from other diseases? This test had been loudly proclaimed from one end of Europe to the other as the only one that could be relied upon as a certain indication of a primary syphilitic sore; and any opposition to this universally-received doctrine at the time I speak of, was regarded as originating in some mistake. Nevertheless, those who were in the habit of seeing inoculations most frequently performed had their secret misgivings upon the subject. It was, for instance, in the year 1851 that Professor Boeck, travelling through the Northern part of Italy, became attracted by the new doctrine of syphilization. On his return to Christiania, he resolved to try the new plan as soon as he was, to use his own words, "able to obtain some inoculable virus." This was not until the month of October, 1852! Are we to suppose that during the first months of the year 1852 there was no such thing as a primary chancre in Christiania; and that the disease during that time was not communicated there by natural intercourse? Or may we not rather see the lurking impression on the Professor's mind, that one form of syphilitic disease was much more easily inoculated than another? In a town where syphilitic disease was known to be as rife as in Christiania, we must take for granted that the Professor had under his care many cases that he well knew were genuine cases of primary syphilitic disease; but from what he had witnessed, he instinctively believed that the secretion of one particular kind of sore would serve his purpose best, and for this he waited.

Although those who practised inoculation were everywhere asking for good pus for their experiments, the universal belief nevertheless obtained that all chancres during the period of progress were alike inoculable. Very few at that period seem to have thought of asking themselves the question, how are the sores which do not furnish good pus communicated? or, whether there was any difference in their mode of being propagated? The dictum of M. Ricord was very universally received, and the characteristic pustule produced by inoculation was as universally acknowledged as the test of a true primary syphilitic sore. M. Ricord had said that the inoculation, when properly performed, never failed, and that the results were regular, characteristic, and uniform.* As usually happens after a theory has been too hastily received, a period of undue reaction now threatens to set in From the hasty generalization, that all primary syphilitic sores are equally inoculable by the point of the lancet, we now hear that the indurated variety of chancre is not inoculable at all, upon the patient who has it. In the 'Gazette Médicale de Lyon' for the 16th of January last, M. Rollet asserts, that as secondary syphilis is not inoculable on the patient, so neither is the primary infecting sore:—

"La syphilis secondaire n'est pas inoculable au malade; mais le chancre infectant ne l'est pas davantage." (p. 36).

"Inoculez la syphilis à la lancette . . . . même l'accident dit primitif, le chancre infectant, et vous n'obtiendrez pas davantage la pustule d'inoculation."

"Il n'y a qu'une maladie vénérienne qui soit inoculable par piqûre au malade lui-même, et cette maladie, c'est le chancre simple, et son dérivé le bubon chancreux."

Here we have one kind of chancre alone acknowledged, as capable of being communicated, by inoculation upon the patient himself, with the point of the lancet; a complete revolution in doctrine since the year 1856.

In the last work published under the direction of M. Ricord,† that distinguished Professor still clings to the belief, that sores affected with the specific adhesive inflammation may be inoculated in the same manner, and that the inoculation will produce the same result as in sores affected with the specific adhesive inflammation. He says:—"Ce que je viens de dire de l'inoculation et des développements du chancre, se rapporte presque aussi bien à l'une qu'à l'autre de ces variétés." Farther on in the work, however, M. Ricord's faithful expositor and distinguished pupil, M. Alfred Fournier, gives a table of 99 cases in which the secretion from an indurated chancre had been inoculated. The result was negative in 98 instances, and positive in one. In this case, however, the *specific pustule* was produced. As no explanation is given of this apparently strange exception, it is not impossible that this may have been an instance of a suppurating sore having become originally inoculated upon a sore affected with specific adhesive inflammation; or of the converse, of the base of the sore having taken the specific adhesive action, while the original inoculable pus was still being secreted from some part of its surface. This explanation has suggested itself to me as probable, from the somewhat analogous results obtained by clinical observation. During parts of the years 1855–56, I examined 100 cases of sores, in which the secretion appeared to be well formed pus. These were considered to be local suppurating sores, and were treated accordingly without mercury. Two only of the whole number returned to me with secondary symptoms, as the result of the primary suppurating sores. Now, in both these cases, upon making a further minute inquiry, it appeared that the patients had subjected themselves to more than one source of contagion shortly before the appearance of the primary disease. It is therefore quite possible that one form of chancre may have been inoculated upon another, and so have masked the diagnosis; and in M. Fournier's case I am inclined to believe that this is what actually did occur, because he does not note that the characteristic pustule which was produced differed from the ordinary pustule resulting from the inoculation of a suppurating sore.

Are we, then, to believe with M. Rollet, that the secretion of the indurated syphilitic sore cannot be inoculated upon the patient affected? Are we to make a complete revolution in our opinions, and from believing, as formerly, that all primary chancres were equally inoculable by the point of the lancet, now hold that the indurated variety cannot be inoculated at all upon a patient who has once been affected?

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* Traité Pratique, pp. 94, 185.

† Leçons sur le Chancre, p. 81. 1858.

As in 1856 I hesitated not to express my belief that all primary syphilitic chancres were not alike inoculable, and that inoculation was therefore no proper test of an ulcer being syphilitic; so now I venture to affirm that it would be error to suppose that indurated infecting sores—those affected with specific adhesive inflammation—are not inoculable at all upon the patients themselves. This I would do upon three distinct grounds:

1st. From the result of direct experiment in inoculating the secretion in the ordinary way.

2ndly. From clinical observation.

3rdly. From the result obtained by inoculation, when the sores from which the secretion is taken have been subjected to certain kinds of irritation.

1. In the beginning of the year 1856 a medical student became diseased for the first time. He inoculated himself on the thigh, and presented himself to me three or four days afterwards. The inoculation succeeded, and became a small, hard, button-shaped induration, exactly resembling the original: a small point of whitish lymph was at first visible in the inoculation, but both sores subsequently remained as small, hard, circular indurations, furnishing scarcely any secretion from their surfaces. In this case, the student had, it is believed, applied some caustic to the sore before he inoculated himself. Here, then, we have an unequivocal case of an indurated sore (subject, probably, to previous artificial irritation) producing, when inoculated with the point of the lancet, not the characteristic pustule, but an indurated sore like itself.

2. The same point may be demonstrated by much more numerous instances, although not so conclusively in any one case, by clinical observation. There is a man now in the Lock Hospital with an indurated cicatrix upon the upper part of the thigh exactly corresponding to a similar induration upon the extremity of the prepuce. There was in this case other causes of local irritation; but no one upon seeing the case would doubt that one induration had been communicated by inoculation from the other. Cases are not very uncommon in which opposed surfaces, of the labia, for instance, present well marked indurated sores of exactly the same size, shape, and appearance. There can here, again, be no doubt that this affection is communicated from one situation to the other by inoculation. But in all the cases which I distinctly remember there has been some cause of irritation superadded to that of the specific adhesive inflammation.

3. The most interesting proof of the inoculability of the secretion of an indurated sore is afforded by the conversion of the adhesive into the suppurative inflammation by the artificial application of irritants to the part. The experiments illustrating this subject have very recently been performed.

Having observed that the indurated sore was inoculable under certain states of irritation, a blister was applied to some indurated sores, and subsequently they were dressed with the ung. sabinæ. By these means I obtained a free secretion of pus from these sores, and it was found that the secretion, before incapable of producing any effect upon the patients themselves, could now be inoculated. The results of these inoculations have been quite different in their course to those which follow ordinary inoculation from suppurating sores. Pustules have sometimes been produced, but these have shortly dried up, and the inflammation consequent upon the punctures has soon declared itself to be of the adhesive character.

Case 1. Bridget C——, aged seventeen years, was sent to me by Mr. Hewett, of St. George's Hospital, in August last. She had suffered from a thick yellow discharge between two and three months, and two months previously two small places formed upon the upper part of the left thigh. On the 26th of August she was admitted into the Lock Hospital. The places on the thigh then presented all the appearances of well-marked indurated primary chancres. They were oval in form, with their edges slightly raised, and these, together with the bases, presented the characteristic abruptly-terminating induration. The surfaces of these sores were covered by a scanty tenaceous secretion in small quantity. Upon making a microscopical examination of the discharge from the sores, no pus globules could be detected. The secretion from these sores was carefully inoculated with the point of the lancet upon the patient's thigh.

Aug. 28th.—The inoculation was repeated. There was at this time no indication of the sores having a tendency to heal.

Aug. 31st.—No result from the inoculations.

The two sores had now been dressed for two days with the unguentum sabinæ, and yielded an abundant secretion, distinctly purulent. The secretion from each sore was inoculated in several points in two distinct places on the thigh.

Sept. 2nd.—Inoculations have produced an appearance of small incipient pustules in both places. The secretion from one of these little pustules was inoculated on the thigh lower down.

Sept. 4th.—The inoculations from the inoculations had succeeded.

One of the inoculations of the 31st of August had produced a small pustule. The others had produced only vesicles. The skin over one of these was broken.

Sept. 9th.—Inoculations from inoculation, performed on the 2nd of September, have dried up.

The inoculations first in order, of the 31st of August, have entirely lost their puriform character. They now appear as circular patches, yielding a serous secretion containing epithelial scales. The original chancres were now in process of healing. A drawing was taken by Dr. Westmacott, and shows the original sores in process of repair, and the inoculations as they appeared upon the tenth day.

Sept. 11th.—The inoculations first in order are desquamating and of a light-red colour.

The inoculations from the inoculations appear as small red pimples, which are gradually fading.

Sept. 14th.—The inoculations are losing their colour, but still present distinct oval patches of red skin, from which epithelial scales are thrown off.

Sept. 17th.—Original chancres were skinned over; the inoculations were fading and desquamating.

Sept. 23rd.—Inoculations from inoculations were still visible, and appeared as shining scales of discoloured epithelium.

Sept. 25th.—A few faint secondary spots appeared on the body. The original sores were quite healed, leaving slight induration. The corresponding glands in the groin were still indurated.

A second drawing was made by Dr. Westmacott, and shows the appearance of the inoculations on the 26th day.

Oct. 4th.—A third drawing was made by Dr. Westmacott, showing the remains of the original sores, of the inoculations on the 35th day, and of the inoculations from the inoculations.

The patient now left the hospital, but again presented herself on the 8th of October. The cicatrices of the original sores were still red and rather tender. The inoculations appeared as brown spots, the colour of which gradually faded into that of the surrounding skin. Some small brown spots marked the situation of the inoculations second in order.

In performing these experiments, it is necessary to select sores which are not liable to be inoculated from a different kind of secretion; otherwise we may in fact be inoculating the product of a suppurating sore, when we suppose that we have only an indurated sore to deal with. This is probably what happened in the following case.

Joseph B——, aged twenty-seven, was admitted into the Lock Hospital on the 25th of November last. He then had a simple chancre on the upper part of the prepuce of five weeks' duration. Below this was a well-marked specific induration, which he said had never been ulcerated. This had existed four weeks. Near the frænum was a third sore, which had existed for three weeks, and was surrounded by some amount of induration.

Dec. 7th.—The secretion from the induration, which had been made to suppurate artificially, was inoculated upon the patient's thigh.

Dec. 12th.—A large prominent pustule with a depressed centre has formed as the result of the inoculation. The pustule was destroyed by the application of a drop of strong nitric acid, and the sore which was left then readily healed.

In this case, although it was intended to have obtained the secretion from the induration only for the purpose of experiment, yet the result being so different from that which occurred in other cases, made it probable that the secretion from the upper sore has run down and imparted its specific qualities to the abraded induration; a *superfœtation* may thus occur, which requires to be guarded against in all similar experiments. The products of a suppurating sore may thus be inoculated by accident from the surface of a specific induration, and there is good reason to believe that the converse of this may take place—viz. the specific adhesive inflammation may be communicated under the mask of the secretion from a suppurating sore.*

The cases and observations which have now been laid before the reader will be sufficient to establish the fact, that although the secretion from indurated sores is not inoculable (upon the patients having those sores) with the point of the lancet under ordinary circumstances, yet that it can be so inoculated, either in the early stage of the disease, or in a subsequent stage, after the sores have been subjected to artificial irritation. The result of the inoculation in these two cases is not the same, and they both differ widely again, from the results obtained by the inoculation of a naturally-suppurating sore. These considerations support powerfully the idea that the indurated variety of chancre differs essentially in its physical characters from other kinds of syphilitic sores, and we cannot but conclude that those who described (as was so common in the text-books upon syphilis some three or four years ago) the gradual conversion of the sore produced by the ordinary pustular variety of inoculation, into the indurated chancre, as of ordinary occurrence, had never in reality witnessed what they professed to teach.

In conclusion, that which has been advanced in the present communication may be summed up in the two following propositions:—

1. That a primary syphilitic sore which upon inoculation produces the characteristic pustule, is not one which, if left to itself, would, as a rule, be followed by secondary symptoms, and therefore is not one which requires mercurial treatment.
2. A primary syphilitic sore affected with specific adhesive inflammation is not, under ordinary circumstances, inoculable by the lancet upon the patient himself; but that in its early stage, or after it has been subjected to certain kinds of irritation, it may be so inoculated; and that then it will give rise to some adhesive form of inflammation.

PART FOURTH.

Chronicle of Medical Science.

HALF-YEARLY REPORT ON MICROLOGY.

By John W. Ogle, M.D., F.R.C.P.

Assistant Physician to St. George's Hospital, and Honorary Secretary to the Pathological Society.

Part I. Physiological Micrology.

EPITHELIAL SYSTEM AND HAIR.

On Uterine Epithelium.—Dr. C. Robin† has communicated a paper of which a part is devoted to the alterations undergone by the epithelium of the body of the uterus during gestation. He states that whilst the cylindrical epithelium of the neck of the uterus retains its form during pregnancy, that of the cavity of the body undergoes marked alteration. At a certain period after fecundation, it exfoliates cell by cell or by flakes, and is replaced by the polyhedral pavement form of epithelium, the cells being of from twelve to eighteen-thousandths of a millimetre in size, containing a spherical or ovoid granular nucleus of about the size of a red blood-globule, but, as a rule, no nucleolus. Many cells are void of nucleoli, and are uniformily filled with yellowish fatty granulations, and this condition of the cells is found on the decidua vera and reflexa. After two months and a half of pregnancy, other cells, much larger and longer, are seen in addition. These are delicately flattened, from four to nine-thousandths of a millimetre long, always irregular, and prolonged at the extremities or angles. They possess a large ovoid granular nucleus, certain slight scattered granulations, and increase in numbers towards the end of gestation.

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* This is probably what occurred to Hunter. He believed that he had inoculated himself with gonorrhœal matter only; but he produced "a thickened sore," which was followed by secondary symptoms.

† Brown-Séquard's Journal de la Physiologie, p. 60. Jan. 1858.

As pregnancy advances, the nucleus of the large cells, which about the second or third month of gestation is often wanting in a nucleolus, obtains one or two, which are yellow and shining in the centre, with a definite contour; and the nuclei have also become very large. In the region of the upper part of the neck of the uterus, where the decidua reflexa at the eighth month is not adherent, cellules like those above described are to be found, some being regularly polyhedral, others elongated, and all containing nuclei of an ovoid or spherical shape, which are from twelve to fifteen-thousandths of a millimetre long, and generally furnished with one or two bright nucleoli. Some of the cellules maintain their prismatic form, but the adherent extremity is generally rounded and swelled. At this period of pregnancy the epithelium of the Fallopian tube is chiefly composed of nuclear epithelium, formed of ovoid, sometimes spherical nuclei, and united by a small quantity of amorphous material.

The author then goes on to speak of alterations undergone by the epithelium enclosed between the placenta and the vascular part of the uterine mucous membrane, and that covering the uterine and reflected deciduæ, where brought into contact by enlargement of the ovum. In these places the epithelium is partly formed of free nuclei and in part of cellules. The latter are partly like those described, and some, although half or double as large as others, yet maintain the prismatic form, and are rounded at their extremity. Others, again, become polyhedral, and in almost all, the nucleus has increased proportionately with the cellule, containing one or two nucleoli having a brilliant centre and dark contour. But in addition to these cellules others are found, isolated or forming laminæ, which are of variable size up to about one-thousandth of a millimetre or more. For the most part they are elongated, and generally irregularly truncated, rarely being sharpened, and at times ovoid or fusiform. Sometimes one or two of the extremities are bifurcated, and this is especially so at the angles of the cells which remain more or less polyhedral. Some are to be seen with two or three nuclei, although they generally possess but a single one, which is very large, ovoid, and having a clear bright centre and regular clear outline. Each nucleus contains one or two nucleoli. The free nuclei of the epithelium are very like those ordinarily named cancerous or carcinomatous, and would be so designated by those not conversant with the subject. These large defined cells are mostly filled with granular fat, situated chiefly around the nuclei or at the ends of the cells, a few remaining finely granular, pale, and transparent. The author concludes by drawing a strong analogy as to structural form between the above described natural condition, and certain pathological states for which they might readily be mistaken.

On the Central Parts of the Hair in a Physiological and Pathological state.—Dr. A. Spiess* controverts accepted views as to the structure of the hair, and applies his own description thereof to the elucidation of diseases of this structure. He takes exception to the statements of Kölliker, that the central material of the hair, is composed of a row of cells containing a tenacious substance with air-holding cavities; of Steinlin,† who looks on it as a projection of the hair papillæ into the hair; of Reichert,‡ who also considers it as a projection of the papillæ, which at the lower part of the hair serves the purpose of a matrix for the hair to be developed around it, and afterwards degenerates into a species of pith, as in the case of feathers, and which also often contains air, although this also exists free between the cells. In opposition, Spiess states that the medulla of the hair is only a canal in the centre for the most part containing air, in which the remains of dried cells exist. He also opposes the statements of Engel as to the method by which hair after section again grows and recovers its point. In a thin hair a certain quantity of nourishing fluid is, according to him, secreted from the vessels of the hair papillæ. This nourishes the root, and passes upwards between and nourishes the cells throughout the hair. At the same time the cells give out the used material, to be again reabsorbed in the follicle below and above, to be cast off as a powder. This fluid, if the hair is not long, goes to form new cells in the root, and thus the hair grows, the cells dwindling and passing into the cortical part as they progress upwards from the root. In thick hair, however, as there exists such an impediment to the passage of the refuse material from the middle parts that they remain in the centre, only the outermost parts being nourished, the central cells shrink, become atrophied, and disappear in part, or remain as débris, whilst their place is occupied by the refuse material forming the medullary part, which consists of cell-remains, gases, and in part fluid. The thick hair consequently consists of only a cortical substance, composed of compressed fibre-cells covered by a pellicle, having a canal in the centre containing hair and fluid, but no medullary substance. The four pathological cases adduced in connexion with the above statements as to the elements of hair, illustrate morbid changes in various portions of the hair, and in hair of various thickness.

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* Henle und Pfeuffer's Zeitschrift, Band v. Heft 1. 1858.

† Ibid., 1850.

‡ Zeitschrift für Klinik Med., Band vi. Heft 1, p. 1. 1855.]

NERVOUS SYSTEM AND ORGANS OF SENSE.

On the Termination of Nerves. By Dr. W. Krause.*—The author made his observations chiefly upon the conjunctiva of the ball of the eye, as affording the best opportunities, owing to the delicacy of its epithelium, its freedom from pigment, hair, glands, papillæ, or strong elastic fibre, &c., and by reason of its abundant supply of nerves with dark contours. Having separated a piece of the conjunctiva from the subjacent tissue of the warm eye of a recently-killed calf, the nerve branches are seen forming a rich plexus by subdividing anastomoses, the single fibres of which gradually diminish. In the smallest branchlets, formed of two or three fibrils, numerous bifurcations of the fibrils may be seen, which finally either arch in order to end immediately, or preserve their course, anastomosing with other fibrils or their branches. Most of the fibrils bifurcate at various angles once more before terminating; more seldom they divide into three. Whenever their terminations can be seen, they will be found to end in a peculiar small, round, or oval corpuscle, with a thicker and a finer extremity, called by the author the terminal bulb. These latter bodies consist of an investment of areolar tissue containing nuclei, and a soft cylindrical mass of faintly glistening substance, in which the double-contoured nerves end, and in the centre of which is embedded an axis fibre—a stripe with a slightly thickened extremity—the investment being an immediate connexion with the neurilemma of the fibril, which before the termination of the latter is slightly elevated. The investment is beset with elongated and often spindle nuclei at the sides and end of the bulb, and in the larger terminal bulbs very fine blood capillaries may sometimes be seen in the investing membrane. The nerve-fibrils in the calf, before their entrance into the terminal bulb, possess a breadth of from $\frac{1}{400}$ to $\frac{1}{300}'''$ (Paris), and get smaller, until they are only from $\frac{1}{800}$ to $\frac{1}{300}'''$ in size. The axis fibre itself measures from $\frac{1}{800}'''$ to $\frac{1}{600}'''$ in breadth; the final swelling of the fibre being about $\frac{1}{400}'''$ broad. The dimensions of the entire final bulb are from $\frac{1}{33}'''$ to $\frac{1}{20}'''$ as to length, and from $\frac{1}{150}'''$ to $\frac{1}{77}'''$ in breadth. Apart from the size, these bulbs have a closer analogy to the Paccinian bodies, excepting that they are wanting in the concentric lamellæ which in the latter surrounds the central substance. The form, however, of these bulbs is not always regularly cylindrical, but at times arched and pear-shaped, indented, &c. They are situated partly in a horizontal direction, immediately beneath the firm areolar layer below the epithelium, and partly more or less in a perpendicular direction to it, with their thicker ends towards it. The author thought he had met with divisions of the axis-fibre and the whole bulb analogous to that of the Paccinian corpuscle. These terminal bulbs soon undergo decomposition, and twenty-four hours after death only a fragment of the axis-fibre remains visible, the whole having become clouded, and at times streaked and fatty. Acetic acid renders the nuclei more distinct, but obscures the axis-fibre and entire bulb. An alkali makes the bulb pale and swollen, the axis-fibre dwindling, and very dark fine fat granules appearing in the interior. The nerve-fibres are, however, in this way brought out in their course. In the case of preparations which have been some days in a solution of chromic acid, the addition of an alkali renders the whole bulb very clear, but for a short time only. These terminal bulbs have different measurements in different animals, as will be seen in the following statements:

In the deer the bulbs are about $\frac{1}{18}'''$ in length, and from $\frac{1}{33}$ to $\frac{1}{60}'''$ in breadth.
" sheep " $\frac{1}{18}'''$ to $\frac{1}{16}'''$ " $\frac{1}{34}'''$ to $\frac{1}{63}'''$ "
" pig " $\frac{1}{30}'''$ to $\frac{1}{16}$ " $\frac{1}{128}'''$ to $\frac{1}{72}'''$ "

Occasionally there are seen windings of the double-contoured nerve-fibres which enter them.

As to the number of these bodies existing, it was found that in the case of the calf, a space about $1\frac{1}{3}'''$ in length and $\frac{1}{3}'''$ in breadth gave as many as ten of them; and in the deer and sheep, in the same given space, a branchlet with three nerve fibrils was seen after numerous divisions to possess the number of twenty-three. In the conjunctiva of the common hen, small Paccinian bodies, as in the tongue of some water birds, were met with.

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* Henle und Pfeuffer's Zeitschrift, Band v. Heft 1, p. 98. 1858.

In man, nerve-knots (*knäuel*), consisting of numerous interlacements of one or more nerve-fibrils, are met with in the conjunctiva, measuring about $\frac{1}{45}'''$ in length, and $\frac{1}{55}'''$ in breadth. They are often spherical, and in some cases possess loops of fibrils on the surface. The terminal bulbs, in man, are rounder than in animals, often almost spherical. The investing membrane, with its nuclei, resembles those above described as to behaviour with reagents, &c.; but the entrance of nerves is, however, considerably modified, and very often the bulb is placed symmetrically upon the nerve-fibril as on a stalk. Or the nerve forms a hook or crook-like bend, and the bulb is placed upon it laterally. The fibrils also may be twisted singly, or numerous convolutions may exist within the investing membrane of the terminal bulb. Sometimes a fibril bifurcates, and each subdivision then enters the bulb, and ends either immediately or after making several convolutions. Under favourable circumstances, firm pale fibres about $\frac{1}{800}'''$ in diameter are seen to proceed out of the above-mentioned knot of nerves within the bulb, and to end inside the bulb after numerous twistings. In these cases there are more axis-fibres inside the bulb, reminding one of the Paccinian corpuscles observed by Kölliker* in the case of the cat. In man the bulbs measure from $\frac{1}{70}'''$ to $\frac{1}{30}'''$ in length, and from $\frac{1}{75}'''$ to $\frac{1}{50}'''$ in breadth; and the diameter of the nerve-fibril before entering is about $\frac{1}{400}'''$ to $\frac{1}{300}'''$, whilst after entrance it measures from $\frac{1}{800}'''$ to $\frac{1}{300}'''$.

Other parts besides the conjunctiva contain these terminal bulbs. Thus, in man they are found in the mucous folds beneath the tongue, the soft gums, the fungiform papillæ, and under the base of the filiform papillæ, and in the papillæ of the red edge of the lip, in the mucous membrane of the glans penis and clitoris. They are to be met with also in the tongue and lips of the deer, in the glans penis and clitoris of the pig, in the sublingual mucous membrane of the cat and rat, in the volar surface of the toes of the guinea-pig. In all these cases they are placed more or less inclined at an angle to the surface. In these localities the bulbs do not enter into the microscopical papillæ, excepting in the lip and perhaps the soft gum of man, where they are found at the termination of the papillæ. In the mucous folds at the bottom of the mouth of man they are very large, being $\frac{1}{19}'''$ long and $\frac{1}{22}$ broad. In the glans penis and clitoris of man the investing membrane of these bodies is very thick and firm, and very deeply situated beneath the papillæ. In the glans penis, &c., of the pig they are very abundant, and very similar to Paccinian bodies, having been described as such by Kölliker and Hylander, possessing a thick laminated investing membrane, but no special capsule. The author then gives the various measurements of these bodies as found in different parts of animals, and proceeds to give his views as to the function of these bulbs. He thinks they are evidently concerned in the conduction (*vermittlung*) of simple sensibility, the investing membrane only serving for protection, and the nuclei being probably connected with nourishment. The knot-like convolutions and bifurcations appear to have the purpose of intensifying sensation. He then passes in review the terminal corpuscles of nerves in various organs which may be looked on as transitional steps one from the other, beginning with the Paccinian corpuscle of the mammalia, advancing through those of the bird to the "terminal bulbs" of mammalia, and thence through the "terminal bulbs" of man to the sensitive corpuscles (*tast-körperchen*) of man and the monkey.

The terminal bulb of the mammalia seems to show the simplest form, consisting of a fine terminal axis-fibre surrounded by the soft conducting substance of the corpuscle, and invested by a nucleated membrane. Then we have in the Paccinian bodies of birds peculiar transversely-spiral fibres between the central fibre and outer investment. In the Paccinian corpuscle of mammalia appear numerous concentric lamellæ which are to be considered as a conducting apparatus; and the 'terminal bulbs' of man are to be looked upon as a further development of the simple structures existing in mammalia. The tactile corpuscles (*tast-körperchen*) consist of a delicate areolar investment containing nuclei placed transversely and lengthwise, and a quantity of soft granular contents. In the interior, the entering nerve-fibres divide into numerous tuft-like axis fibres, which contain a semi-fluid substance, and which are the sensitive part of the corpuscle. The author quotes his father, as well as H. Henle and Meissner, as supporting the above described observations.

OSSEOUS SYSTEM.

On the Development of Bone-substance in Plasmatic Fibroid Tissue.—The subject is treated by Dr. C. Ronget in a paper,* illustrated by plates, on the corpuscles of bone and on the development of secondary bones. After alluding to the controversy regarding the supposed method in which the bone-substance is formed where no cartilage previously existed, whether, for example, the calcareous matter be deposited in a continuous membranous blastema of which but certain parts become ossified, or in an alveolar cartilaginous framework—or whether, again, the cellules of the blastema be transformed into osseous corpuscles, or whether the latter be only cavities formed either at the expense of the interstices of the cartilaginous framework or directly by ossification—the author proceeds to detail the results of his own examination of the subject. He states that in the human embryo of ten or twelve weeks, if the fibrous membranes (the periosteum and dura mater) between which the bones of the cranium are developed, be carefully examined, a lamina partly osseous and partly membranous may be obtained, which is continuous throughout the whole extent about to be occupied by the perfectly formed bone. The membranous zone is in direct continuity with the zone in course of ossification, and, like it, is quite distinct from contiguous fibrous structures. In each lamina already four distinct regions may be recognised. Of these, two occupy the extreme limits, the one completely membranous and homogeneous in structure, the other completely osseous. Of the two middle ones, the one contiguous to the osseous region is itself in process of ossification, whilst the other, which is contiguous to the membranous region, presents certain peculiarities, and this zone the author designates the intermediate zone or region. The membranous zone is formed of a fibroid and granular connective tissue, traversed here and there by decussating fibrous bundles, and characterized by innumerable rounded or ovoid plasmatic cellules and free nuclei. In the intermediate zone the same elements exist, but in addition a network of transparent and homogeneous hyaline trabeculæ, which appears to repeat the disposition observed by the inter-crossing bundles of the membranous zone, with which it is manifestly continuous, but by a gradual fusion of the characteristics of the two elements.

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* Zeitschrift für wissensch. Zoologie, Band v. p. 119. 1854.

Towards the zone in course of ossification the trabeculæ multiply, become thicker, and circumscribe large and small spaces. At the limit of the two regions the trabeculæ of the hyaline framework are continuous with trabeculæ quite analogous as to general disposition, but differing in appearance, being less transparent, and occupied by yellowish granulations and innumerable microscopic cavities; and a decided line of demarcation exists between the extremities of the trabeculæ in course of ossification and the hyaline trabeculæ, although both cavities belong to one and the same system. The web-work which is undergoing ossification intercepts two kinds of meshes distinguishable by their peculiar dimensions. Of these, the one forms veritable areolæ, the other small microscopic cavities, both possessing trabecular walls either hyaline or in course of ossification, and containing connective blastema and cellules. On approaching the perfectly ossified zone and the more obscure and yellowish trabeculæ, the minute cavities in the thickness of the trabeculæ become more and more small, and a hyaline network, which speedily becomes ossified, extends in the areolæ, which it closes or obliterates. Where, also, the hyaline framework becomes involved, groups of cellules, more or less numerous, and one or two isolated cellules, insinuate themselves in the inter-cellular connective substance. Thus, when the primitive membranous layer has undergone perfect transformation, we have a transparent homogeneous blastema gradually poured out in the granular and fibrous connective substance, and in this blastema subsequently are deposited the elements of ossification, whilst during all these changes the plasmatic cellules of the connective tissue remain intact within the small irregular cavities of the new osseous substance. In this way, in the place of a membranous blastema, whose elements appear only at the period of ossification, it must be admitted that there is a pre-existence, at the spot where the osseous substance is primarily developed, of a fibrous or fibroid membrane, which at first occupies all the space possessed at a later period by osseous formation. True it is that an areolar hyaline framework gradually precedes the calcareous deposit, but there is no reason for looking upon this framework as being cartilage, for it is wanting in cartilage cellule, and the persistence of the prismatic cellules in the midst of this framework shows it to be a simple modification, a return to the primitive form of the connective inter-cellular substance. Throughout the paper reference is made to the late theories of Kölliker and Robin, and most authors who have written upon the formation of bone, as well as to the views of Reichert, Virchow, Donders, &c., as to the original identity or homology of the various plasmatic or connective tissues of cartilage, bone, fibre, medullary tissue, &c.

THE BLOOD.

On the Modifications undergone by Blood-Globules of certain Animals injected into the Circula-

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* Brown-Séquard's Journal de la Physiologie, p. 764. Oct. 1858.

tion of other Animals, &c. By Brown-Séquard.*—This physiologist has observed, on examining by the microscope the blood of the dog, cat, or rabbit, even a single hour after injection therein of the blood of the bird, that none of the oval large globules are to be met with; and if examined some days, or even several hours, after, none whatever are to be seen in any organ of the body where it might be supposed that they had been arrested. The blood, however, of such an animal, if examined a quarter of an hour after the injection of the bird's blood, will be found to contain oval globules in the veins of all the organs. Thus it appears that all the capillaries of the body are permeable by and do not arrest these large oval globules. But, even for the space of a month after injecting the blood of the dog, rabbit, guinea-pig, &c., into the circulation of the bird, some of the circular discs are to be seen within it when examined by the microscope. In the case of the cock, into whose veins dog's blood has been transfused, on the following day as many round as oval globules are to be seen; but in a few days the circular globules have become much fewer, and three weeks after only two or three are to be seen in proportion to a thousand of the oval ones.

On certain Peculiarities in Form and Size exhibited by the Red Globules of Blood in Embryos. By Dr. C. Robin.†—According to this author, the red globules in the embryo of man, from the time of their appearance to the period when the body has attained the size of twenty-five millimetres or more, possess a diameter for the most part of eleven thousandths of a millimetre, their thickness being from three to four thousandths, unless when rounded by contact with water. In the embryo of three millimetres in size, a few are only eight thousandths of a millimetre, the most being from eleven to thirteen thousandths. In an embryo of twenty-five millimetres long, some of the globules were from seven to eight thousandths, and some from fourteen to sixteen thousandths long and three thousandths in thickness. In an embryo of nineteen millimetres long, in addition to red globules of the above size, there were a number sixteen or seventeen thousandths in size, with a thickness of five thousandths. In embryos from three to twenty-five or thirty millimetres long, most of the globules are discoid and biconcave, and often thicker at one point than another; but some are spherical, and some are ovoid but not flattened, measuring seven, eight, or ten thousandths of a millimetre long by from twelve to fifteen thousandths. Some have a wallet shape, with a contraction towards the centre, and at this age the red globules are remarkable for the facility with which they may be put out of shape by reciprocal pressure, or when compressed in small capillaries, &c.; but they are very elastic, speedily assuming their natural shape. The above particulars hold good in all domestic mammalia and rats.

In very young embryos the red globules change very quickly after death, from being discs, becoming in many cases hemispherical or spherical, then softening, becoming diffluent, and running one into the other; some also become indented and plicated irregularly, the circumference of the globule becoming generally irregular.

The author proceeds to speak of the nuclei of embryonic blood-globules. In almost but not quite all embryos, from the time that the globules are first found to that when the embryo attains twenty-five or thirty millimetres in size, these globules contain nuclei; but about that period nearly half of the globules are deficient in any nucleus, and the number of these goes on increasing. At the fourth month of intra-uterine life occasional globules contain nuclei, but after this time none are to be seen containing them. The globules which towards the fourth month retain a nucleus are from one to four millimetres longer than surrounding globules. As to the nucleus itself, it is spherical, of three or four thousandths of a millimetre in size, greyish, insoluble in water, as also in acetic acid, which contracts it slightly, and renders its edges clear and its centre brighter. It is finely granular, and without any nucleoli, and pretty often situated at a point slightly removed from the centre. When placed centrally, the nucleus masks the central depression of the discs, which, when the nucleus is on one side, is well seen.

The embryonic blood-globules which possess nuclei contain, especially in the herbivora, one, or rarely two, very small fatty granulations, often disappearing after distension with water.

PART II.—PATHOLOGICAL MICROLOGY.

TUMOURS, MORBID GROWTHS, EXCRESCENCES, ETC.

Papillary Tumour.—M. Maier, of Freiburg,‡ communicates at some length the history and description of a tumour of this nature. It was removed from a man aged seventy-six, who for some time had experienced loss of vision in the left eye, and eventually blindness, along with giddiness and loss of memory. Death occurred in the form of apoplexy, owing to extravasation of blood into the central part of the left cerebral hemisphere. The tumour, which was of the size of a walnut, was found covering the sella turcica and neighbouring parts. It was lobulated, of a whitish-red colour, and moderate consistence, and apparently originated, as it were, by numerous filaments, mainly from the dura mater, crossing a part of the sella turcica, and pushing aside and flattening one of the optic and olfactory nerves, also compressing the pituitary body and both carotid arteries. On section of the tumour it was found to be surrounded by areolar tissue, which passed deeply between the lobules composing it, and to consist of a network of trabeculæ, between which occasional cells existed. The trabeculæ in some places had the characteristics of fibrillated, twisted areolar tissue, forming close meshes, whilst in other places they were sharper in contour, and obviously composed of partly simple spherical nucleated cells and partly spindle-shaped cells, and oftentimes at the extremeties of such trabeculæ isolated fibre-cells existed, as if engaged in the formation of fibre. Sometimes also these fibre-cells were more clearly seen to be departing from a greater branch or stem, as it were, and forming small trabecular work, such outgrowth appearing like papillary growth proceeding from the general stroma. These offshoots were often more homogeneous, and nuclear formation was only seen at the edges, the fibre formation coming on later. Along with the areolar mesh-work other structures existed also, in the form of trabecular scaffolding, which consisted of pale, vitreous-like, transparent, cylindriform trabeculæ, of various forms and thickness, and showing lateral out-growths, &c. This structure was but little influenced by the action of water added to it, but acetic acid caused it to shrink and exhibit clefts, and an alkali quickly destroyed it. In many of these hyaline trabeculæ, blood-vessels were seen included as if in a sheath, and the new formation of blood-vessels could be traced by spindle-shaped cells, as well in the case of old vessels surrounded by areolar tissue as in those surrounded by hyaline structure. At one time, in the walls of the old vessels nnmerous nuclear formations were to be seen, from which elongated spindle-shaped cells proceeded. These new structures are at first solid in such formations, but soon the cavity appears, which becomes filled with blood-corpuscles, and which can be traced from the old vessel to the point where a new lumen begins to form. At another time there exist more homogeneous partitions, with nuclei and a lumen, as in continuation of pre-existing vessels. In the hyaline pouches, as well with as without vessels, here and there exist pale, round, or elongated cells embedded, but often also spindle cells, as in young areolar tissue. Inasmuch as it is only vessels of small diameter, or recent formation which possess such hyaline sheaths, so is it presumable that these hyaline cylinders become converted into trabeculæ of fibrillated areolar tissue of the vessels, surrounded by transparent sheaths, which are, later on, to be sought for in the midst of the fibre-bundles of areolar network, in like way as the trabeculæ formed of spindle-cells are only transitional steps to the decidedly fibrillated areolar-tissue bundles. Besides the mutual interlacing of these trabecular formations and the new offshoots, in many places these structures, as well as the hyaline transparent cylinders, have spiral forms, and give rise to peculiar formations. For instance, the offshoots often have the cactus-like and often a tufted formation. By longer growth these rounded projections become more separated from their orgin, and hang like berries on their stalk; their stem is often broken, and rounded structures, like balls, are found remaining, which are either quite solid, and consist of spindle-shaped cells, or they may be homogeneous and transparent rounded bodies, and may have a line of separation between their periphery and the centre. Very often the original clear outline of spindle-shaped cells is lost, and we have left only more or less arched and connected markings, giving to the whole the appearance of partly concentric laminæ, and this is specially deceptive when spherical structures are accumulated within, owing to subsequent nuclear and cell formation, which, along with mutual pressure, makes the laminated condition appear clearer. Very often these structures resemble the corpora amylacea in look, and often they exhibit fatty changes in their interior, which also often occurs in the pedicles, owing to which their rupture takes place, and the bodies are isolated. So fatty have these structures become at times, that an appearance as of colloid is established. Often also hollow spaces are formed in the out-shoots from the trabeculæ, containing spindle or spherical cells, which generally contain or two, or more, nuclei.

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* Journal de la Physiologie p. 178. Jan. 1858.
† Brown-Séquard's Journal de la Physiologie, p. 285. April, 1858.
‡ Virchow's Archiv, p. 276. Sept. 1858.

On examining the dura mater in the neighbourhood of the roots of the tumour, the fibrous network of which it was composed was found to be in places interrupted or separated by small accumulations of nuclei and young cells, and here and there collections of spindle-shaped cells, along with the closely formed fibres. The roots of the tumour were chiefly of trabeculæ, of spindle-shaped cells, and vessels with or without homogeneous sheaths. After the above description which follows a very long clinical history, the author concludes by comparing the growth with like ones described by Henle,[*] Meckel,[†] Volkmann,[‡] Billroth, &c.; and shows its relation to villous cancer or cancroid, noticing the resemblance between the tubular trabeculæ and nerve-tubes or capillaries.

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[*] Zeitschrift für rat. Med. Band iii. Heft 1. 1844.

[†] Annalen des Charité-Krankenhaus, Jahrgang vii. Heft 1.

[‡] Archiv für Ophthalmologie, Band i. Abth. 1, s. 418.

Observations upon the Form of the Alveoli of certain forms of Cancer, and the disposition of the Cells within. By Dr. E. Wagner, of Leipzig.* The author allowing that in most cases of cancer the alveoli, when they exist, possess a roundish form, and that the contained cells observe no special disposition, points out that in many cases of undoubted cancer the arrangement of the alveoli is that of a gland or pouch-like in character, with numerous modifications, and that the cells are disposed in an epithelium-like manner, the increase of the cells probably proceeding from the peripheric cell-layers for the most part.

He goes on to point out some species of tumours described by authors, in which a similar form of alveoli and a similar disposition of contained cells has been noticed, as by Reinhardt, Billroth, Förster, Rokitansky, Robin, &c.; and then he gives in detail, and at length, 7 out of 40 cases of cancer examined, being chiefly though not entirely affections of the uterus, vagina, &c., in which the morbid deposit consisted of gland or pouch-like closed spaces, limited by a distinct membrane, and filled more or less with cells, together with a very slight amount of intercellular substance. The cells located at the periphery were always of a cylindrical form, and observed a regular epithelium-like arrangement, and were apparently quite unconnected with the areolar tissue corpuscles of the adjoining stroma. In the centre were cells more or less of a polygonal or round shape, without any order, sometimes only a nucleus being in the centre. The author supposes that this form of cancer may exist in all organs. He looks upon the growths of these new formations as analogous to that of many glandular tumours. There is also a resemblance to villous cancer, but in the latter case the stroma is central and the cells outside, whilst in these tumours it is peripheric, the cells being situated with inclosed cavities. As to the exact point of origin of the pouch-like alveoli of this form of growth, the author appears to be by no means certain. He here alludes to the observations made by Förster on the origin of gland-like cavities out of areolar-tissue corpuscles, and those by Virchow on the origin of pearly tumours from the same source. He seems to think it most probable that these alveolar structures may arise from areolar-tissue corpuscles. The changes observed by him in the contents of these alveoli were into a fatty and a mucus-like material.

Cysto-Carcinomatous Growths.—Dr. J. Woodward† read before the Biological Department of the Academy of Natural Sciences of Philadelphia, the history of three such cases. In the first, the growth was removed from a healthy girl, aged sixteen, from between the right scapula and the spinal column, and consisted of a fibrous sac filled with clear straw-coloured fluid. In the upper part of the cyst a soft pultaceous mass was attached to the inner surface, and also implicated the neighbouring muscles through the wall. The cyst-wall, which presented an areolar arrangement on its inner surface, consisted chiefly of white fibrous tissue, along with some yellow elastic tissue, embryonic elements being very abundant. The contained liquid coagulated on being left to stand, and after removal of the clot the remainder coagulated also under heat and nitric acid. No microscopical forms existed in the fluid. The soft pultaceous mass consisted of granules and nuclei embedded in a transparent matrix, the nuclei being chiefly oval or elliptical, with granular and fat contents. Some were angular, elongated, or irregular, the wall of the nucleus having sometimes a double contour, and not being affected by acetic acid. The patient seemed to have died of so-called secondary deposits very shortly after the operation.

The second case was that of a man, aged seventy-six; the tumour was removed from the axilla, and consisted of a very large sac of fibrous tissue, containing one pint and a-half of pink, grumous, and bloody fluid. Soft pultaceous masses adhered everywhere to the inner surface, and at certain parts infiltrated the surrounding tissues. The cyst wall and the pultaceous mass showed the same microscopical elements as in the former case, but the grumous contained fluid in this case also contained nuclei. Three months after the operation there was a return of several small tumours in the cicatrix, which ran rapidly through the course of encephaloid cancer. These secondary growths did not present the characters of the original one, but were found to be composed of large nucleated cells of diverse shapes, like those found in ordinary medullary cancer.

In the third case, that of a man, aged forty, the growth was on the back of the left thigh, and had been once before removed. The cyst was like the preceding one, but was quite *filled* with a soft, pultaceous, brain-like mass, infiltrating the wall and surrounding parts. This mass was composed of cells as in the two former cases, but contained also a few cells containing similar nuclei, and having the character of those usually found in ordinary cancer. The tumour returned in a medullary form, and caused death.

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* Archiv für Physiol. Heilkunde, Heft 5, p. 306. 1858.

† Hay's American Journal, p. 54. July, 1858.

The author looks upon the fluid as the blastema of cancer, giving origin to the pultaceous mass, and considers these cases to be instances of several conditions of one form of growth which differs from cancer in having a distinct sac limiting to a certain extent the primary exudation of the blastema, also in the unusual quantity of blastema, and in the slow development of nuclei from the blastema and of cells from the nuclei. He points out the resemblance between this supposed fluid blastema on the one hand, and the liquor sanguinis and plastic inflammatory exudations on the other, in the fact that fibrin as well as albumen was contained, in opposition to the theory of Rokitansky that cancer diathesis is essentially an albuminosis, and in support of that of Wedl, that the fundamental character of cancer is that of an aborted and degenerating new formation of connective tissue.

NERVOUS SYSTEM.

Ulcerated Neuroma in the Palm of the Hand, of the size of the Fist.—This case is related by Dr. R. Volkmann,* and was one in which the main branch of the median nerve was affected also. The patient, a girl of ten years of age, had for a year suffered from weakness in the left arm, and pain in the left forefinger, which finally became most acute, lasting for several hours. The course of the growth was as follows. The muscles of the hand, and especially the forefinger and thumb, became greatly wasted, an anæsthesia of the second and third fingers came on, as well as of the forefinger and thumb in part. In other respects the girl was up to this time in good health. Afterwards a small hard tumour of the size of a pea appeared at the fold separating the root of the forefinger from the palm, and quickly extended into the palm, becoming larger and softer, and at the last fluctuating. The pain became less acute, more limited to the growth, and paroxysmal. Eventually the growth extended from the base of the forefinger to the ball of the thumb, the surface being blue, polished and tense, and the veins covering it of a purple-red hue.

The median nerve along its entire course, from the wrist to the shoulder, was sensitive to pressure, but specially so at one place in the wrist where a small swelling was felt in the course of the nerve. As to the diagnosis, notwithstanding the external resemblance to soft cancer, the want of a general constitutional disturbance and of any glandular affection, were facts militating against the supposition. The tumour was punctured, and a yellowish slimy fluid came out, along with a yellow gelatinous, softish, solid matter, showing beneath the microscope pale spindle-shaped cells, with large dark elongated nuclei almost filling up the cells. After puncture the tumour increased, the surface becoming of a dark reddish-black colour and ichorous, and the above-mentioned swelling of the nerve becoming enlarged and very painful. Hæmorrhage came on, and led to extirpation of the growth, which at that time extended from the roots of the second and third fingers to the wrist, growing round the metacarpus of the thumb, and appearing at the back of the hand. The portion of the growth which had been exposed by ulceration was partly of a blood-red colour, owing to extravasation, and was semi-transparent, and consisted partly of soft irregular masses of a yellowish-red colour. There was no trace of medullary juice, but a slightly fibrous texture appeared after maceration.

The remainder of the growth consisted of a yellowish, and in places rose-tinted, mass. The median nerve was found in close connexion with the growth, and was at this point thickened to the extent of from eight to nine millimetres, being of a yellowish-red colour, and at the edges very transparent. At the wrist there was a swelling upon the nerve of ¾″ in length, and slightly spindle-shaped. At this swelling, owing to a division of the thin connective-tissue-covering of the nerve during the operation, an extremely soft and yellowish lumpy mass protruded. Between this enlargement and the base of the tumour the nerve was of the same size as when it was divided on resection of the growth, and the investing membrane then was spread out in bands, enveloping the base of the growth as an imperfect capsule. The investment of the nerve in front of the tumour was divided lengthways, and was found to be filled with a yellowish-red gelatinous mass, which passed into the substance of the large tumour; whilst the remainder of the nerve was found in the middle of the growth as threads of a reddish yellow colour, three branches of a rather firmer character being traceable over the expansion covering the growth. No trace was found of that branch which ordinarily supplies the adjoining sides of the thumb and first finger. The spindle-shaped swelling, which appeared to have formed the starting place of the disease, was full of a gelatinous mass which, when washed, had in many places a fibrous appearance, with a tendency to splitting up into bundles (*carcinoma fasciculatum*). In the soft parts the texture was soft and homogeneous. On microscopical examination, the mass was found to show multitudes of darkly-contoured round or oval nuclei, with granular contents, and one or more clear nucleoli; whilst in some places the nuclei were rod-shaped. The round ones were rather larger than blood-globules, and the rod-shaped ones were from 0·0040‴ to 0·0091‴ long.

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* Virchow's Archiv, Band xii. Heft 1, p. 27.

Where the growth was more rapid the round ones predominated, and in some places fatty changes were observed; whilst the rod-shaped nuclei were more abundant where the growth was covered by an investing neurilemma, and were found to be nuclei to very pale spindle-shaped cells. The entire growth was well supplied by capillaries, and in the interior, at one place, a cavity was found filled with slimy fluid, but without distinct walls or epithelium. As to the three branches of the median nerve stretched over the tumour, they were found to be in a state of complete degeneration, only showing longitudinally-striped bands with numerous oval nuclei; the surrounding connective tissue being for the most part very fibrillated. In addition, in the main branch of the median nerve where it passed into the large tumour, and in the spindle-shaped enlargement, several very soft thin transparent filaments were seen, showing under the microscope an unchanged areolar investment, striated longitudinally and transversely. These filaments yielded on pressure a soft material, consisting of round and oval nuclei and cells. As the median nerve was diseased as high up as where section was made, it was impossible to say to what extent up the arm it was affected.

After describing the growth, the author proceeds to the important question of the method of propagation of this growth. He determines against the supposition that the areolar tissue investing the nerve and passing between the nervous bundles was the seat, inasmuch as the outer cellular covering of the median nerve was not at the same time affected. He supposes that the diseased process advances lengthwise along the connective-tissue tubes, which correspond to the dwindled nerve bundles; and that after atrophy and destruction these allow of the confluence of the cell products. He goes on to say, that possibly the new formation arises from the growth and separation of the nuclei of the limitary membrane of the primitive fibre, as the histological elements of the growth, where more recently formed, showed every transition to the nuclei arising in paralytic degeneration. This latter idea received support from the fact, that the so-called fibres of Remak, of intestinal nerves, with which the degenerated nerve-strands are very comparable, give out, on pressure, a finely-granulated contents, along with those nuclei which so clearly exist in the course of these fibres; and also from the observation that in the diseased nerve in the present case, far from the tumour, a similar granule-containing fluid with nuclei could be expressed from the degenerated nerve.

No traces of any doubly-contoured nerves were visible in the skin of the fingers; and the bones, although very atrophied, were natural in structure. The author imagines the disease to have originated before the full growth of the patient. Three months after the operation, the median nerve as high as the elbow was very painful on pressure.

Colloid Cyst in the Third Ventricle of the Brain, with Lipoma of the Choroid Plexus.—Dr. H. Walmann, of Vienna, relates a case* of the above affection, associated with hydrocephalus, atrophy, and softening of the lower part of the spinal cord, colloid cyst of the thyroid gland, and fatty state of the heart and kidneys, &c. The patient was a soldier, aged fifty-two, who had lived freely, and who was paraplegic before death. In addition to the dilated condition of the ventricle, and wide separation of the optic thalami, there was found situated beneath the fornix, an oval cyst with thick walls, and of the size of a walnut, springing from the central choroid plexus, and surrounded by a very delicate transparent membrane-part of the arachnoid. It was connected with the "commissura mollis" also. The interior of the cyst consisted of a gum-like tenacious yellowish-brown mass, and at its edges the growth was transparent and of a yellowish-grey look. It slightly occupied the third ventricle, the floor of which was softened and thinned. The lipoma before spoken of was of the size of a bean, being soft and lobulated, and of a yellowish-white hue, and was attached to the right choroid plexus. On microscopic examination, there was found beneath the outer arachnoid covering an investment of areolar tissue half a line in thickness, and within this was a mass of colloid or glue-like material.

The tumour connected with the choroid plexus consisted of an áreolar-tissue investment, and a mass of vascular connective tissue containing a vast number of fat-cells grouped together the vessels, the mass being continuous with the plexus. The affected portion of the spinal cord showed connective tissue-fibres, fat, and molecular material, along with many delicate nerve-fibres, the nerve cells being beset with much fatty matter.

The author alludes to a case by Rokitansky of lipoma of the inner surface of the dura mater, and the ependyma of the venticles; and to one by Meckel, of lipoma beneath the optic commissure, of which the preparation now exists in the museum of the Joseph's Academy at Vienna.

Ossified Lipoma of the Brain.—Dr. L. Benjamin, of Hamburgh,† relates a case of this nature. The patient died of diseased kidney, but had been paralysed on the right side, and was the subject of epilepsy. The left cerebral hemisphere was found to be very hardened, and a tumour resting on the right optic thalamus and slightly touching the corpus striatum was found, chiefly occupying the right cerebral hemisphere. This tumour possessed an areolar investment, which was firmly connected with a fatty layer beneath it, and below this latter layer was a hard mass forming the bulk of the tumour, but which at one part of the posterior surface was not covered by the fatty layer. At this part it was seen to be invested by a periosteum-like second capsule of areolar tissue, similar to the one first described, only firmer. The fatty layer was penetrated by projections of connective tissue from the outer areolar layer, and was very like to lipoma, as well in general as in microscopical appearance. From it passed into the right lateral ventricle a pedunculated pear-shaped mass, and there it was found united with the choroid plexus. The peduncle of this mass was lobulated and clustered.

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* Virchow's Archiv, p. 885. Sept. 1856.

† Ibid., Band iv. Hefte 5, 6, p. 552.

On removal of this fatty covering, the periosteum-like membrane investing the mass of the tumour was arrived at. This was bigger than a walnut, of stony hardness, and consisted of one larger and several smaller irregularly-formed portions united by a firm areolar tissue. The small ones were posterior, uncovered by fat, and could easily be cut with the bone-scissors. On section, it was found to be like the spongy part of an apophysis, with very small meshes filled with fat, no cartilage being discovered, and in the softer parts the bone-stroma was much finer, and contained more clear yellow fat. The harder parts contained, however, well-marked bone-structure, the Havershian canals being of unequal diameter, and concentric bony lamellæ being easily seen especially in the very hardest portions.

In the softer parts, Haversian canals and concentric bony lamellæ were wanting, although bone corpuscles existed. Where the delicate bone-stroma was covered with fat, areolar tissue existed, in which the origin of bone corpuscles by calcareous deposit around could be traced. The author alludes to the rarity of lipoma of the brain, and states that only one case exists in the Vienna collection; also to the rarity of even calcification of lipoma in man, although tolerably common in the lower animals.

He remarks upon the ossification of the growth without any connexion with the bony parietes of the skull, and concludes by commenting upon the change of areolar-tissue corpuscles into fat cells, the formation of new bone from areolar tissue in soft parts, spoken of by Virchow, and the investigations of H. Müller concerning the calcification of the neighbourhood of ramifying cells, which appears to him to be the only method of development of normal bone.

DIGESTIVE SYSTEM.

Multilocular Ulcerating Echinococcus Tumours of the Liver.—Dr. Schiess, of St. Gallen,* describes a tumour of this nature associated with enlarged spleen removed from the body of a woman, aged fifty-nine, who had been the subject of jaundice, itching of the skin, ascites, &c. In the middle part of the liver, the right lobe of which was atrophied and cirrhosed, existed a whitish knotted and raised mass, the most prominent part of which consisted of fluctuating projections of the size of an egg, covered by thickened peritoneum, and rendered uneven by numerous small yellowish transparent bodies of the size of hemp seed. On making a section of this, a quantity of thin greenish-yellow and turbid fluid gushed forth from large hollow cavities lined by a thick membrane, and evidently consisting of dilated bile-ducts. The small elevations crunched under the knife, and contained partly a granular material capable of being turned out in cylindrical masses.

On complete division with the scalpel, behind the cavernous substance before described, a large mass was brought to view, of the size of a child's head, in part limited by the cavernous substance and in part by peritoneum merely. Inwards existed a thick areolar-tissue stroma in which were insulated portions of liver substance, and in which many large and small cavities were seen, with distinct lining membranes, and filled with a brawn-like transparent substance. Still deeper a hard stroma existed, in which the above-named cavities were, but the brawny masses became less numerous deeper down. Beyond this the stroma was found of a yellowish colour, softened, and broken down, with here and there calcareous spots. In the centre it was so soft that it might be compared to rotten pumpkin. On microscopical examination of a section of the hindermost mass before described, a number of small, partly isolated, partly accumulated echinococcus vesicles with their characteristic laminations were brought to view, the smallest showing a dark granular contents with calcareous corpuscles. In one of the larger ones the remains of the animal were found as an oval body, surrounded by a structureless membrane, closely beset by a brownish-red granular pigment. In this the

* Virchow's Archiv, p. 371. Sept. 1866.

hooklets so well known were seen. As to the stroma, the outer part of the tumour consisted of a structure possessed of the characters of normal areolar tissue with its anastomosing cells: more inwards, where the breaking down began, a fatty and calcareous metamorphosis was seen, commencing in the areolar-tissue corpuscles, and surrounding the echinococci as centres of calcification, the transparent vesicles thus contrasting with the neighbouring dark spots.

The above case is considered by the author as supportive of the view held by Virchow, that sterile echinococci vesicles may exist. He concludes by allusion to the similar cases already published, as by Virchow, Kuhl, Luschka, and Heschl, &c.

HALF-YEARLY REPORT ON FORENSIC MEDICINE, TOXICOLOGY, AND HYGIENE.

By Benjamin W. Richardson, M.D., L.R.C.P.

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I. Toxicology.

Asphyxia from Imperfect Combustion of Gas.—The case of a gentleman found dead in his bath-room, and whose death was attributed to inhaling air rendered impure by a gas stove used to warm the room, was communicated a short time since to the Boston Society for Medical Improvement. The Society appointed a committee to inquire into the matter, which reported that the fatal result was adequately accounted for on the hypothesis aforenamed. In support of this view, the committee relate an experiment in which they exposed a cat to the air of a room heated to 90° Fahr. by one of the gas stoves. The cat was placed in an iron cage elevated two and a half feet from the floor, the room being eight feet square and ten high. The first thing noticed was a "snapping of the eyes" (qy. jaws, but we write as we read) then a crying (as is frequently heard in the night from cats), this becoming in fifteen or twenty minutes loud. In a short time this crying stopped. The mouth moved, but without any noise. She sneezed fifteen or twenty times, and rubbed the nose and face with her paw; afterwards tried to bite the iron grating of the cage. There was a flow of watery fluid from the mouth, but without any frothing. The mouth was open most of the time. While lying down she would try to get on her hind legs, and would fall over immediately. In thirty minutes there was a tremulousness and throwing back of the head. The respiration became long and stertorous. Convulsive movements came on over the epigastrium, which increased over the body generally, and in forty-eight minutes the animal was dead.—*American Journal of the Medical Sciences, April*, 1858.

[The symptoms produced in this experiment are, as we infer from certain analogous experiments of our own, due rather to exposure to the heated and dry air than to the inhalation of a poisonous vapour arising from imperfect combustion of gas. In our experiments we found the same symptoms in cats where the possibility of any poisonous inhalation was excluded. The symptoms are, moreover, not those of pure asphyxia, while the post-mortem appearances in our observations yielded lungs free from congestion, and a condition of blood in which the venous blood, from its rich redness, could not, by mere examination, be distinguished from the arterial.]

Poisoning with Scheele's Green.—Dr. Schroff has performed a series of experiments with this substance. From these inquiries he draws the following conclusions:—1. Commercial Scheele's green and chemically pure arsenite of copper agree in their effects on the animal economy, only the latter is in proportion the more poisonous. 2. The symptoms during life and the appearances after death are those which arise from arsenious acid, while those of copper poisoning are absent. The passage of arsenic into the blood and urine was ascertained in all Dr. Schroff's experiments, both with Scheele's green and with pure arsenite of copper. 3. The influence of the poison is localized chiefly in the stomach, especially when that organ is full and the poison remains a long time in contact with one part of the organ. No great effect was produced in the experiments on the rest of the alimentary canal, which is very different from what occurs in poisoning by metallic arsenic. 4. Compared with other arsenical compounds, arsenite of copper is not the most poisonous if the quantity be taken into account. A decigramme of Scheele's green or of the pure arsenite of copper did not kill rabbits, while the same quantity of arsenious acid, arsenic acid, and of arsenite of potassa, constantly proved fatal. 5. In regard to the rapidity with which the poisonous effects are produced, arsenite of copper is allied to the soluble preparations of arsenic. The quantity of the poison bears a direct proportion to the rapidity of its operation and the period of death.—*Zeitschrift der k. k. Gesellsschaft. der Aerzte zu Wien*, Jan. 11th, 1858.

Magnesia as an Antidote in Poisoning by Cobalt and Arsenic.—From a series of comparative experiments, Dr. Schroff has also arrived at the following conclusions relating to magnesia as an antidote to arsenic:

1. Death followed the administration of cobalt to four rabbits, at periods of from fifty to fifty-six hours, in one case in twenty-six hours. In three animals treated with metallic arsenic the periods of death were eighteen, sixty, and fifty hours. The symptoms arising from the two metals and their duration are not very different, and the occasional difference in the duration is easily explained by the circumstance that death occurs more rapidly in rabbits when diarrhœa has set in. The difference in the symptoms during life depends on the circumstance whether the poisonous powder has adhered to the same part of the stomach or cæcum, so as to have a longer or shorter time for becoming dissolved, and producing a local effect, or whether the whole powder has become distributed over the whole surface of the mucous membrane. In one experiment the local phenomena were less extensive than intense; inflammation ensued, and proceeded to sloughing, and the symptoms were prolonged. In another experiment the local effects extended over larger portions of the alimentary surface, and their intensity was small; nevertheless, death occurred rapidly, either in consequence of the greater extent of surface acted on by the poison, which was thus placed in a more favourable condition for being absorbed, or from the supervention of diarrhœa. The amount of food in the stomach also exerted a great influence on the period of appearance of the symptoms. If the stomach were full, the powder remained in contact with the same part of the mucous membrane; if the stomach were empty, the powder was rapidly spread over the alimentary canal.

2. The quantity of poison does not seem to exercise, within certain limits, any influence on the duration of the symptoms. One decigramme of cobalt produced death in fifty hours in a full-grown rabbit; six decigrammes of the same substance killed a rabbit four months old in fifty-six hours. Thus death was produced in a strong animal by a sixth part of the dose which was fatal to a weaker animal. In comparing similar quantities of arsenious acid, cobalt, and metallic arsenic, the latter were found to act most intensely, and this markedly with arsenious acid if it were administered in the form of a dry powder. In solution, however, the cobalt surpasses the other two in poisonous properties. Cobalt and metallic arsenic doubtless undergo in the alimentary canal such changes as render them capable of absorption, as is proved by the presence of arsenic in the urine after their administration; but whether arsenious acid or chloride of arsenic be formed, Dr. Schroff does not decide. These experiments bear out Buchheim's statement, that the poisonous properties of arsenical preparations are in proportion to the amount of arsenic which they contain; but account must be taken of their power of being dissolved and absorbed.

8. Magnesia, in the form of hydrated oxide, or calcined, acts without doubt as an antidote to cobalt; the same obtains with arsenious acid if it be not dissolved before being introduced into the stomach. But when arsenic is introduced in the dissolved state it is rapidly absorbed, and antidotes, however speedily given, come too late; nay, when the poison and the antidote are given together, the antidote is of little avail. At least, in all his experiments with solutions of arsenious acid and with arensite of potassa, Dr. Schroff found antidotes of no avail, provided that the quantity of poison were large. In most of these cases death followed sooner than when no antidote was used, while the local changes were less intense. When cobalt and metallic arsenic are introduced into the stomach in the form of powder, and magnesia is administered, the magnesia has sufficient time to form, particle with particle, an insoluble compound, and thus to render the poison innocuous.—*Ibid.*

Detection of Arsenic in a Body that had been Burned.—A woman, forty years of age, who had long suffered from an insatiable appetite and from diarrhœa, and who lived very unhappily with her husband, died with symptoms of violent pain and repeated vomitings. The husband having been informed of an impending judicial examination of the body of his wife, set fire to his house, whereby the body was burned to a shapeless mass. The stomach and œsophagus were nevertheless tolerably preserved. In the interior of the stomach there were present several white granules, which on chemical examination yielded arsenic by the usual tests. Arsenic was also obtained from the œsophagus, stomach (structure of), and duodenum, as well as from some vomited matters which were found lying in the front of the house in which the deceased woman had resided. Some reddened patches were present in the œsophagus and duodenum, which could not have proceeded from the action of the fire, as parts of the same organs, which from their position were much more exposed to the heat, were almost in their natural state. The husband ultimately confessed both the facts of poisoning and incendiarism, but died before the trial terminated.—Dr. Schäffer, *Vjhrsch. f. ger. Med.*, Juli, 1858, and Clarus in *Schmidt's Jahrbücher*, October, 1858.

Arsenicated Wall Papers.—Dr. James Whitehead, of Manchester, is of opinion that arsenicated papers which are perfectly smooth and properly glazed are probably harmless, but that it is altogether different with the tufted or flock papers, and also with flat papers which are unglazed. Dr. Whitehead gives the subjoined case, in which symptoms were attributed to arsenical poisoning:

"In the autumn and winter of last year, I attended a youth labouring under symptoms of arsenical poisoning—viz., aphthous ulceration of the gums and tonsils, violent frontal headache, great languor, nausea and occasional vomiting, inappetence, diarrhœa, and disturbed sleep.

"The complaint, mild at its outset, gradually increased in severity, spite of treatment, and at the end of eight or ten weeks, the patient was removed to the country, where he was speedily restored to health. I had repeatedly expressed a suspicion that the symptoms were those of effluvial or other kind of poisoning, and the supply of water and state of the drains were consequently examined; but in these no cause of complaint was found.

"On his return home, perfectly well, he was placed in the same apartment; but in the space of four weeks, was worse than before. He had spongy gums, diphtheritis, violent neuralgia of the forehead and face, great languor, occasional diarrhœa, and emaciation. Attention was next directed to a cistern placed against the wall outside his bed-room, which it was thought might possibly have had a share in producing the mischief, and it was therefore subjected to alteration. This measure necessitated a change of apartment for the space of a fortnight, at the end of which period he was the third time located, again in perfect health, in his own room. In the space of three to four weeks after his restoration, the symptoms reappeared in an aggravated degree of severity, and this time it was believed, as had already been several times suggested, that the cause of all the annoyances would be found in the paper with which the apartment was lined. Such being the conviction, both of myself and the family, the paper was replaced, without loss of time, by one of a totally different tint, and was followed by results every way satisfactory. The youth, who has continued to occupy this room from that time until now, remains perfectly well, having had no relapse of symptoms since the removal of the green paper.

"The apartment in question, formerly used for other purposes, was converted into a sleeping-room in the summer of last year, 1857. Its walls were covered with a rich green tufted or flock paper which was placed thereon about four years ago; and it was remembered that the workman who was engaged to hang the paper had made use of the expression, during the operation, that he disliked working with that sort of paper, as it always made him ill. During the process of papering, the floor of the room was coated with green dust, and since that time, whenever the room was cleansed, the dust which rested on the furniture always imparted to the articles used in the process of cleansing a green tinge, showing that particles were constantly being detached from the wall. When used for a few days in succession as a sitting-room, as it had been formerly, the occupants began to feel uncomfortable, and were, from that circumstance, induced to desert it for a period.

"Having procured a portion of the detached paper, I scraped off from the raised parts a quantity of green powder, which was submitted to chemical examination. Thirty grains were sent to a chemist, with a request that he would ascertain whether the article contained arsenic, and if so, in what proportion. The remainder was examined by myself. The results were as follow:—

"1. Note from the chemist:—'I find the quantity of arsenious acid contained in the thirty grains of substance which you forwarded amounts to about eleven grains.'

"2. A quantity of the green powder thrown upon a hot iron emitted an odour of garlic, characteristic of volatilised arsenic; mixed also with a peculiar metallic flavour.

"Four grains (all that remained of the powder) were boiled in four ounces of water in a glass vessel, and submitted, after filtration, to the following tests:—

"3. Ammoniacal nitrate of silver threw down a pale brownish precipitate.

"4. Ammoniacal sulphate of copper gave no result.

"5. Sulphuretted hydrogen gave no result.

"It is highly probable that the quantity of the green powder used in making the solution for the last three experiments was too small; and moreover, by an unfortunate mistake, the solvent employed was not distilled, but spring water. This would be sufficient, probably, to render the tests inoperative.

"The fragment of paper from which the green powder was obtained measured less than a square foot in extent, and the ascertained superficies of wall covered by the paper was 350 square feet. Allowing, therefore, that eleven grains were held by the portion experimented upon, it follows that the quantity of arsenious acid remaining on the wall-paper, after four years' usage, would amount to at least 3850 grains."

Although the tests Nos. 4 and 5 gave no indication of arsenic, Dr. Whitehead believes that arsenious acid, and probably arseniuret of copper, formed the deleterious agents in this paper. —*British Medical Journal*, Sept. 25th, 1858.

Identification of Arsenic and Arsenious Acid.—Dr. Guy has introduced a new method for the production and identification of crystals of arsenious acid and crusts of metallic arsenic. By this method the processes of reduction and of conversion from metal into arsenious acid take place in a tube used only for that purpose, and the deposit is received on a flat surface free from any admixture of extraneous matter, and affording complete facilities for microscopic examination. The tube used by Dr. Guy is smaller than the smallest-sized specimen tube in common use, of larger bore than the reduction-tube commonly employed, and about three-quarters of an inch in length. The tube is supported in a vertical position by being dropped into a hole punched in a slip of copper foil. Into this short tube the powder or mixture is dropped. The tube suspended in this manner is held by the left hand with its sealed end in the point of the outer flame of the spirit-lamp, while a piece of clean microscopic glass, large enough to cover the mouth of the tube, is adjusted with the right hand. After a few seconds, the glass is covered with a circle of sparkling crystals or with a distinct metallic crust, either of them in a state most favourable for further examination. In regard to the advantages of this plan, as far as the crystals of arsenious acid are concerned, the advantage is limited to the greater ease with which the crystals can be examined and identified under the microscope. When dealing with a very small quantity of the acid, this advantage is a real one, for cases occur in driving a small metallic crust up and down the old form of reduction-tube, and in identifying the coating of the slips of copper used in Renish's process, where crystals which could not be identified by lens or microscope on the inner surface of a small reduction-tube, may be readily recognised on the flat surface of a piece of microscopic glass. But it is in testing for arsenic by the process of reduction, that the advantage of a flat surface for the deposit of the metallic crust chiefly displays itself, for not only does the thin glass coated with arsenic lend itself readily to the further process of identification by the formation of crystals of arsenious acid, but the metallic crusts may often be recognised by distinctive characters when examined under the microscope.

The microscopic appearances presented by the thicker crusts are highly characteristic. When viewed by reflected light, they either present buff-coloured masses in strong relief on a ground of uniform tint and texture, some of these masses being distinctly octohedral, or the surface, though uneven, is less irregular, and consists of small bright spots, sometimes arranged in a zig-zag pattern on a flat surface variously tinted, from which surface brilliant triangular facettes, and distinct transparent octohedra, project in large numbers, mixed with less-regular shaped spots of blue, brown, grey, yellow, and red. The octohedra in the first variety seem to consist of arsenious acid tinted on the surface by a thin layer of, or powder of metal. In the second variety they evidently consist of arsenious acid which has escaped decomposition. The crusts just described are, it must be understood, obtained from arsenious acid reduced by charcoal, and they are in every case a mixture of the metal with its oxide. The appearance of the metal itself is quite different, and such as even a practised chemist would not be prepared to expect. When the very same mixture of arsenious acid and charcoal is introduced into a specimen tube of the same kind, and covered with bicarbonate of soda, so that the vapour of the metal may be deposited unchanged in an atmosphere of carbonic acid gas, the resulting crust of pure unmixed metallic arsenic, when examined by the microscope, is found to consist of distinct globules not distinguishable at the thin edges of the crust from globules of mercury, but distinguished at the thicker parts of the crust by resting on a ground of uniform colour and texture, sometimes grey, sometimes copper-coloured, sometimes of the two colours blended. The globules of arsenic behave in every way as do those of mercury. They run together into larger masses, and they undergo the same change of shape when submitted to pressure. The only difference between them is in the temperature necessary to maintain them in a liquid state.

Dr. Guy states that by this means so small a quantity as the thousandth of a grain of metallic arsenic may be readily converted into arsenious acid, and easily and completely identified.—Beale's *Archives of Medicine*, No. III., 1858.

Poisoning by Sulphuric Acid.—An ingenious but somewhat distressed artisan, according to the statement of his wife, took on the 21st of September, 1857, at half-past three in the afternoon, about sixteen drachms of concentrated English sulphuric acid. During the exhibition of the effects, the symptoms were, sour, acid, bitter, styptic taste; sharp burning heat in the mouth, pharynx, œsophagus, and stomach; unquenchable thirst, hiccup, choking, and violent vomiting. The fluid vomited was of the colour of ink, mixed with flakes and mucous epithelium, and with red colorations, as from arterial or venous blood. Pressure over the stomach produced much pain, and there was very painful spasmodic contraction of the abdomen, and diarrhœa. The urine was entirely suppressed. In the throat and chest there was a dull pricking pain; the voice was peculiarly changed, being at first faint and croupy, and afterwards more and more feeble, until perfect aphonia occurred. The breath exhaled was very offensive; the respiration was embarrassed and stertorous, and during the vomiting there was dyspnœa. The pulse, at first at seventy, rose to ninety, and later to a hundred, becoming more and more feeble, thread-like, and trembling, and eventually imperceptible. The temperature of the body gradually decreased; the face, tongue, and breath became cold; the secretion of the skin cold, clammy, free, and of bad odour; from time to time there was violent shivering and shuddering, with extraordinary restlessness and convulsive movements of the muscles of the face, accompanied by groans and screams. Ultimately, this state subsided into one of collapse with general convulsion. Death took place on the 22nd of September, at three in the morning, the mind remaining entirely undisturbed.

At the post-mortem (date after death not given), the following appearances were found. There were the bluish-red death-marks on the back; the gums, the lips, the teeth, and cheeks, were covered with a brown sanguineous moisture; the abdomen contracted. The membranes of the brain were thickened, the brain-substance infiltrated with dark blood; in the cavity of the brain was more than an ounce of serum, and a portion of the right hemisphere was changed into a soft mass divided into cavities by serous fluid. On the surface of the wind-pipe and œsophagus was a black slimy fluid; both lungs were inflated and œdematous. The pericardium contained an ounce of serum. The heart was contracted; its ventricles and the larger vessels were filled with coagulated blood. In the stomach there was about a pound's weight of dirty, reddish, effused matter. The peritoneum covering the stomach was dark-coloured, injected, and ecchymosed; that covering the bowels was here and there in a similar condition; the liver was of dirty red, and bloodless; the gall-bladder contained a pale yellow secretion; the spleen was very small, bloodless, and tough; the duodenum was filled with gas and with a black, dark liquid; its mucous coat was raised, carbonized, and shrivelled; the intestines contained a brownish fæculent matter; the kidneys were of yellowish-white colour, and the bladder was contracted.—Dr. F. F. Pellischek, *Oesterr. Zeitschrift f. prakt. Heilk.*, 11–23, 1856; and Clarus, *Schmidt's Jahrbücher* Sept. 1858.

Effects of Poisons during Hunger and Repletion.—Dr. Köhler, in an interesting and very valuable paper, gives some curious evidence on the above subject. The animals subjected by him to experiment were rabbits, dogs, and pigeons; the poisons employed were strychnine, hydrocyanic acid, emulsion of almonds, and ether; and the absorbing surfaces where the poisons were applied or injected were the alimentary canal, the peritoneal cavity, the respiratory surface, and the cellular tissue. The experiments themselves are arranged in five groups, in each of which is noted the colour and sex of the animal, the duration of the inanition, the weight of the body, the quantity of poison used, the commencement of the action of the poison, the time of death, and the difference in the period of action in fed and unfed animals. It has been held by all modern writers, that by hunger the energy and activity of all the organs are reduced; but it has also been maintained that hunger favours, strongly, the absorption of poisons. In the paper before us, Dr. Köhler, admitting and demonstrating the former of these opinions, disputes the latter, and adduces his experiments in proof that hunger prolongs the absorption and the reaction of poisonous substances. In offering a reason in the way of explanation of this statement, Dr. Köhler argues that during abstinence from food, the absorption of oxygen decreases till death, and the proportion of carbonic acid evolved is correspondingly diminished. The blood cannot, therefore, circulate with its accustomed rapidity during hunger, hence poisonous substances are incorporated and borne along with the blood with corresponding slowness, and exert their influences on the nervous system with proportionate tardiness. Starvation itself, to a certain extent, does not, according to this author, cause any diminution in the quantity of blood. The quantity is made up by the fluids from the tissues, and the blood becomes thinner and altered in quality, but not less in quantity. The same rule obtains after a small venesection. If, however, a large venesection is made, or if inanition is allowed to proceed to a considerable period, then is there a true decrease in the quantity of blood and a contraction of vessel. In the experiments referred to, where the starvation was never made to extend beyond sixty-six hours, the above consideration does not enter.

Regarding the parts of the body into which the poison was thrown, as contrasted in cases of animals well-fed and animals starved, no difference seems to have occurred; the larger the

surface of contact, the quicker the absorption in both kinds of cases.—*Archiv fur path. Anat. und Phys.*, von R. Virchow, July, 1858.

Further Experiments with Bibron's Antidote for the poison of Reptiles.—Dr. Walker, of Gonzales, Texas, gives the following illustration of the effect of Bibron's antidote. He was called, on the evening of the 30th of June, 1858, to Benjamin Watkins, aged thirty-five, who had been bitten over the metacarpal bones of the third and fourth fingers of the left hand on the Sunday previous, by a black mocassin or cotton mouth of the South, a variety of the trigonocephalus piscivorus. The family being absent, he took a pint bottle of whiskey, and after drinking some of it, started to a neighbouring house, fell to the ground, arose and made another effort to proceed on the way, fell a second and a third time; after which he was unable to rise again. He remained where he last fell, in the sun, without water, from about 11 o'clock A.M., to 6½ P.M., when he was found and carried home. He drank about a pint of whiskey during the day; had some nitrate of potass administered on Sunday night. Tuesday, had some tea from the curcle burr, with various kinds of poultices to the hand and arm.

Dr. Walker found Watkins delirious, with occasional lucid moments; pupils much dilated; bathed in profuse perspiration; pulse 120, and very feeble. The hand very much swollen, with some sloughing, and immense sanious discharge. Forearm and arm swollen, with much purple discoloration, and extensive abrasions along the belly of the "biceps flexor cubiti," with sanious discharge. The muscles of the whole chest, back and front, were much swollen and very tender to the touch, with purple discoloration over the pectoral, deltoid, and scapular muscles of the left side, and dark purple appearance from the clavicle to the ilium of the right side; his breathing deep and laboured. Dr. Walker gave him in ℥ss. of whiskey, Bibron's antidote gtt. xx.; in thirty minutes he repeated the same dose; immediately after which the patient vomited a quantity of white frothy fluid. In an hour, the same dose was repeated, immediately after which a quarter of a grain of sulphate of morphia was given. The patient continued delirious about an hour, when he went to sleep, waking in about two hours. The delirium had subsided, and the pulse was at 110. The breathing also was somewhat relieved. Dr. Walker left him about sunrise. He ordered sulph. quinine, gr. iii., camphor powder, gr. iii., to be given every six hours, and the sloughing and abraded parts to be dressed with charcoal poultices, and all the swollen parts to be bathed frequently in salt water. As Dr. Walker had to leave the country for a few days, he procured the services of Dr. McKay for the case, who continued the treatment for two days, when finding the patient with a furred tongue, he gave him a mercurial, and substituted iodide of potassium for the quinine and camphor, a stimulating ointment for the poultices to the sloughing parts, and Lugol's solution of iodine in place of the salt bath to the swollen parts.

Dr. Walker resumed attendance on the case on the 7th July. He continued the iodide of potassium, substituted the charcoal poultices with the addition of a solution of creosote for the stimulating ointment, and suspended Lugol's solution. On the 10th July, the abrasions on the arm had healed, the sloughing of the hand had extended all over the third and fourth fingers, over the second, third, and fourth metacarpal and unciform bone on the dorsal aspect of the hand, and all over the palmar surface of the hand, extending back to the radio-carpal articulation. In connexion with Dr. McKay, amputation of the forearm was determined upon, which they did, performing the double-flap operation in the lower third. On the 18th day after the operation, the patient was riding over the country with the stump entirely healed. Upon examining the wrist-joint after the amputation, they found about a drachm of pus in the synovial sac. Dr. Walker thinks this recovery cannot be attributed to any other agent than the bromine compound of Prof. Bibron.—*American Journal of Medical Sciences*, Oct., 1858.

Experiments with Bibron's Antidote by Dr. Sabal.—Dr. Sabal, of Riceboro', Liberty Co., Ga., reports six experiments with Professor Bibron's antidote. In his first experiment, a dog was bitten at the sacro-lumbar articulation. Four minutes after the wound, he staggered and fell; twelve minutes afterwards, he vomited blood freely, when Dr. Sabal administered a dose of Bibron's antidote. This seemed to revive him, but although he made many attempts to rise, he failed in doing so. Fifteen minutes after the first dose was given, Dr. Sabal administered a second dose. The dog again revived, but could not move. In twelve minutes six drops of the mixture were given to him. He died in taking it. This dog died in forty-three minutes from the time he was bitten; there was no swelling; his eyes became of a green colour several minutes before his death; the tongue contracted to one half the normal size, and became of a dark purple colour. In the second experiment, the dog was fine, fat, and hearty. He was bitten three times in the flank. Swelling did not commence for fifteen minutes, when he whined, and seemed much distressed. Ten drops of the bromine mixture were administered, and he seemed much better. He was sick fifteen minutes afterwards, and frothed at the mouth. A second dose was given in the same proportions as the first. He revived immediately, and has been well ever since, with the exception of a swollen leg, which disappeared in twenty-four hours. In the third experiment, the dog was very severely bitten on the leg in three places. He vomited the contents of his stomach; immediately subsequently he vomited frothy blood and bled at the nose. Twenty minutes after the wounds were made the antidote was given, and repeated four times at intervals of ten minutes. This dog was sick for six hours, when a fifth dose was given. He made a complete recovery, but the leg remained swollen for two days. In the fourth experiment the dog was thin and old. The medicine was given as described in the preceding cases, but he only lived thirty hours, and unlike any others that died, he was enormously swollen. The fifth experiment was made on a young dog, which was bitten in the neck. This dog lived five hours under the administration of the medicine, but Dr. Sabal having been called away, the dog was neglected, and died apparently from suffocation. In the sixth experiment, the dog was bitten in the flank, received all attention, but died in an hour and a half.—*American Journal of Medical Sciences*, Oct., 1858.

SUMMARY.—An instance of wholesale poisoning by lozenges charged with arsenic, occurred at Bradford on the last days of October and the 1st of November, 1858. Mr. Neale, a confectioner, sent to Mr. Hodgson, a druggist, at Shipley, for what is technically called "daff," but which is really sulphate of lime. The "daff" this time supplied, was, by a careless accident, arsenious acid; lozenges were made with it, and Hardaker, a stallman, sold them. There were more than twenty deaths as the result, while a hundred were made sufferers. Excepting one fact, of the mode in which the accident occurred, this case has but little interest now to the medical jurist; since no medical scientific record has been made which can be considered as throwing any light on toxicological studies.

Poisoning by Hyoscyamus—Scarlatinal Eruption.—The 'Montreal Chronicle' records a case by Dr. R. Craik, of Montreal, in which a child was poisoned by swallowing a portion of some plants of hyoscyamus niger, growing in the yard of the Montreal General Hospital. The plant eaten was not quite ripe, and nearly an ounce of capsule and seeds had been swallowed. The symptoms induced were flushing of countenance, restless and violent tossing, momentary listening to imaginary sounds, and eager clutching at visionary phantasms; the eye was brilliant, the pupil widely dilated, the pulse hurried, and the respiration laboured. The most curious symptom was this: the whole surface of the skin was of scarlet redness, exactly as occurs from the rash of scarlet fever; the mucous membrane partook, to some extent, of the same appearance as in scarlatina. Emetics of sulphate of zinc and mustard removed the hyoscyamus seeds and capsules from the stomach, and recovery took place; but the acute symptoms did not entirely cease for twenty-four hours; the pupil remained dilated for several days. On the fourth day varicellar eruption appeared, followed by extensive desquamation of the skin.

Death from eating Holly Berries.—The 'Gardener's Chronicle' and 'Pharmaceutical Journal,' for March, 1859, contain record of the following case: A little boy, named Fuller, at Northiam, was allowed by his parents to eat from twenty to thirty of the red berries from the holly. He was ill and vomited through the night. In the morning Mr. Harris was called in, but was summoned too late for his attendance to be of any avail. The jury returned a verdict that death occurred from some "vegetable irritant." The dangerous properties of red holly berries have been pointed out by some botanical writers, as De Candolle and Lindley. The latter author states, "the berries are purgative and emetic, six or eight will occasion violent vomiting." A fatal case of poisoning from them does not, however, seem to have been before noted, and Christison, Orfila, and Pereira are silent on this poison. It is well, therefore, to make known, that the berries with which our churches and houses are decorated at Christmas, are to be ranked amongst the vegetable irritant poisons.

Detection of Phosphorus.—The 'American Medical Monthly,' for September, 1858, contains a note on a new mode of detecting phosphorus, discovered by Professor E. Delarue. In examining a body supposed to have been poisoned by phosphorus, the ordinary tests failed. It then occurred to him to test for some metallic substance. He therefore submitted a very small portion of the stomach to combustion by means of sulphuric acid, when suddenly, to his surprise, the carbonaceous mass was covered with brilliant sparks, which burned at the side of the capsule with all the characteristics shown by phosphorus when burning in air. In another case, the intestines of a child, four years old, were examined for phosphorus, but without success. Professor Delarue, for the sake of experiment, mixed with a portion of the stomach the material derived from twenty matches, and submitted the structure to combustion with sulphuric acid, when all the phenomena of phosphorus combustion were again markedly developed.

In the 'Glasgow Medical Journal,' for October, Professor Easton continues his very able paper on the Elimination, Catalysis, and Counteraction of Poisons. In the 'American Journal of the Medical Sciences,' for October, 1858, Dr. Buel gives instances of two men who were poisoned by Seidlitz powders. One man died within a few minutes with symptoms of the tetanic type; the other had similar symptoms, but recovered under the use of emetics. The evidence at first was in favour of the view, that the powders were charged with strychnine; the remaining powders from the same parcel were submitted to a strict chemical analysis by the Professor of Chemistry in the Brown University, but the inquiry threw no light on the subject.

Since our last report, four deaths at least have occurred from chloroform. Three of these are recorded in full. One occurred at the Opthalmic Hospital, Moorfields, in a child eight years of age; the others in Paris, in subjects young and apparently healthy. In all the cases the chloroform was given on a handkerchief or from lint.

II. Miscellanea.

Extra-Uterine Pregnancy and Death.—The subject of this report was a free negress, who had been twice married, had borne two children at the full period, and had suffered two abortions. The periods of pregnancy at which the abortions had occurred, were not ascertained. The history of the last attack commenced on the 9th of December, 1857. About nine in the morning the patient felt sudden abdominal pain, and soon after became so faint that she would have fallen, but for the support of her husband. She was laid upon a bed and partly recovered from her fainting condition. The pain, however, continued and increased in intensity. Dr. Metcalfe, who reports the case, found the woman with all the symptoms of hæmorrhage. She had menstruated five or six weeks previous to this time. He ordered stimulants and anodynes, under which she seemed at first to rally; but on his return, after a short absence, she was hopelessly sinking; she died about fourteen hours after the sudden expression of pain in the morning. On the following morning, on making a post-mortem examination in this interesting case, which in this country would have given rise to strict judicial inquiry, Dr. Metcalfe found that hæmorrhage had taken place into the abdominal cavity, and that the cause of the hæmorrhage was a rupture in the right Fallopian tube, near its connexion with the uterus. The rupture was about one-fourth of an inch in extent, and on increasing the opening a cavity was found as large as a chesnut, involving both the tube and proper tissue of the uterus. In this cavity the mouths of several small vessels were found to terminate, and near the opening was found a displaced ovum. The uterus was lined with decidua.—*Semi-Monthly Medical News*, Jan. 1st, 1859, Louisville, Kentucky.

Detection of Blood and Blood-stains.—Dr. Heller, in an elaborate paper on Hæmatin and its Detection, supplies some notes of a test for this colouring matter, from which we make one or two short extracts. After describing various colouring substances which may give a red or inky coloured water, he explains that in all cases the water must be tested for albumen, the absence of which excludes hæmatin. If albumen be present and be separated by boiling, the coagulum containing the hæmatin is not white, but of a red or rusty-brown colour, and the whole red colour of the water is lost, and the clear fluid becomes first of a light and then of a darker yellow tone. The collected dried albumen coagulum will assume, if a very little hæmatin be present, a brownish-black colour, whilst, if no albumen be present, the coagulum will be yellow.

To obtain a reaction of hæmatin, Heller recommends the following. The coloured water, whether it contains much or little albumen, is boiled in an open eprouvette, and solution of caustic potassa is added. In the presence of hæmatin, as the potash solution is added, a faint bottle-green colour appears. The solution is warmed for a short time and shaken, upon which the earthy phosphates subside by reason of the ammonia which is being produced. The hæmatin escapes, and after a time appears partly above, partly at the lower part of the vessel, at one time of brownish-red, again of a beautiful blood-red. The eprouvette is now allowed to stand aside for a time quietly, so that the earthy phosphates which have been coloured of a blood-red by the hæmatin settle at the bottom. Under the microscope this sediment presents yellow amorphous masses. The red earthy phosphates, after two days, again become discoloured by the further action of the potassa. If the solution contains very little earthy phosphate, or if it consists of any fluid, such as vomited matter, which has to be tested for hæmatin, then an equal volume of pure water must be added to the fluid to be tested, in order to obtain a sufficient quantity of earthy phosphate.

This reaction is specially recommendable in cases where the blood-colouring stuff has undergone partial decomposition and lost its red character, or where it is hidden, as by biliphein; or where fluids exist which from their exterior appearance would not be suspected to contain blood, as in the fluid of an abscess and in the contents of cysts. Dr. Heller gives special precautions on the detection of blood-stains, pointing out that success or failure depends principally on four points—viz., the quantity of blood found, the age of the blood-stain, the place in which the corpora delicti have been preserved, and lastly, the means which may have been used for removing traces of blood. His hæmatin test given above affords a further contribution to the analytical method of ascertaining the fact of blood.—*Zeitschrift der Kaiserl. Königl. Gesellschaft der Aerzte zu Wien*, November 22nd, 1858.

Detection of Blood-stains.—Dr. Andrew Fleming, of Pittsburg, Pa., has also a paper of value on blood-stains. The paper is rather useful as a comprehensive review of the whole subject, than as offering original material. He considers the "chemical characters of blood-stains" and the "microscopical examination." His note on the method of procuring blood-crystals may be of use. The method of obtaining crystals is to put a drop of blood on a glass slide, add a small quantity of alcohol, water, or ether, and then allow evaporation to take place slightly, covering the blood with a glass slip, and first interposing a hair between the glasses, to afford room for the crystallization; carmine-red crystals of different sizes will appear in from half an hour to a few hours or several days, according to the kind of blood and the situation in which it has been placed. The crystals are more rapidly obtained by exposure to sunlight, and more easily from defibrinated blood. The blood from which it is desired to make crystals does not require to be taken immediately from the vessels, but they are with less trouble procured when it has stood from twelve to twenty-four hours, which is a great advantage in legal cases. From blood that has been dried in clots for several months, the hæmatin crystals may sometimes be obtained. Dr. Fleming is of opinion that in the microscopy of blood corpuscles great difficulties arise in the recognition of the corpuscles special to the blood of special animals. [We can ourselves fully attest to these difficulties, and add that they are insurmountable.]—*American Journal of Medical Sciences*, January, 1859.

III. Hygiene.

Life and Labour.—The 'Sanitary Review' for January has an article on the influence of various common occupations on health and life. The effects of sand-paper making are illustrated as giving rise in the young to a modified phthisis, which is sometimes rapidly fatal. Walking-stick making and hemp and flax dressing are described as exciting bronchitis and bronchorrhœa. The Neapolitan hemp has also the peculiar property of producing a spasmodic paroxysmal attack like that produced by drying hay and by ipecacuan. Trimming manufacturers' work leads to bronchial mischief. Fur dyeing, by exposing the artisans to the fumes of nitrous acid and to the dust arising from dried sulphate of copper, specially leads to a number of serious evils, affecting the teeth, the digestion, and worst of all, the organs of respiration. Cigar and snuff-making, in the first instance, give rise to the peculiar toxical effects of tobacco, and afterwards to chest disorder, ending in chronic bronchial flux and inanition. The writer of this paper suggests that a Parliamentary committee of inquiry should be organized to inquire into the subject of occupations and health. He concludes as follows:

"When we look at diseases as a whole, we stand amazed at the varieties of type which they assume. When we classify them into groups according to their causes, we stand equally amazed at finding to how very few groups all diseases may be reduced. We find all the disease causes out of the body and clothed in externals. Resolving the causes, there stand out some dozen poisons of communicable and reproductive power, improper dietary, variations of atmosphere, and occupations. Of all these, the last stands most invitingly for inquiry. The inquiry is of a kind to which the most rigid rules are applicable. Its results might be demonstrations, its suggested remedies simple certainties."—*Sanitary Review*, January, 1859.

Mortality of England in 1856.—Of 390,506 deaths in the year, the causes were not specified at all in 4666 cases, and 3474 are simply tabulated as sudden deaths, all inquiries having failed to elicit further definite information; 94,407 of the whole number of deaths were those of infants under one year old.

The causes of death are arranged in a few classes with numerous subdivisions. 78,047 of the deaths are classified as zymotic diseases: typhus (15,398), scarlatina (14,160), and diarrhœa (13,815), proving fatal in 43,373 instances, or considerably more than one-half of the whole number due to this class. Hooping-cough, measles, and croup stand next in order. Small-pox, formerly so fatal, was the cause of death in 2277 cases.

Of the class of diseases denominated "constitutional," 82,856 persons died. Phthisis (consumption) stands sadly pre-eminent in this list, its victims numbering 48,950 persons, by far the greater proportion of whom were young women. "How many of the thousands of deaths are to be ascribed severally to the fatal stays and to the in-door life of women, &c., it is not easy to calculate. Air is the pabulum of life, and the effects of a tight cord round the neck, and of tight lacing round the waist, differ only in degree in the time of their manifestation and in some of their symptoms; for the strangulations are both fatal. To wear tight-laced stays is, in many cases, to wither, to waste, and to die, and is perhaps the natural chastisement of the folly which inflicts this Chinese deformity, natural only to wasps and other insects, on the human figure." The tubercular diseases carried off in all 63,832 persons.

The "local diseases," as inflammations, the allied pathological phenomena or their results, and functional diseases of particular organs, proved fatal to 149,911 persons. 50,535 of these died of diseases of the brain and nervous system, including also 23,946 deaths by convulsions. 13,672 deaths were referred to diseases of the organs of circulation. Diseases of the respiratory organs proved fatal to 52,908 persons, 21,528 of whom died from bronchitis and 22,658 by pneumonia. These diseases, and all others of the class, were less fatal than in the previous year. 22,620 persons died from diseases of the digestive organs. The other causes of death under this heading are numerous, but the cases are few in each.—*Condensed from the Registrar-General's Annual Report for* 1856.

Effects of Sewer Emanations.—Dr. S. J. Radcliff, of Baltimore, reports a case in which a man was exposed for two hours to the gases arising from a privy sink. The man, a strong athletic German employed in the night business, was engaged with two others in cleansing a sink which was eight feet deep. About 950 gallons of the fluid had been removed, when the man attempted to descend into the sink to complete the work, the contents being then only two feet and a half deep. He was overcome by the noxious gases, and precipitated to the bottom. Every means was used to extricate him, but for some time to no purpose. He was kept alive by continued douches of cold water; and finally, after a lapse of two hours, finding that he was too exhausted to grasp a rope, a ladder of sufficient length was procured, and at the risk of their own lives, several men descended, and succeeded in rescuing him. The symptoms exhibited by the man on coming to the air were those of partial asphyxia, with coldness of the body and shivering. The fresh air revived him, and reaction soon set in. After a time he complained only of nausea, and thought he could taste and smell nothing but the disgusting effluvia. In two days, without any peculiar symptom, and with no indications whatever of typhoid, the man was fairly convalescent. He made a perfect recovery.—*American Journal of Medical Sciences*, Oct. 1858.

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

By Edward H. Sieveking, M.D.

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I. *On "Ataxie Locomotrice Progressive."* By Dr. Duchenne (de Boulogne). (Archives Générales de Médecine, Décembre, 1858.

Under the above title, for which we are as yet unable to suggest a suitable English version, Dr. Duchenne describes what he regards as a new disease. He states that for six years past his researches have induced him to believe in its existence, and that once having satisfied himself of the fact, he had no difficulty in discovering many instances of the disorder. Without making a minute search, he states that in a brief space of time he collected twenty cases. The characteristic features are: progressive abolition of the co-ordination of movements and apparent paralysis, contrasting with the persistent integrity of the muscular forces.

The loss of power of co-ordinating the movements which is manifested in the lower extremities, consists in a difficulty of maintaining an erect position without trembling or without support, or of performing certain movements in progression, such as lateral or gyratory movements. The patients commonly at such time feel giddy, and as if about to lose their balance, and sometimes think that their lower extremities have lost power; the tactile sensibility of the sole of the foot becomes blunted or otherwise altered. They soon seem to be walking on soft substances when treading upon a pavement, or the ground appears to be elastic and to jerk them up. Subsequently they are unable to walk without throwing the legs forcibly forwards and striking the ground violently with the heel. These movements are at times so violent and sudden, that the body is shaken at every step, and that the patient loses the balance; this irregularity increases to such an extent as to render standing and walking almost impossible. It is then necessary to support the individual on both sides, and in attempting to make a few steps, their limbs are agitated in the most violent manner, without apparent object; the strength is speedily exhausted by these efforts, and the patient begs to be led back to his arm-chair. In this condition the patients pass their life, sitting or lying down. At the same time, with all these symptoms of paralysis, there is great force in individual movements, as shown by a dynamometer invented by Dr. Duchenne. The inquiry into the state of these movements must be made while the patient is sitting or standing; but it is more convenient to examine the lower extremities in the horizontal posture. The author first causes the patient to perform those movements which he proposes to study, then when the muscles which produce them are in a state of contraction and are maintained so by the patient, he acts upon the extremity of the member in question in the opposite sense to the movement performed, so as to support that part of the limb which serves as the attachment of the contracted muscles. For instance, in measuring the force of the extensors of the leg on the thigh, the patient lying on the opposite side, Dr. Duchenne has the thigh held very firmly at its inferior extremity, and whilst the patient tries to extend his leg forcibly, the Doctor acts upon it in the opposite direction, and applying the force at the level of the ankles, until he has produced flexion of the limbs. This the patient can do without fatigue; the dynamometer determines the force of the movement of extension employed by the individual experimented upon. A drawing of the instrument is annexed, but the exact method of its action is not explained.

II. *On Acute Inflammation of the Parotid Gland.* By Rud. Virchow. (Annalen des Charité Krankenhauses Achter Jahrgang, 3 Heft.)

In this paper Professor Virchow combats the doctrine that the seat of the morbid action in parotitis is in the inter-glandular cellular tissue, and not in the gland-tissue itself. According to his experience it is the latter which is affected. He has never been able to make a cadaveric examination in a case of primary idiopathic parotitis, but has repeatedly investigated the secondary and metastatic forms in various stages in the dead body. The first change which the author has observed is a considerable hyperæmia of the gland, causing it and the inter-glandular cellular tissue to become more moist and swollen. The red acini become more and more isolated from the surrounding yellow tissue, and, on section, resemble groups of red grains. The secretion accumulates in the follicles and ducts; it soon becomes purulent, and pus corpuscles are early detected mixed up with the large salivary corpuscles. If the morbid process continues, a sort of fusion takes place, and the follicles appear directly converted into small abscesses. The inter-glandular areolar tissue next suppurates, and diffuse phlegmonous inflammation may ensue. In this case, a portion of the gland tissue sloughs away.

Professor Virchow suggests that this description may not apply to ordinary mumps; as the metastasis which at times takes place to the testicles militates against the assumption of the gland being so seriously affected as just described, whereas a mere serous exudation might be readily absorbed and removed. He is himself of opinion that there is no essential anatomical difference between the various forms. He agrees, however with Bamberger, in establishing three nosological varieties:—1. The primary simple catarrh of the glands commonly occurs epidemically without tendency to suppuration and ulceration (cynanche parotidea, mumps). 2. The secondary purulent catarrh (blennorrhœa parotidea), readily leading to abscesses, commonly associated with catarrh of the oral cavity, alphthæ, and the like; not unfrequently with affection of the labyrinth. 3. Specific, ichorrhæmic catarrh (almost invariably leading to gangrene (verjauchung), and commonly associated with metastases. The author is of opinion that the disease results chiefly from circumstances that induce catarrhal and rheumatic influences, and an analysis of his cases shows that the majority of cases occurs in the months of March, April, and May. Some other remarks are added, for which the reader is referred to the original.

III. *On Jerking Respiration (respiration saccadée.)* By Dr. A. Bourgade. (Archives Générales, November, 1858.)

Dr. Bourgade is of opinion, and we think justly, that the profession have not paid sufficient attention to interrupted or jerking respiration, as one of the earliest symptoms of pulmonary tubercle. He details nine cases, and states that he has observed others, in which jerking respiration heard at the apex of one lung was the first auscultatory phenomenon indicating the deposition of tubercle subsequently proved by more palpable symptoms, and in part confirmed by post-mortem examination. In discussing the rationale of its production, he quotes one post-mortem examination, which absolutely disproves the views of Messrs. Barth and Roger on this point. They hold that it is due to the presence of the adhesions which are so frequently met with at the apex of the lungs in phthisical subjects. In the case in question there were no adhesions at the apex or about the middle of the lung in which the jerking respiration had been heard. It is to be remembered that the symptom is most commonly met with at a time when no evidence exists of preceding or accompanying pleurisy; the character of the sound and the structure of the parts in which it is found has always seemed to us to justify our attributing it to a partial constriction of the smaller bronchi owing to adjoining tubercles pressing upon them, the effort of inspiration causing the successive opening of the obstructed passages. We have met with cases in which spasm appeared to be the cause of the constriction; but we hold with Dr. Bourgade that, in the great majority of cases, jerking respiration is a sign of tubercular deposit.

We would endorse the following summary of observations made by Dr. Bourgade. Respiration is jerking when the respiratory murmur presents three or four brief intervals, and the ausculting ear experiences the sensation of a certain difficulty in the expansion of the pulmonary tissue from the prolonged expiratory murmur. The respiratory murmur otherwise preserves its normal timbre, and the symptom is chiefly observed during inspiration; it occurs, but less frequently, during expiration. This change in the respiratory murmur does not persist beyond a limited period; a prolonged expiratory murmur follows, or, more rarely, progressive diminution of the respiratory sound. This fact probably explains the circumstance of the symptom having been overlooked by many hospital physicians, who, seeing phthisis chiefly in its more advanced forms, do not meet with jerking respiration here as frequently as in private or extra-hospital practice. Jerking respiration is commonly limited to the apices of the lungs, which we should explain by the greater resistance of the parietes of these parts favouring the pressure of tubercle upon the adjoining bronchial tubes. When the patient is made to breathe more fully, the symptom is not rendered more palpable, but commonly disappears altogether. Jerking respiration is not always continuous, but at times presents regular intermissions; it may occur at every second, third, or fourth inspiration, sometimes even at greater intervals. It is chiefly at its first appearance that it presents this character, but when well marked it is commonly continuous; like other auscultatory phenomena, it may vary in strength and precision from day to day.

In concluding his paper, the author, though attributing much more importance to jerking respiration as an early sign of pulmonary tubercle, states that he does not regard it as an absolute evidence of the deposit having taken place; for this purpose he justly requires the presence of collateral, general, or local symptoms.

IV. *On the Treatment of Croup.* By M. Malgaigne. (L'Union Médicale, Dec. 9th, 1859.)

In a letter addressed to the Academy of Medicine, *à propos* of a long discussion on the treatment of croup, M. Malgaigne complains of the erroneous manner in which his doctrines as to the employment of tracheotomy in this disease have been represented, and sums up the views he entertains thus: he regards the operation as a sad but imperative duty, justified only by the absence of all other chance for the patient, and he distinctly opposes the doctrine of M. Trousseau, who inculcates that the operation should be had recourse to as soon as false membranes make their appearance in the larynx. M. Malgaigne considers that the time for the surgeon to interfere is when the physician states himself to be unable to do anything more.

V. *On Perityphlitis: a Clinical Lecture.* By Professor Oppolzer. (Allg. Wien. Med. Ztg., 20, 21, 1858, and Schmidt's Jahrbücher, 1858, No. 9.)

The author observes, with reference to the diagnosis of perityphlitis, that the site of the tumefaction is either under the iliac fascia, immediately upon the subjacent muscles, nerves, and vessels, or above them in that portion of the cellular tissue of the iliac fossa which is in immediate contact with the peritonæum and cæcum. In the former case the swelling is slight and causes but a trifling projection of the anterior abdominal parietes; but the symptoms are much more urgent owing to the pressure of the rigid fascia, and the pain on moving the right foot intense from the iliac muscle being involved. In the second case the tumour is larger, more distinctly visible and tangible; the symptoms are less urgent. Perityphlitis may be confounded with partial peritonitis, perforation of the vermiform process, accumulation of fæces (this we have found associated with perityphlitis so as to act as an exciting cause), congestive abscesses, new growths in the neighbourhood of the cæcum. The history and progress of the disease must in each case determine the diagnosis, which however is not always easy. The second variety of perityphlitis is the most favourable of the two, and when caused by catarrhal influences, generally ends in resolution; the first form generally terminates in suppuration, and the pus makes its exit in various directions. The treatment is to be directed to diminishing the pain and arresting peristaltic action; hence leeches, warm fomentations, and opium are indicated, followed by alteratives and warm baths after the inflammation has subsided. In the case of suppuration an early opening should be effected.

VI. *Notes on Pigment in the Urine.* By A. J. Paine, M.D., Bengal Medical Service. (Indian Annals of Medical Science, July, 1858.)

Dr. Paine has made it a rule to examine the urine of his hospital patients with reference to the amount of pigmentary matter contained in it, and he arrives at the conclusion that it is invariably a test of the presence or absence of hepatic disease; the intensity of the colour produced by heat and the slow addition of nitric acid, being in the exact ratio of the amount of disease in the liver. He has found the test of especial advantage in determining the diagnosis of hepatic from other forms of dyspepsia. Dr. Paine carefully distinguishes between the iridescence produced by nitric acid in urines which contain bile, and the more or less deep-red colour caused by the same acid after heating the liquid to be tested; it is the latter to which the present paper is devoted. He establishes seven shades, varying from a pale sherry colour, the reaction of pale healthy urine with heat and nitric acid, to a colour which is so deep as not to allow the transmission of light. Five cases, with autopsies, are given; other cases in which recovery resulted are also recorded by the author, who sums up the evidence, proving that excess of pigment in the urine is proof of liver disorder, thus—"In all the cases of organic disease of the organ, that have afforded opportunity of post-mortem inspection, the connexion has been established; in a wide range of cases, where experience teaches us that functional disorder prevails, pigment is found; in others, where structural disease is known to be present during life, it is found; where it is assumed to be an indication, even in opposition to other evidence, and practice is based upon it, such practice is successful; pigment does not occur under any circumstances which preclude hepatic affection; and lastly, in functional disorder, the quantity of pigment varies from time to time, in the same manner as the state of the function may naturally be supposed to vary."

VII. *The Diagnosis of Melanotic Cancer by the Urine.* By Dr. Eiselt. (Vierteljahrsschrift für die practische Heilkunde, xv. Jahrgang, 1858, Dritter Band.

Dr. Eiselt reports 8 cases which appear to show that the urine offers a means of diagnosis in melanotic cancer. The first is that of a man, aged sixty, who, in 1856, came under observation with symptoms of hepatic cancer, and with cancer of the left eye. There was no icterus, but the urine exhibited a remarkable peculiarity; when passed it was perfectly clear, but on standing it became as dark as porter without losing its transparency. A portion was drawn by the catheter; it was found to contain copious uric acid, and a normal quantity of urea; when exposed to air and light it became dark in a few hours, but concentrated nitric acid caused the change instantly; other oxidising substances, especially chromic acid, produced the same effect, and the black matter was regarded as melanin, which induced the opinion that the cancer was melanotic, a diagnosis confirmed by the autopsy. A year later a man, aged sixty-eight, was admitted into the Prague Hospital with cutaneous melanotic cancer. The urine at first exhibited the peculiarity shown in the last case feebly, but as the disease spread to the internal organs, and especially as the liver became affected, the reaction of the urine became as characteristic as in the former case. A third case occurred in May, 1858, in which there was hepatic cancer, and cancer of one eye. The urine again induced the attending physician to diagnose melanotic cancer; some urine of May 8th was closed hermetically and kept in the dark; some that was passed on May 9th was also closed hermetically and placed in the shade. On May 25th Dr. Eiselt exhibited both specimens to the College of Physicians of Prague; the urine of May 8th was slightly turbid, pale yellow, and had deposited phosphates; the urine of May 9th was black with reflected, and dark-brown with transmitted light. On opening the first specimen, nitric acid and chromic acid at once induced the black colour. The autopsy confirmed the diagnosis of melanotic cancer, for which there had been no other indication.

VIII. *On Hæmorrhagic Measles.* By Dr. Otto Veit. (Archiv. für Pathologische Anatomie und Physiologie, Band xiv., Hefte 1 and 2.

The prevailing view that the hæmorrhagic or petechial form of measles is of a malignant or septic character indicating great danger, is combated by Dr. Veit upon the strength of the experience which he has derived from the various epidemics that have occurred in Berlin, from 1847 to 1857. After quoting various authors from Huxham downwards, of whom Rilliet and Barthez alone coincide with the views advocated by Dr. Veit, he details his own observations. He met with the petechial form in 11 out of 160 cases; and although 9 of them lived in needy circumstances, they all passed through the disease without any peculiar disturbance or ill effects. The author does not deny *in toto* the occurrence of a septic form of measles, but is unwilling to admit that the petechiæ in the cases observed by himself were due to decomposition of the blood, but that they must be regarded as a proof of the greater intensity of the *physiological* process, in the same way as the catarrhal affection of the respiratory organs may be converted into croup or bronchitis. In the hæmorrhagic form the eruption, after the usual prodromata of fever and catarrh, on the second, third, or fourth day, instead of becoming paler, suddenly assumes a dark-red colour. These spots become still darker on the ensuing days, and even black; they are round or angular, but have a sharply-defined margin; they vary in size from a flea-bite to that of a pea or bean, and more. They do not disappear on pressure, but behave exactly like extravasations. These spots retain their intensity of colour for a day or two, and then pass through the various changes of colour observed in other extravasations, becoming purple, brown, and yellow before they disappear altogether. The desquamation of the epidermis is more marked over these spots than elsewhere. Dr. Veit has not met with cases in each epidemic of any intensity; sex appears to exert some influence on the occurrence of the hæmorrhagic form, seven having been males and four females; while of the 160 cases of measles, 80 were males and 78 females, the sex of two children not having been noted. All the epidemics observed by the author had a benignant character, only 3 of his 160 patients having died.

IX. *Anatomical and Clinical Researches on the Dropsy consequent upon Typhoid Fever.* By E. Leudet, Titular Professor of Clinical Medicine at Rouen, &c. (Archives Générales de Médecine, October, 1858.)

During ten years of study in the hospitals of Paris, Dr. Leudet met with no cases of dropsy consequent upon typhoid fever; but during the four years that he has been chief medical officer at the Rouen Hospital, no less than seven instances have occurred to him, where the inferior extremities and the entire surface of the body became œdematous about the second or third week of the fever. All but one, who died of peritonitis, recovered; and none had albuminous urine, although this symptom was very frequently observed in other typhoid fever patients. The author regards œdema as a matter of no great consequence, except that it retards the recovery of the patient; several of Dr. Leudet's patients were more than two months before they recovered sufficiently to return to work, one even returned to the hospital on account of his debility. Dr. Leudet considers his treatment to have had no influence in the production of the œdema, as he never employed antiphlogistic remedies and abstained from repeated purgations and emetics, his treatment of the fever having been expectative and tonic rather than *adynamising*. His patients were all fed with *bouillon*, even from the commencement of the disease, and on the supervention of the œdema they received a more nutritious diet, with quinine, wine, or chalybeates. Dr. Leudet attributes the œdema to the feeble constitution of the inhabitants of Rouen, and of the same nature as the typhoid fever, which is more frequently complicated with stomatitis and pleurisy than elsewhere; he also states that the pyrexiæ and phlegmasiæ of his townsmen generally present an adynamic character.

X. *On Intermittent Fever; the Result of the Observations made during several Epidemics.* By Dr. HEIDENHAIN in Marienwerder. (Archiv für Pathologische Anatomie, Band xiv., Hefte 5 and 6.)

This paper is one of interest, as bearing upon the question of the change of type of disease generally, as well as on account of the special matter to which the author devotes his attention. Marienwerder, a town of western Prussia, not far from the delta of the Vistula, is the seat of intermittent fever and its congeners; and Dr. Heidenhain, having been in practice in that town and its vicinity for twenty-eight years, gives us the results of his experience during that period. He commenced his practice in 1831, shortly after cholera had claimed its first victims in the district surrounding Marienwerder. This town and the immediate vicinity had been spared, but instead of cholera there was an epidemic of gastric or bilious febrile conditions, with a remittent, or, more frequently, intermittent type. Gastric, bilious, and intermittent fevers almost disappeared when the cholera actually invaded Marienwerder; but the cholera itself assumed an intermittent character of the tertian type in some cases. On the cessation of cholera in 1831, intermittents ceased entirely in Marienwerder, and did not occur in the epidemic form until 1849. Even in the August of 1844, when the valley at the foot of the heights upon which Marienweder is built, was flooded by the Vistula, and after the subsidence of the waters the atmosphere was poisoned with effluvia of decomposing vegetable matter, intermittents did not make their appearance. During these eighteen years febrile intermittents only occurred sporadically; the paroxysm was not ushered in by a severe rigor, but merely by a passing sense of cold, and the other symptoms of fever were not strongly marked. These affections were cured by large doses of quinine. In the place of genuine intermittents, intermittent neuralgic affections made their frequent appearance; the nerves affected were almost exclusively the first and second division of the fifth pair; the patients were affected with severe hemicrania for three or more hours; the forehead, the orbital region, the cheek, half the nose and lip were the seat of the affection; the face was pale, at times slightly swollen, the eye dull, the pulse small, no trace of fever, the hands cool; the type was quotidian or tertian. The paroxysms commonly occurred in the morning, without premonitory symptoms, and ceased almost as suddenly. Twice the author observed similar neuralgic affections in the nape of the neck, and once in the right tibial nerve; in the last instance the subject affected was a multipara, in whom the neuralgia took the place of the after pains immediately after confinement; the neuralgia lasted eight hours, then put on the tertian type. In addition to these neuralgic intermittents, the doctor observed a convulsive intermittent disease in children. It occurred between the years of two and nine, and the affection was at times difficult to distinguish from an apoplectic condition, unless reference was had to the intermittent type. Dr. Heidenhain's description is briefly this:—The child loses consciousness, the eyes are closed, the pupil enlarged and sluggish, the head hot, the facial muscles are distorted by convulsive movements, the lips quiver or are drawn on one side, permitting the passage of frothy saliva, at times tinged with blood; it is almost impossible to effect deglutition. The extremities are violently thrown about; breathing is short and intermittent; there are suffocative attacks; profuse perspiration breaks out over the body. After four, six, or eight hours the symptoms abate, and the child appears to wake up exhausted, but otherwise in comparative health.

In the autumn of 1849, with the cholera, all the forms of intermittent fever returned with greater frequency; but in 1850 and 1851 they increased still more; and in 1852, the bad year of the cholera, they were at their maximum, but did not, as in 1831, disappear with the cholera, but remained the prominent form of disease until the autumn of 1856, since which time they have diminished. The paroxysms were not characterized by severe rigors, and the cold stage was uniformly short, compared with the length of the hot and sweating stages. The affection attacked all ranks and ages, and however favourable the circumstances and healthy the dwellings of the individuals. A characteristic feature during this period was the severe pain which, in the fever, often affected the lumbar and sacral regions, and the tenderness of the last cervical and first dorsal vertebra. This "spinal irritation" disappeared on the curative effect of the treatment being manifested.

During the period last spoken of, in addition to the neuralgic affection, neuroses of the motor nerves have made their appearance. Intermittent affections of the vaso-motor system of nerves have also been observed, such as intermittent ophthalmia with a tertian type; the pain was unusually severe in these cases, and out of proportion to the severity of the inflammatory symptoms.

Enlargement of the spleen and anasarca are the chief sequelæ which the author has met with. Of the latter he says, that it used, previous to 1854, never to be associated with albuminuria, but that since that time the majority of cases exhibited albuminous urine; these cases commonly proved fatal.

With regard to the treatment of all these various conditions, Dr. Heidenhain recommends that no energetic treatment be adopted during the paroxysms, not even in the convulsive intermittent affection of children. Quinine is his sheet anchor, given in large doses during the free intervals; he has found eight grains generally suffice for an adult, given about the middle of the free interval, or immediately after a paroxysm.

XI. 1. *On Œstrus or Affectio Bovina.* By G. W. Spence, M.D., Lerwick, Shetland. (Edinburgh Med. Journal, Nov. 1858.)

2. *On the Occasional Occurrence of Bots in the Human Subject.* By J. Matthews Duncan, M.D. (Edinburgh Veterinary Review, Jan., 1859.)

Alexander von Humboldt so frequently found the natives of South America affected with a number of the genus Œstrus, an insect of the order Diptera, that he established a distinct species, under the name of œstrus humanus. Kirby and Spence, Kirchenmeister and others, dispute the propriety of this distinction, and Dr. Spence and Dr. Duncan bring forward fresh arguments and facts as well as cases that fell under their own observation, in proof of the view that œstrus humanus is merely a transference of one of the varieties of the bots found on animals, to the human body. In our October number (1858), we quoted from the Archives Générales an interesting account by Dr. Coquerel of the development of the larvæ of a diptera in the frontal sinuses and nasal fossæ of man. In Dr. Duncan's case, the symptoms were less severe; it is as follows:

E. C——, a girl aged thirteen, came from Perthshire in September, 1858, to reside in Edinburgh. She had never been in bad health till shortly after leaving the country, when she began to suffer pains which she connected with the bots. She first felt a little lump on the back of the neck, which slowly changed its position in various directions; then a hole opened over it, and a worm was squeezed out. Some weeks afterwards, another similar lump was felt on the right side of the trunk. It also wandered about subcutaneously, till a hole opened over it, and it was forcibly rubbed out. A third made its appearance over the spine, high in the chest, then travelled up the neck, when for a time it was lost, and was supposed to reappear on the right side of the neck, where a hole formed over it. Dr. Duncan was now (3rd March) fortunate enough to be called, and observed a small, not inflamed tumour, of the size of a large field-pea, and having an opening on the top as big as a pin's head. In this hole, Dr. Duncan saw one or two black points rolling about. On squeezing moderately the little lump, there was discharged a living larva half an inch long, evidently of the œstrus bovis, and the same as those previously noticed by the girl. A little dirty-yellow juice issued with the animal, containing a few blood globules and pus corpuscles. The girl said that while in Perthshire herding cows, she was much exposed to the air, and was frequently stung by insects.

It appears from Dr. Spence's paper, that in the Shetland Isles the human subject is peculiarly subject to being attacked by the skin-bots; the larvæ occur in exposed parts of the body, and in women who are loosely dressed. They have generally been engaged during the summer in working with peats, in localities where cattle were numerous; in some instances they had lain down on the grass and fallen asleep. Dr. Spence's patients never remembered having felt a sting; they were engaged in the only occupation by which people in Shetland are liable to be detained in localities where the gad-fly or bot-fly may be met with.

The patient's attention, according to Dr. Spence, is first attracted by a severe burning pain under the skin, in a circumscribed spot. It is next observed that the seat of pain shifts its position, and that the course between the two spots is marked by a reddish or ecchymosed line, which fades in a few days. This movement of the seat of pain, attended with a thin linear discoloration of the skin marking its track, may be almost considered as a diagnostic sign. The rate of progress varies; in one case, Dr. Spence traced it between the 29th September to the 10th October, from the left hip to above the left mamma. The larvæ appear to drop out after a time of their own accord, but the safest and most expeditious plan is, as soon as their nidus is ascertained, to cut down upon and remove them.

XII. *Case of Spontaneous Gangrene of the Fingers Cured by Localized Electrisation.* By M. Duval. (Quoted from Echo Médical Suisse, Sept., 1858, in Gazette Hebdomadaire, Jan. 28th, 1859.)

A girl aged seventeen, without known cause felt violent pain in the hands; the four fingers of the right and two of the left hand became slate-coloured, cold, insensible—in short, exhibiting all the symptoms of incipient gangrene. Movement was almost entirely abolished in these fingers. The induced current was applied, giving rise at first to increased pain, but soon arresting the sufferings of the patient. After ten or twelve sittings, at about the end of the week, the sensibility, the normal temperature and colour, as well as motion, were restored. The epidermis came off to the extent of the first appearance of the gangrene. During electrization, a fœtid sweat was noticed over the electrified parts. The editor of the 'Gazette Hebdomadaire' expresses some doubt as to the propriety of terming this a genuine case of gangrene, but admits the importance of the case, as showing the influence of induced electricity in restoring the circulation.

XIII. *Researches into the possibility of Recalling to Life Temporarily, in Persons Dying of Disease.* By Dr. Brown-Séquard. (Journal de la Physiologie, Oct. 1858.)

A considerable number of cases of transfusion have satisfied Dr. Brown-Séquard that in mammiferæ dying of various diseases, and especially of peritonitis, life may be restored for some hours by the process, when the agony has all but ended in death; he concludes that in human beings who have already become unconscious, and in whom agony has commenced, the intellectual faculties, the senses, and speech, may be restored for some hours by the combined effect of transfusion, artificial respiration, and bleeding at the jugular. Dr. Brown-Séquard does not give us the result of his entire experience, but he finds that of eleven experiments made upon dogs, cats, and full-grown rabbits, four animals came to life entirely for two, three, and four hours, that three others recovered the circulation, respiration, and reflex action for one or two hours without restoration of the voluntary movements or sensibility, while in the four remaining animals there was no result, except a slight increase of the movements of the heart. The following is a brief summary of one experiment, which the author gives in detail:

In October, 1851, a dog in whom the abdominal sympathetic had been divided was attacked with peritonitis and death was at hand. Voluntary and reflex movements had entirely ceased: there was no respiratory action, and the convulsions of death were limited to a few muscular tremors; the fæces and urine were expelled, the pupil was dilated, and no movement of the heart was perceptible. A silver T-shaped tube was now inserted into the left carotid of the dying dog, and connected with the carotid of another dog, which was attached firmly to a table. The healthy arterial blood at once passed to the head and heart of the dying animal. At the same time, the left jugular vein and one of the femoral veins of the dying dog were opened. The jugular almost immediately, and the femoral after twenty or thirty seconds, yielded some blood. The period of transfusion lasted two minutes, and ligatures were placed on the carotids of both animals. The jugular vein was left open four or five minutes, during which the heart was felt beating. Gradually the pulse was restored as the blood flowed from the jugular. Artificial respiration was then had recourse to, and continued for half an hour. Eight minutes from the commencement of the insufflation, the cornea became sensitive, and soon after, respiratory movements were perceptible. After twenty minutes, the animal executed voluntary movements, and on leaving off insufflation, respiration continued regularly. The return to life was complete as regards all the chief functions of animal and organic life. Though feeble, the animal raised himself on his fore-feet and wagged his tail when caressed. The dog lived eleven hours and a half after the transfusion.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

I. *On Intraocular Hæmorrhage consecutive to Extraction of Cataract.* By Mr. W. White Cooper. (Annales d'Oculistique, tome xl. p. 1.)

This is a note by Mr. Cooper in reply to M. Rivaud-Landrau's paper upon the subject noticed in our last Report. He commences it with the narration of an interesting case that lately occurred to him. He performed the operation of extraction by the superior incision upon a lady, aged eighty-five. Notwithstanding her great age, she was very active and in excellent health, her eyes being perfectly healthy; and, in fact, no contraindication being present beyond the fact of her having wept much of late in consequence of domestic sorrow. The operation succeeded completely, no force being required to extract the lens, and not a drop of vitreous humour escaping. Everything seemed to promise a rapid convalescence, when, on the evening of the fourth day, while Mr. Cooper was conversing with her, and just after she had blown her nose, the patient complained of excessive pain in the eye. Nothing could be seen, and under the belief that the pain was only spasmodic, fomentations were applied. It kept increasing, and at the end of five minutes blood issued from between the eyelids, the edges of the wound having cicatrized with such solidity as to require this space of time after the rupture of the vessel before they yielded. A bloody mass now gradually projected, evidently consisting of the hyaloid membrane filled with blood, and to which, on examination, a portion of the retina was found adhering. The flow of blood, which was of a venous colour, was not considerable; but, notwithstanding the application of ice, it continued to ooze away during thirty-six hours. Much nausea and general uneasiness followed, and the patient's life seemed for some time in danger. She rallied from this condition, and is now well, the globe being distended by a coagulum. There has been but little suppuration, and the eye will probably undergo atrophy.

While believing in the probable correctness of M. Rivaud-Landrau's opinion, that the sudden issue of a portion of the vitreous humour may be considered, in the majority of cases, to bear a direct relation to intraocular hæmorrhage supervening upon extraction, Mr. Cooper considers that there must also be some predisposing cause, some abnormal conditions of the vessels of the choroid—inasmuch as the issue of a portion of the vitreous humour during the operation is of frequent occurrence, while it is very rare to find this followed by hæmorrhage. In the case just related no such escape had occurred. In Mr. Cooper's opinion, the most reasonable explanation that can be given of the occurrence of intraocular hæmorrhage is, that in certain cases the sudden removal of the part, which serves as a support to the vitreous humour, determines rupture of the enfeebled, and perhaps congested vessels of the choroid. In other cases (as in the one here related) the diseased vessels give way from sudden distension or forcible compression of the eye. A case that occurred to Mr. Bowman demonstrates that rupture of the vessels may take place on the sclerotic surface of the choroid.* In a postscript Mr. Cooper refers to specimens in the Moorfields Museum, demonstrating the presence of coagula between the choroid and sclerotic, indicating that the vessels furnishing the blood in intraocular hæmorrhage are those of the external surface of the choroid, and not those in closest union with the vitreous humour.

II. *Statistics of Fracture.* By M. Velpeau. (Gazette des Hôpitaux, 1858, No. 102.)

M. Velpeau, in his annual summing up at his Surgical Clinic at la Charité, furnishes an account of the fractures admitted under his care during the last twelve years. These were 1497 in number, three-fourths of them occurring in persons of the male sex. There are, however, certain fractures to which females are as liable as males—e. g., fracture of the neck of the femur. Here occupation has nothing to do with the accident, a fall producing it in either sex, age exerting the same effect in modifying the condition of the osseous tissue of the cervix in both. We may regard also a special predisposition to this fracture in females as arising from the predominance of the fatty element, which is usually more abundant in them than in males. We also find a pretty equal number of cases of fracture of the lower end of the radius in either sex—the same cause producing it, usually a fall on the palm.

The following was the order of frequency of occurrence of the respective fractures: Radius, 165; femur, 157; fibula, 141; ribs, 134; clavicle, 132; humerus, 109; tibia, 60; olecranon, 34; ulna, 28; patella, 25; scapula, 10; fractures of the leg taken altogether, 164. Then came fractures of the cranium, spine, &c. From these figures it results, 1. That fractures of the radius are of most frequent occurrence; 2. Fracture of the leg comes next, but that is only when both bones are reckoned; 3. The femur comes thus immediately after the radius; 4. The fractures of the fibula alone amount to 141, but these figures do not represent all the instances of fracture of that bone, as it existed, in fact, in the greater part of the cases designated as fracture of the leg; 5. Fractures of the humerus are set down at 109; but causes of error may easily prevail here, as certain fractures of the glenoid cavity, anatomical neck, and of the olecranon, may have been mistaken for fractures of the body of the humerus, and *vice versa*, 6. Fractures of the tibia are set down at 60 cases; but it is probable that some of the cases set down as fracture of the leg were fractures of this bone alone. 7. Fractures of the forearm are put down at 52; but here the same difficulties from error of diagnosis may arise, as must be the case in any part of the skeleton presenting a complex composition and multiple bones. 8. There were 46 cases of fracture of the cranium; but the signs of this accident are not always sufficiently obvious. 9. Comparing the fractures of the upper and lower extremities together, we find that there were 564 of the former and 587 of the latter, giving a difference of 23 in favour of the lower extremities, in spite of the considerable proportion of fractures of the clavicle.

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* Transactions of the Pathological Society, vol. ix. p. 364.

III. *On the Preliminary Treatment of Affections of the Urethra.* By M. Civiale. (L'Union Médicale, 1858, No. 129.)

On the occasion of presenting the new edition of his work "On the Diseases of Genito-Urinary Organs" to the Academie des Sciences, M. Civiale draws its attention to procedures as yet but too little known, having in view facilitating operations practised on the genito-urinary organs, and rendering them less painful and more successful. They consist in a special preparatory treatment. The lining membrane of the urethra is, in the normal condition, so sensitive in the majority of persons that the simplest instrument cannot be passed along it, even with the greatest care, without exciting a painful sensation of burning heat, which may go on to that of excessive pain. The sensibility of the neck and body of the bladder is less developed than it is usually supposed to be; but under the influence of inflammatory action it may become so exaggerated as to render all operative procedures impossible. When in a state of disease, in place of a simple catheter we have to introduce instruments into the urethra which, by reason of their form, volume, or rigidity, induce distension or friction of the part, the suffering excited will be much increased; and when, in place of a mere temporary passage of an instrument, we have to leave it in contact with the surfaces, to execute prolonged and extensive movements, or to invade the texture of the parts by means of caustic or a cutting instrument, the severest suffering and alarming reaction may be the result. Moreover, the practitioner, disturbed by the cries and involuntary movements of the patient, and fearing the possible consequences of too great irritation, sometimes either renounces a desirable operation, or desists from its performance before he has completed it.

Speaking from the multiplied experience of many years, M. Civiale states that this sensibility of the urethra and bladder may be most effectually subdued by the methodical employment of soft wax bougies. A very small, smooth, soft bougie is to be passed into the urethra and immediately withdrawn, and the same operation is to be repeated daily. If the urethra is very irritable, the bougie is to be withdrawn as soon as the patient begins to complain, although it may not have passed far in. Sometimes the bladder is not reached until after four or five days. By proceeding with extreme slowness, and never by irregular movements, both in passing in and withdrawing the bougie, and never letting it remain, the instrument causes very slight pain, and this even is diminished every day. Very gradually the size of the bougie is increased until one is reached which fills the normal capacity of the urethra without producing distension. During this local preparation, which usually requires from eight to twelve days, any general irritation or morbid conditions that may exist should be remedied.

To appreciate the benefit of this simple procedure, it requires a person to be present at a series of operations practised on patients, who have, and upon those who have not, been so prepared. Not only in the former are the operations executed with far less suffering and much more facility, but they are followed by much fewer of the consequences of violent reaction.

This procedure is not to be placed on the same line with opiates and anæsthetics, for by it we seek to obtain a slow and progressive diminution of the sensibility of a determinate organ, the action being exclusively local and in no wise changing the general conditions of the organism. When we resort to opiates or anæsthetics, we leave out of view the organ on which we are to operate; and it is upon the nervous system, the centre of life and perception, and consequently upon the entire economy, we seek to make an impression. In the one case we effectually diminish the irritability of the organ, in the other we disguise or suspend it. The one leaves the patient in the full exercise of his faculties, the other plunges him into a temporary state of intellectual and moral annihilation. "The inconveniences of opiates are well known, and I have not here to discuss the utility of anæsthetics in the general practice of surgery; but I cannot too forcibly protest against the abuse that has been made of them in the treatment of the diseases of the urinary organs. With the exception of cystotomy, external urethrotomy, and some other rare operations, the employment of chloroform is not only useless, but liable to cause the committal of grave mistakes and to give rise to great misfortunes."

IV. *On the Treatment of Aneurism by Digital Compression.* By Dr. Samuel Gross. (North Amer. Medico-Chir. Rev., vol. iii. pp. 78–85.)

In this paper, Dr. Samuel Gross relates the case of a negress, aged thirty-two, who applied to him on account of an aneurismal tumour situated within Scarpa's triangle, in the femoral region. The tumour was 4¼ inches long and 5¾ broad, the signs of aneurism being well marked. An instrument for making compression not being found to fit well, it was determined to employ the fingers in effecting this. The aid of several assistants was procured, and a system of continuous compression of the femoral artery, as it passed over the pubic bone, was commenced on June 10th. The considerable pain produced at the point of pressure compelled its temporary suspension, and the administration of occasional doses of morphia. However, the ultimate result was highly satisfactory, for she was discharged well on the 6th of July, and when seen on the 13th October, the tumour was very solid, and had diminished to the size of a walnut. The total amount of time during which the compression was continued was forty-five hours and fifty-five minutes—viz., thirty-one hours and twenty minutes at the first *séance*, and fourteen hours and thirty-five minutes at the second. The compression was almost total, preventing the blood from entering the tumour at all; and the whole time during which the blood did traverse the sac did not exceed two hours.

Dr. Gross refers to 23 other cases, in which digital compression has either partially or solely constituted the means of treatment, 15 of these being popliteal, 4 femoral, 2 inguinal, and 2 artero-venous aneurisms. Of these 23 cases 15 were successful, and 8 failed. In the successful cases, in 5 digital compression was applied principally and alone, in 4 it succeeded after apparatus had been abandoned, in 5 it was employed alternately with apparatus, and in 1 it succeeded when combined and alternating with apparatus and direct compression of the tumour.

The following are the conclusions the author arrives at:—1. Digital compression, uncombined with apparatus, was first attended with success in the hands of Dr. Knight (of New Haven); but to M. Vanzetti is due the merit of having first introduced it into practice. 2. It has never been followed by bad consequences, and when not successful it so modifies the tumour and the collateral circulation as to render a cure by other means almost certain. 3. It has been employed alone, either previous or subsequent to mechanical compression, in 14 cases, 8 being failures. 4. In only 7 cases has it been employed primarily and alone, and in 5 with perfect success. 5. When double and alternating, it has effected cures in every case (5 in number), and therefore deserves special attention. 6. In most of the cases, the compression has been total, but this is not necessary for a favourable result. 7. It has effected cures, whether it was continued, interrupted, or intermittent; in some cases the patient applying the pressure. 8. When properly employed, and continued for a sufficient length of time, and in suitable cases, it can scarcely fail to accomplish a cure. Inguinal aneurisms are not fit cases for this procedure. 9. It is less apt to give rise to inflammation of the integument, and has been borne when mechanical pressure has produced an eschar. 10. It can be used when apparatus has failed or become intolerable. In a majority of such cases, a cure has been accomplished 11. In certain situations it can be made to bear upon the artery alone, It is far less painful, and requires a much shorter time for the cure than any other method of treatment.

[Dr. Gross is not aware of the extent to which Professor Vanzetti has carried the practice of intermitting compression.]*

V. *On Labial Cancer.* By Professor Riberi. (Omodei's Annali, vol. clxvi. p. 331.)

In a notice of the forthcoming third volume of Professor Riberi's 'Lezioni Orali,' an account is given of his experience with respect to labial cancer at the Turin Clinic. The *ages* of the 81 patients were as follows:—2 between twenty and thirty, 3 between thirty and forty, 11 between forty and fifty, 28 between fifty and sixty, 20 between sixty and seventy, and 17 between seventy and eighty. Of these, 69 belonged to the *peasantry* class, a predominance perhaps attributable to their unnutritious food, their abuse of peppers, garlic, vinegar, and the like condiments, their neglect of personal cleanliness, and their exposure to vicissitudes of the weather. Another predilection of the disease was for the *male* sex and the *lower* lip, inasmuch as only 3 of the cases occurred in women, and in only four instances was the upper lip affected, two of these occurring in men and two in women. In all but one patient, the sanguineous temperament was manifested in a greater or less degree, showing the influence of the conditions of the bloodvessels and of the blood in this disease as compared with that of the nervous system. In 76 of the subjects, the constitution was good, robust, or even athletic. This confirms the observation made by Pravas, that the general belief is erroneous, which supposes that lymphatic, delicate, cachectic constitutions, are most liable to cancer. Persons become cachectic, and enfeebled as the disease advances, as its result, not as its cause.

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* See Brit. and For. Med.-Chir. Rev., Jan. 1859,

In most of the patients, an unhealthy state of the skin prevailed, and there were few cases in which some complication was not observed, arising from disturbances of the respiratory or circulatory organs, varix, varicose ulcers, chronic gastro-hepatitis, pellagra, &c. After a while, the *glands* in the vicinity enlarge, and it is of importance to determine whether their increase be merely sympathetic or symptomatic of invasion of the disease. In the former case, a single gland only usually becomes enlarged, being of recent origin, round or oval in form, moveable, and liable to spontaneous changes in size; it is painful and tender to the touch, the skin being warmer than usual, and in some cases slightly reddened. In symptomatic enlargement, two or more glands are almost always affected, large and indurated lymphatic cords stretching between them, and often down the side of the neck. After a while, the glands may acquire a large size, assuming an irregular form, becoming more or less fixed at their base, being slightly moveable, and not undergoing spontaneous change in size.

Before proceeding to the operation, M. Riberi submits his patients to hygienic and medical treatment calculated to relieve any complication or subdue any inflammatory action that may be present. Some cases of cancroid would indeed be cured by such procedures, had the patient sufficient patience to await the result. Believing the employment of caustics mischievous in almost all other forms of cancer, M. Riberi regards them as of great utility in epithelial cancer, especially of the face, when the base is small enough to admit of its entire destruction. But as the tissue of the lip is very soft and yielding, and cancer soon sends widely-spread roots into it, and as patients usually do not apply until the lesion has thus become extensive, the employment of caustics is not admissible. Moreover, considerable deformity may result from its application, and an aggravation of the disease may be produced when the whole has not been extirpated. The operation with the V incision, having its base towards the labial edge, and conjoined when necessary with cheiloplasty, is that to which Professor Riberi gives the decided preference. He enters into considerable details upon this part of the subject, for which we have not space. Whatever form of the operation be adopted, he insists upon the necessity of removing during its performance all glands that may be symptomatically affected.

Of 78 persons operated upon, 73 left the Clinic cured; some of these, however, returned at the end of more or less long periods suffering from other cancerous diseases, 2 succumbed to a reproduction of the disease while in the Clinic, and 3 died after the operation from causes not connected with it. The following are the conclusions drawn from a consideration of the cases of the 81 patients:—1. The disease almost always commences as epithelial cancer or epithelioma of the skin or mucous surface of the lip, spreading thence to the parenchyma, and very rarely begins in this last, extending thence to the surfaces. 2. The skin is almost always primarily affected, and only in some rare instances by morbid diffusion from the mucous surface. 3. Although very frequently unaffected at first, the mucous membrane becomes almost always implicated in the course of the disease. 4. The cellular tissue of the parenchyma is always simultaneously affected, as are very frequently the mucous and sebaceous crypts, to the great number of which in the lips Benjamin Bell attributed the frequency of labial cancer. 5. The muscular tissue is sometimes unaffected, sometimes participates slightly in the disease, and in some cases is so involved as to become entirely destroyed. 6. Whatever our nosological distinctions may be in respect to the species of cancer, Nature shows how ill-founded they are, by exhibiting more than one of these together; but facial cancers are those in which this junction is seldomest observed.

VI. *On the Union of Wounds by Collodion.* By M. Goyrand. (Gazette Médicale, 1858, Nos. 49 and 50.)

M. Goyrand observes that collodion intended to act as an agglutinative agent should possess the consistence of a very thick syrup. The following formula furnishes such a collodion: Sulp. ether. at 60°, hundred parts; pyroxiline, eight parts; and alcohol at 36°, five parts. Collodion so prepared is very adhesive, drying into a thin transparent pellicle, only capable of being removed by means of ether. It possesses, however, but little extensibility, and if applied to parts liable to change in volume it cracks. When, therefore, it is required as an impermeable covering for parts which are inflamed or menaced with inflammation, its elasticity should be increased by the addition of a little castor oil or turpentine. This elastic collodion should not be employed as an agglutinative agent.

Any very superficial breach of surface may be united by means of collodion. It is a bad plan, however, to apply it upon strips of linen, which conceal the wound from the surgeon and render the exact apposition of its edges doubtful. The edges should be brought into perfect contact, and kept somewhat pressed together by the fingers of an assistant; the part being *well dried*, a thick layer of collodion is then applied by means of a pencil, and is extended for some little distance beyond the edges of the wound, the contact of the parts being maintained by the assistant until the pellicle has dried. This answers admirably in clean wounds comprising only a portion of the thickness of the skin, such as the small cuts on the hands and face, which if neglected give rise to much inconvenience by inflaming and suppurating. The ordinary plasters not only conceal the condition of the wound, but become detached by washing, while the collodion does not separate until healing is effected.

This simple procedure is insufficient when there is a tendency to separation in the wound, and when there is a considerable loss of substance. Strips of adhesive plaster often hold such wounds in only imperfect apposition, and make insufficient or irregular traction; while when union by the second intention is only possible, they impede the flow of the discharge. In such cases M. Goyrand makes a frequent use of what he calls the *dry collodion suture*. Two strips of linen are prepared, being somewhat longer than the wound itself, and broad in proportion as it is deep. These are soaked in collodion, and fastened parallel to the two sides of the wound, at some millimetres from its edges. To these strips, and perpendicular to their direction and to that of the wound, are stuck by one of their outer extremities some narrow, thin, and very supple ribbons, forming part with the linen strips, until the edges of the wound are reached. The ribbons vary in number according to the length of the wound, and their free ends being placed opposite each other, may be tied together in pairs. This tying draws together the linen strips, and consequently the edges of the wound, which can then be brought together with as much exactitude and pressure as may be deemed desirable. This mode of uniting the deeper wounds possesses many advantages. It maintains the parts in exact apposition unattainable by plasters, is applicable to wounds taking any direction, and admits of the consentaneous employment of irrigation if required. Vidal's pretty little instruments, the *serres-fines* suitable enough for a superficial wound, only bring the edges of the surface of a deep wound in contact, and they cannot be kept on more than twenty-four hours without risking the production of small eschars. The collodion, acting over large surfaces, brings into contact not only the edges of the incision, but also the subcutaneous bleeding surfaces. It has the advantage over the ordinary suture of being painless, of remaining *in situ* as long as required, and of allowing of the wound being easily re-opened on account of secondary hæmorrhage.

M. Goyrand has no intention of indiscriminately recommending the substitution of collodion for other means of securing union, but claims for it admission into practice concurrently with them. He states his practice in the matter as follows:—1. When I have good collodion at my disposal, I always employ a layer of it to unite very superficial clean cuts, to which gummed plasters are usually applied. 2. When the wound comprises all the thickness of the dermis, its edges being clean, easily approximated, and not likely to become displaced, I often employ the *serres-fines*, which allow of very exact adaptation, and are very easily applied. 3. No uniting means is comparable to the *serres-fines*, when the object is to bring together the edges of a wound involving very thin and very moveable skin, and when one edge of the wound is formed by delicate skin and the other by a mucous membrane. Thus, I consider them preferable to any other means for fixing the flap in certain anaplastic operations of the face, and for uniting the wound made by circumcision or castration. The difficulty of obtaining an immediate union of a large wound of the scrotum by means of plasters, or even by the interrupted suture, is well known. A rolling of the edges of the incision takes place, bringing the epidermic surface into contact with the deeper seated parts, and union never takes place without suppuration. The *serres-fines*, on the contrary, maintain the edges of the wound in exact contact, and usually lead to union by the first intention. 4. Adhesive strips may be advantageously employed in wounds comprising the entire thickness of the skin, or even when the subcutaneous fatty tissue is divided, providing their direction is longitudinal. I especially employ them when I think that the compression they exert may prove of utility. 5. When the wound, though simple, is transverse, and does not seem to be able to be united throughout its depth by means of *serres-fines*, I obtain the best effects from the dry collodion suture. 6. This suture is the sole means by which we can, through an energetic and sustained action, keep together the edges of a large wound accompanied by loss of substance, whether our object be to secure complete contact, or to change the round form of the wound, so unfavourable to cicatrization, into an elongated elliptical form. 7. I know of no means comparable to the dry suture in the case where, during the production of secondary union, it is desired to keep for a long period parts in contact whose adhesion to each other it is sought to obtain. 8. Finally, I am certain that no means can replace the twisted suture in hare-lip operation, or the deeply-acting sutures which are employed in uniting penetrating wounds of the abdomen and the pared edges of a ruptured perineum.

VII. *On the Treatment of Wounds and Ulcers by Ventilation.* By Professor Bouisson.
(Gazette Médicale, 1858, Nos. 44, 45, 46, 47, and 48.)

Superficial wounds in animals become rapidly desiccated, and cicatrization is performed in the most favourable manner under the crust so formed. The idea occurred to Professor Bouisson of endeavouring to imitate this procedure by inducing the desiccation of wounds and ulcers through the employment of *ventilation*. Cicatrization might naturally be expected to take place more rapidly and more favourably under the protective crust so formed, than when the open surface continued exposed to the air and other sources of irritation.

The trial of the plan was commenced at the St. Eloi, Montpellier, in March, 1857. It was applied to various examples of recent and old wounds, local ulcers, solutions of continuity after operations, &c., these altogether amounting to above twenty in number. We can refer only to some of the details given by the author. The first case was a large ulcer of the leg of many years' standing, characterized by the usual obstinacy of the malady. Four times in the twenty-four hours a sharp current of air was propelled over its surface during a quarter of an hour, the common bellows being the instrument and the patient the operator. By the very next day a crust had commenced forming, and by persistence for several days in the ventilation it was rendered thicker and thicker. At the end of about a fortnight the crust was detached by means of a bath, and the sore was found much reduced in size and of a very healthy aspect. A reproduction of the crust by renewed ventilation still further reduced its size; and had the patient not become content with the benefit received, complete cicatrization would have followed. More rapid success attended the employment of the means in more recent cases; thus, a small wound in the leg was healed in eight days, and even extensive wounds, after injury or operation, were more rapidly healed than by any of the ordinary procedures. Generally speaking, however, the procedure could not be adopted at once as most of the patients admitted having some complication, this required removal before the production of cicatrization could be attempted. The immediate effect of its employment was the production of a sense of coolness, while the pain became moderated and the surface of the wound paler, as if under the action of an astringent. The crust increased in thickness and tenacity in proportion to the duration and force of the ventilation, and after some days assumed a horn-like texture—the discharge, if still abundant, escaping somewhere at its detached circumference. The crust takes no part in the cicatricial process which is going on beneath it, but acts simply as a natural protective dressing. When, however, the wound is small, uninflamed, and exempt from purulent secretion, the evaporation induced dries up the plastic lymph itself, reducing it to its organizable portion. The crust, in this case, becomes confounded with the cicatrix itself, which, forming rapidly, fills up the interval between the edges of the wound, and calls to mind what takes place in union by the first intention.

M. Bouisson believes that other therapeutical effects are also derivable from the plan, and enumerates these under the several titles of sedative, astringent, siccative, and antiseptic action. Into his account of these we have not space to enter, and merely add the general conclusions of his essay:—1. Ventilation of wounds and ulcers is of utility as a curative agent in a very great number of cases. 2. It induces healing by desiccating exposed surfaces, covering them with a crust formed of the residue of the co-effused liquids. 3. This crust acts by isolating the wound from the contact of the air, and by favouring a simpler and more regular mode of cicatrization than that which takes place in exposed wounds, the dressing of which frequently destroys the cicatrix while in process of formation. 4. Subcrustaceous cicatrization bears the same relation to open wounds, that subcutaneous cicatrization does to closed wounds. 5. Ventilated wounds and ulcers cicatrize more rapidly and with fewer primary or consecutive accidents than do wounds dressed with fatty bodies or other medicinal topical applications. 6. Ventilation develops effects which are exhibited by local refrigeration, antiphlogistic and astringent action, desiccation of the wound, together with its isolation and occlusion, and its preservation from the septic action of pus. 7. It may be performed by the ordinary bellows or any other apparatus. It should be continued for a quarter of an hour, and repeated several times a-day. 8. It is applicable to old or recent, to small or large wounds, to ulcers, burns, &c. It may also be preceded by other means of general treatment, or employed as adjuvatory to these. 9. It presents several indirect advantages, especially economy and simplification of dressing, and the maintenance of greater cleanliness.

VIII. *Loss of the Testicle consequent upon Scarifying the Scrotum.* By M. Demarquay.
(Bulletin de Thérapeutique, tome lv., p. 549.)

Long since M. Velpeau recommended the little operation of scarifying the scrotum in cases of gonorrhœal orchitis; and its practice is usually followed by very advantageous results, especially when it gives issue to a notable quantity of citron-coloured fluid accumulated in the tunica vaginalis. M. Vidal went much further than this, by opening in painful orchitis the tunica albuginea itself, an operation he declared to be without inconvenience, and one which he performed himself more than four hundred times. M. Demarquay for some years past has not had recourse to this practice; and he does not even employ scarification of the scrotum unless there be fluid in the tunica vaginalis, fearing injury to the gland itself. Having seen an account of a case by M. Montanier, in which severe hæmorrhage followed scarification, he determined to make known four instances which came under his own notice, in which still graver consequences ensued.

The two first cases occurred in persons between fifty and sixty years of age, in whom orchitis coming on during the treatment of stricture, scarifications had been resorted to. They only came under M. Demarquay's notice when the testicles were in part gone, spermatic filaments being found every morning in the dressings. The patients were cured, but the testes were lost. In the third case, in a man forty-six years of age, intense orchitis came on during treatment of stricture; and after various means had been tried in vain, scarifications were resorted to, and gave rise to a tolerable flow of blood. No great amendment resulting, the author was called in next day. All the apertures were closed but one, the edges of which were everted. Presently a plug of greyish matter, at first no larger than a millet-seed, but gradually increasing to the size of a cherry, projected. It proved to be the testicular substance, infiltrated with a little pus; and gradually the whole of the testicle was in this way discharged. The fourth case occurred in a young man, under the same circumstances, but here the loss of the testis was only partial. It may be supposed that these are mere coincidences, being really examples of inflammation of the testis terminating in suppuration. Such termination is, however, very rare, as the experience of MM. Ricord and Monod sufficiently prove.

The less liability to such an occurrence in gonorrhœal orchitis, as compared with orchitis connected with disease of the genito-urinary apparatus, may be thus explained. In gonorrhœa, it is the epididymis which becomes especially seized with inflammation, this being propagated to the envelopes of the testis, and sometimes to the organ itself, and to the tunica vaginalis. Fluid then distends the latter, and great relief may follow the discharge of this by punctures. But in orchitis symptomatic of disease of the urinary organs, it is generally the testis that is affected, and the same abundant effusion into the vaginalis does not take place. Scarification may here easily attain the secretory organ itself, and ill effects will the more readily follow in consequence of the economy being already under the influence of the original disease.

IX. *Six Cases of successful Operation for Congenital Cataract in one Family.* By Dr. Williams. (Boston Medical and Surgical Journal, vol. lix., p. 149.

These cases occurred in a German family living at Boston, the mother and four children suffering from cataract in both eyes. The mother was not aware of any cases having occurred among her eight brothers and sisters or other relatives, and she has two other children who exhibit no traces of cataract. In her the opacity of each lens was greatest at the centre, the margin being comparatively clear, so that in a moderate light she could see enough to perform her household duties in an imperfect manner. The capsules, as also in the children, were transparent. In all the children, opacities, consisting in dots of various sizes and occupying different planes, occupied nearly the entire field of vision; and in a bright light, reflections from crystals of cholesterine were plainly seen. In a bright light the children were nearly blind, and their sight was never sufficient to allow of their learning to read or to get their livelihood. Six operations were performed on the same day upon three of the children, aged seventeen, twelve, and ten respectively, the lens and capsule being freely divided by a needle introduced through the sclerotic. Sparkling reflections from cholesterine were distinctly seen in the posterior chamber. It was several months before the pupils all became clear, and in the youngest girl one of the eyes had to be operated upon a second time. The children now have perfect vision, and with the aid of cataract glasses will be able to follow any occupation. The eyes of the mother, and of the other child, aged two years, have not yet been operated upon, the woman still feeling too timid. Dr. Williams knows of another family, presenting no less than *seven* cases, others of the family being free from disease. Some of the eyes had been operated upon, but with imperfect success, portions of the capsule left behind having become tough.

[In the January number of the 'North American Medico-Chirurgical Review,' is a very interesting case of congenital cataract, related by Dr. Rohrer of Philadelphia. The patient had been totally blind for sixteen years, and continued in the enjoyment of unimpaired vision at a period of twenty-one years after the performance of the operations.]

X. *On the Operation for Hernia in Children.* By Dr. Ravoth.

(Deutsche Klinik, 1858, No 29.)

Dr. Ravoth communicates an interesting case of inguinal hernia occurring in a male child, fourteen months old. Observed soon after birth, a truss had not been applied to it until the child was six months old, and this, owing to the frequent projection of the large hernia, seems to have been ill-fitting. When the child was seen by the author, incarceration of the hernia had taken place during forty-eight hours. The scrotum was distended with a tumour as large as a man's fist, having very much the appearance of a hydrocele. As repeated attempts with the taxis, under chloroform, were found useless, the operation was at once resorted to. In order to avoid loss of blood, two small arteries, which were divided while laying the sac bare, were at once tied. The parts were replaced without opening the sac, and the wound was simply dressed, a little pad of charpie being laid over the inguinal canal. Chloroform was employed during the operation, and frequently since during the dressing of the wound, on account of the child's cries—the hernia then very easily redescending. In about three weeks the wound had cicatrised, and a properly fitting truss was after a while well borne.

In reference to the history of this case, the question may be asked, when is the most suitable time for the application of a truss in infants? Most practitioners say not before the sixth month, and some even not before the twelfth. This advice is grounded upon the great sensibility of the child's skin, the necessity of great cleanliness, the rapid growth of the child rendering a frequent change of the truss necessary, the rarity of strangulation in childhood, and the frequency with which a spontaneous cure is brought about in congenital hernia. Although these reasons may have their weight in particular cases, the author does not consider that they have validity enough to raise this practice into a general rule. On the contrary, he believes that the truss should be applied at the earliest possible period, and chiefly because the partial or total protrusion of the hernia gives rise to much abdominal pain and gastric disturbances, causing crying and restlessness, and impeding development. The hernia, too, increases in size, and the abdominal ring in width, so that the tumour is retained with more and more difficulty, and a radical cure, which at this early age may usually be expected from a well-fitted truss, becomes more unlikely to happen. The pressure from the large pad and strong spring, too, now required, will not be borne by the child's delicate skin. Finally, there is the danger of strangulation, which at this early age is not always easily detected, and the operation for the relief of which is one of great danger.

The successful issue of the present case is chiefly attributed by Dr. Ravoth to three circumstances—viz., the promptitude with which the operation was performed; the executing this without opening the sac, and dilating the ring with a blunt hook in place of a cutting instrument. This child had rejected all food during twelve hours, and had been deprived of refreshing sleep during forty-eight hours; and had a further delay taken place from the use of baths, clysters, &c., the issue would probably have been different. Moreover, the hernia had not suffered from excessive employment of the taxis, and chloroform was resorted to during its application. After repeating the well-known arguments in favour of not opening the sac, Dr. Ravoth observes that in children this should never be opened when there is any possibility of avoiding it, and especially in the case of large hernias, where repeated protrusion may be produced by the child's cries and movements before the healing is accomplished. In the present instance the hernia was very large, containing the cæcum and *processus vermiformis.* The old practice, now almost forgotten, of attempting to dilate the aperture without the use of a cutting instrument should, when practicable, be preferred. As far as the author is aware, there are about thirty cases on record in which the operation for hernia has been performed on infants and young children; and of the seventeen operations of which the issue is given, nine terminated in death.

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D., Lond.

PHYSICIAN TO THE ROYAL MATERNITY CHARITY.

I. Physiology and Pathology of the Unimpregnated Female.

1. *Researches on the Erectile Organs of Woman, and on the Muscular Tubo-ovarian Apparatus, in their Relations with Ovulation and Menstruation.* By Charles Ronget. (Brown-Séquard's Journ. de Physiol., October, 1858.)
2. *On the Functions of the Placenta.* By Dr. Cl. Bernard. (Gaz. Hebdom., Jan. 1859.)
3. *Note on Hæmorrhages of the Fallopian Tubes.* By Albert Pueck. (Gaz. Hebdom., Jan. 1859.)
4. *Extirpation of an Inverted Uterus by Ecrasement.* By Dr. McClintock. (Dublin Quart. Journ. of Med. Science, Feb. 1859.)

1. M. Charles Ronget's interesting researches on the anatomy of the female organs are summed in the following propositions:

1. In woman the body of the uterus presents the structure of an erectile organ, of a true corpus spongiosum.
2. That to the ovary also an erectile bulb is attached.
3. That in all the classes of vertebrata, and especially in all mammifera, a special muscular apparatus embraces the oviduct and ovary, and effects their adaptation to each other.
4. That the fasciculi of the ovario-tubal muscular membranes (*mesoarium* and *mesometrium*) have such relations with the corpora spongiosa, and especially with their efferent sinuses, that, at the moment of contraction, the meshes of the network in the midst of which run the venous channels, contracting in every direction, these must necessarily be compressed, and the discharge of blood more or less completely stopped.
5. That the contraction of the ovario-tubal muscular apparatus persisting during the whole period of ovulation, the obstacle to the passage of blood and the erection of the corpora spongiosa of the uterus and ovary, which is the result, have the same duration.
6. That menstruation coinciding also, on the other hand, with ovulation, it is natural to consider it as the immediate consequence of the erection of the uterus; a true menstrual hæmorrhage, moreover, never presenting itself but where the uterus possesses a truly erectile structure.
7. That if sexual excitation, as seems probable, can determine the erection of the uterus and ovary, it is easy to account by that fact for the coincidence of the periods of menstruation and ovulation.

2. Dr. Claude Bernard has communicated to the Société de Biologie, of Paris, an account of some researches on the function of the placenta. He had some time before announced the presence of glucose in the amniotic fluid. In pursuing his researches into the origin of this substance, he found that the placenta of certain mammifera contains in the normal state a considerable quantity of glycogenic matter. Ruminants alone seemed to offer an exception; but M. Bernard now finds that in these the glycogenic matter, instead of being met with in the placentas, has its productive organs in certain points of the amnios. The glycogenic matter of fœtal age, whatever the species under examination, is formed by cellules, which in all their histological characters are exactly similar to the cellules of the liver of adult animals. In the rodents these cellules are formed between the fœtal placenta and the maternal placenta. In ruminants, there are seen on the amnios whitish spots, which have either been completely overlooked by observers, or taken for epithelial pathological productions. These white spots are composed of agglomerations of cellules similar to the preceding. On microscopical examination of these spots, they are seen to be composed of a kind of papillæ filled with cells, and circumscribed by an amorphous membrane. Iodine gives a rose colour to the cellular contents. The layer of cells separating the fœtal and maternal placentas in rodents, and the papillæ containing the cells in ruminants, are developed from the earliest epoch of fœtal life, and immediately enter upon their functions. Like the hepatic cells in form, their function is similar—at least, as far as the production of glycogenic matter is concerned, for the author has not yet concluded his researches relative to the possible secretion of bile by these organs. It is a true liver, absolutely unknown hitherto. Whilst these organs are in function, the internal liver of the fœtus is in a rudimentary state; its cells, perfectly embryonic, produce

neither glycogenic matter nor glucose. Later they change their appearance, take gradually their normal shape and dimensions, and begin to secrete glycogenic matter. At the same time, the hepatic cells of the placenta or of the amnios become atrophied, and disappear.

3. M. Albert Pueck, house-surgeon to the Hôtel Dieu at Toulon, makes an interesting communication on hæmorrhages of the Fallopian tubes. He cites the following cases:

1st. A married woman was attacked with small-pox; when admitted into hospital she complained of acute pain in the lower part of the lumbar region and towards the sacrum, and this although she had menstruated a fortnight before. She rolled about in the bed screaming. Two days later, she was seized with profuse uterine hæmorrhage, and died almost suddenly. On examination, the uterus was found full of clots; the mucous membrane was quite healthy, except at the fundus, where it was thickened, violet, infiltrated with blood; at this point a clot remained, which stretched into the left Fallopian tube. The tubes were of a violet aspect, of the size of the little finger, and filled with a large vermicular clot. There was not a drop of blood or serosity in the peritoneum. The right ovary presented no rupture, but had a clot the size of a small walnut on its outer surface. The left ovary, of the size of a hen's egg, was almost entirely converted into a sac containing a fatty matter and hairs. This case was reported by M. Laboulbène to the Société de Biologie in 1853; the conclusion being that it was a case of hæmorrhage of the Fallopian tubes.

The next case occurred under the observation of M. Pueck himself. A strong, robust woman, aged thirty-eight, was admitted on the 3rd of July, 1858, and died ten days after, of meningeal hæmorrhage. The mucous membrane of the uterus was quite healthy; at the level of the left horn were some sanguinolent mucosities, resembling those contained in the inner half of the corresponding tube. The right tube contained a white opaque mucus, it was fixed to the anterior surface of the ovary; two or three serous cysts the size of a pin's head were on the oviduct; the mucous membrane near the fimbriæ was dull red, and pale elsewhere. The left tube was violet, as if ecchymosed; its outer third was as large as the index finger; the fimbriated end was widely open, and connected with the ovary by a tubular membrane continued from it, forming a complete canal to the ovary. This portion was dilated by sanguinolent mucosities, and on pressure escaped from the uterine mouth of the tube; the mucous membrane was dark red, finely injected. There was no rupture of the ovary; but an apoplectic cavity in its substance. This the author considers to be a case of tubal hæmorrhage, partly emptying itself into the uterus.

4. Dr. McClintock relates an important case in which an inverted uterus was removed by linear *écrasement.* The patient, aged twenty-two, mother of one child, very anæmic, was admitted into the Dublin Lying-in Hospital in September, 1858. For twelve months she had suffered very profuse discharges of blood, always coming on at the menstrual periods, and lasting for fourteen or twenty-one days. A pediculated tumour, of pyriform shape, and of the size of a walnut, was found low in the vagina; the neck of this tumour was embraced, but not constricted, by the thin os uteri; it was quite insensible to ordinary manipulation; its surface was smooth, dark pink, and discharged blood when scratched. The patient had been delivered after a protracted labour, by a rude country midwife, fourteen months before; the after-birth, she says, twice "slipped away" from the nurse, the cord being broken. The tumour being drawn down by a vulsellum, the os was entirely effaced, the vagina becoming quite continuous with the neck of the tumour. This led to the conclusion that the case was one of inverted uterus. Several attempts were made under chloroform to effect re-inversion, without success. On the 20th of October, a silk ligature was passed round the neck of the uterus by Gooch's canula; this caused much pain, and some vomiting. In the evening the ligature was tightened, and again on the next day. After forty-eight hours the écraseur was applied below the ligature, the uterus having been drawn down by a vulsellum. The chain was worked very slowly, the uterus being severed in eight minutes. Pain attended the operation; and febrile excitement followed; opium was given, and turpentine epithems applied. In a fortnight the patient was allowed to get up. Six weeks afterwards, the os uteri presented almost the ordinary appearance; a catheter passed about one-third of an inch up the cervical canal. On the 27th of December, Dr. McClintock was informed that the patient was quite well, but had not menstruated.

II. PREGNANCY.

On the Diagnosis of Pregnancy. By Professor HECKER. (Monatsschr. f. Geburtsk., Dec., 1858.)

Professor Hecker, of Marburg, has published the result of extensive investigations into the prognostic value of the penetrability to the finger of the os internum uteri in pregnancy. It is generally known that shortly before the onset of labour the os uteri internum opens. Dr. Hecker has endeavoured to give precision to the indications of this phenomenon. Since 1833, when he succeeded to the charge of the Marburg Lying-in Hospital, records have been kept of the date on which the os internum was ascertained to be open, and of the lapse from that event to labour. He of course points to the difficulty, that the time when the opening of the os internum was *felt* was not necessarily that when the os *first opened;* but he believes that the results of a large number of observations compensate for this source of error, although, since all the error is always on the same side, it is not easy to see how the multiplication of cases can tend to its neutralization.

Dr. Hecker remarks that he has several times observed, in the case of women who had walked a long distance to come to hospital, or who recently had undergone violent exertion, that the inner os uteri was penetrable on admission, but closed again after a few days' rest.

His general results are as follows:

Out of 2593 persons examined between 1833 and 1858, there were 946 in whom the inner os uteri was penetrable to the finger. Of these 946,723 were pluriparæ and 223 primiparæ. Of the 723 pluriparæ,

366	were delivered	within	7	days of ascertained penetrability.
510	"	"	14	"
586	"	"	21	"
638	"	"	28	"
85	"	after	28	"

Of the 223 primiparæ,

138	were delivered within	7	days
169	"	14	"
189	"	21	"
209	"	28	"
14	went over	28	"

Thus it appears that in primiparæ, penetrability of the inner os uteri was followed by labour in seven days in 62 per cent. of the cases, and in pluriparæ in about 50 per cent.; that labour followed within fourteen days in 75 per cent. of the primiparæ, and in 70·5 per cent. of the pluriparæ.

III. Labour.

1. *Four Deliveries by Cæsarian Section in extreme Contraction of the Pelvis.* By Dr. G. Pagenstecher. (Monatssch. f. Geb., Aug. 1858.)
2. *Cases of Cæsarian Section, with Successful Result for Mother and Child.* By T. J. Freericks and J. A. Groesbeck. (Nederl. Tijdschr, ii., Jan. 1858; and Schmidt's Jahrb., No. 8, 1858.)
3. *On Perforation and Cephalotripsy.* By Dr. Charles Hennig. (Monatsschr. f. Geburtsk., Jan. 1859.)
4. *Obstetrico-Medical History of a Woman in twelve Labours.* By Dr. Perrin. (Gazette Médicale, Jan. 1859.)
5. *A New Obstetric Forceps (called Leniceps).* By Dr. A. Mattei. (Gazette Médicale, Jan. 1859.)
6. *On the Inflammation of the Fallopian Tubes and Escape of the Purulent Secretion into the Cavity of the Abdomen, as a cause of Peritonitis in Puerperal Women.* By Dr. A. Martin. (Monatsschr. f. Geburtsk., Jan. 1859.

1. The four cases of Dr. Pagenstecher illustrate various conditions which are held in Germany to be motives for resort to the Cæsarian section.

Case I.—*Rupture of the uterus during labour, in a pelvis contracted from osteomalacia; escape of the child into the abdominal cavity; Cæsarian section.*—On the 9th November, 1857, Dr. Pagenstecher was called to a woman, aged thirty-eight. She had borne a first child three years before, and had been unable to move for four months after. During this second gestation, she had suffered from pains in the bones of the pelvis and hips, and during the last three months had been unable to walk or lie down, being reduced to sitting on the edge of the bed. During stormy and painful contractions, repeated vomiting set in, after which the child's head, which had been before felt, suddenly disappeared, and all pain ceased. Dr. Pagenstecher found the patient an hour and a half after the catastrophe much prostrated, cold at the extremities, cold sweat on the forehead, pulse imperceptible. The belly, very tender, was hanging over the symphysis; below the navel he felt a hard round lump, filling both sides of the abdomen; above this, to the left of the navel, a foot was made out. The pelvis presented a strong curvature of the lumbar vertebræ forwards, and to the right a considerable curvature of the sacrum, it being compressed from above downwards. The conjugate diameter was 2¼ inches, the symphysis was pointed to the right, and the rami of the pubes formed two nearly parallel lines, running to the tubera ischia. The tubera were scarcely two fingers'-breadth across. The os uteri could not be reached. When the abdomen was incised, the uterus was seen pale and contracted in the lower half of the wound, the right shoulder-blade of the child in the upper. The child extracted, much thick dark blood flowed from the abdomen. The incised wound yielded hardly any blood. There seems to have been no reaction. Death followed twenty-six hours after the operation. On dissection, numerous coagula were found in the peritoneal sac, but no peritoneal exudations. The uterus was tolerably thick and contracted at its fundus, and very distended and thin below. Its posterior wall was rent in an oblique direction, from the os uteri to the insertion of the right broad ligament. The fist could be passed through the wound, and onwards into the vagina through the open os uteri. The walls of the uterus at fundus were pale, but sound and thick; the lower parts, and especially around the rent, were discoloured, very soft and thin. Dr. Pagenstecher is unwilling to declare that the uterine walls were in a diseased state prior to the rupture. He thinks that possibly the long pressure of the uterus against the projecting point of the pubes, and the sharp ridge of the last lumbar vertebra, favoured the rupture.

Case II.—*Cæsarian section in contracted pelvis from osteomalacia.*—A woman, aged forty, had borne eight children, the last four dead. Labours, tedious, with forceps. Since the second pregnancy she has suffered from pains in the bones, and can only walk with difficulty. Labour began on the 2nd of December, 1857. The external conjugate diameter measured six inches. The symphysis was strongly beak-shaped, and the pubic arch, down to the tubera, so squeezed up that these were scarcely an inch apart. The pubic arch assumed the shape of a key-hole. The promontory was easily felt by the finger. The fœtal heart was distinctly heard. The Cæsarian section was performed under chloroform. The incision had to be extended upwards above the navel, on account of the rising of the bladder so high above the pelvis. The extraction of the child was rendered difficult by the extremely energetic contraction of the uterus. The placenta had to be detached from a strong adhesion to the anterior wall. This occasioned great hæmorrhage both during the operation and in the following hours, when contractile pains entirely ceased. The child lived. The after-treatment consisted in small draughts of cold water and cold compresses; one-quarter grain doses of morphia every three hours. Pains and distension of the abdomen continued for the next four days, then a discharge of thick coagulated blood. At the end of the first week a normal lochial flow. The sutures were removed on the seventh and eighth days. At the end of the second week peritoneal symptoms appeared, with vomiting and constipation, and painful distension of the belly. Small doses of opium with castor-oil, and lavements, allayed these conditions, and in four weeks' time the patient was fully restored. The child lived.

Case III.—*Cæsarian section in contraction of pelvis from rachitis.*—Primipara, aged thirty-seven; 8th March, 1858. The patient is scarcely three feet and a half high; her skull was very flat and low, with very angular parietal bones. Labour began last night; the water flowed about midnight; the child was alive. The conjugate diameter was estimated at two inches and a quarter; the pelvic outlet very narrow. The head, with large caput succedaneum, presented. The patient was narcotized, and an incision carried from the navel to two fingers'-breadth above the pubes; this was afterwards extended above the naval. The peritoneum opened, the very fat and œdematous omentum lay behind the abdominal wall. This was pushed aside, and the uterus was opened. On account of the incomplete anæsthesia and the restlessness of the patient, the protrusion of some folds of intestine could not be prevented. The left shoulder presented first; the left arm, then the right, were freed, when the breech was brought forward, and the child delivered by the legs. It was very cyanotic; it breathed, however, after a slight loss of blood and cold aspersions. The placenta lay below and to the right; it was removed without difficulty. Free bleeding of the uterus was stilled by ice. The wound was brought together, a small plug inserted at the lower angle, the belly properly supported, and an ice bladder applied for twenty-four hours. Internally, morphia and ice. The sutures were removed on the sixth day; the entire wound healed by the first intention, with the exception of a small spot. The patient was quite well in the third week, suckling her child.

Case IV.—*Cæsarian section in pelvic contraction from osteomalacia, performed for the second time on the same woman.*—On the 28th of March, 1858, a married woman, whom Dr. Pagenstecher had delivered by Cæsarian section in 1852, came again under his care. In the first year the patient had suffered frequent relapses of osteomalacia, and was now in a state of marked marasmus. With extreme muscular atrophy, the abdomen had undergone an enormous development, reaching to the knees; and its walls, thin as paper, were necrosed in several places. For months the patient could neither walk nor stand; the pelvic bones were in the highest degree painful, much thinned, and the diameter, as far as could be determined, considerably narrowed. Labour began with strong pains in the morning; at six P.M., when seen, the pains had ceased, and with them the fœtal movements. The fœtal heart could not be heard.

The incision was carried to the middle of the abdomen. The transparent uterus was exposed and opened. It had, notwithstanding the extreme thinning of its walls, maintained the compression of the pains; the membranes protruded uninjured; they were much thicker than the uterine walls. It was ascertained that the placenta was fully detached, so that the intra-uterine labour was completed. The removal of the dead child and placenta followed without trouble, and the uterus contracted into a hard fist-sized ball. The abdominal wound was united; the hæmorrhage was very violent; morphia and ice-bladder were unable to allay it. The belly became distended, so that on the next day it had attained the same size as before delivery. Vomiting set in, and death followed eighty hours after the operation.

2. Case I.—A woman, aged twenty-nine, with short and crooked legs, had been delivered in her first pregnancy of a dead premature child; in her second pregnancy, of a child at full term by craniotomy; in her third pregnancy, of a dead child in the eighth month, also by art. Her fourth pregnancy seemed at an end on the 9th of July. The sacrum was much curved, the promontory easily reached. The conjugate diameter was two inches and a half. Cæsarian section performed. The details are not related, but the patient is reported to have completely recovered.

Case II.—A primipara, aged twenty-one, was in labour. The promontory projected greatly; the upper pelvic strait was narrowed to a fissure two inches across. The abdominal incision was made a little to the side of the linea alba, on account of the lateral position of the uterus. There was little loss of blood; the uterus contracted well. Slight unpleasant symptoms were removed by morphia. In the eighth week complete recovery.

In neither case is the fate of the child recorded.

3. Dr. Hennig has made an analysis of a number of cases of cephalotripsy, with the view of comparing the results of this operation with those of perforation. The cases are arranged in three series: 1st, Those in which the mother recovered; 2ndly, Those in which the mother's life was saved, but the structures injured; 3rdly, Those in which the mother died. 64 cases are collected; of these 41 mothers recovered completely, 6 imperfectly, and 12 died. The result of the remainder is unknown. [The detailed examination of the cases is not decisive in favour of the operation. The collection, like most others of the kind, is worthless as an element of statistical comparison. Some of the operations were performed in hospitals, and the deaths might be, at least in part, ascribed to puerperal fever; and other circumstances vary, especially the indications taken by different physicians to call for the operation. Thus one case is quoted from Dr. Credé in which this physician waited three hours in order *to let the child die.* It is quite clear that no trustworthy comparison can be made between the results of operations performed on the Continent and in hospitals, and of operations performed in this country.—Rep.]

4. The obstetrical history of a woman in twelve labours, related by Dr. Perrin, is of extreme interest, as showing two points—1st, The presumed increasing pelvic contraction; 2ndly, The application of turning to delivery as a substitute for craniotomy. The first seven labours passed without remarkable difficulty; the seventh, however, required the forceps. The children were all living, and some of them showed symptoms of rickets during infancy. In the remaining five labours none of the children survived. In the eighth labour the head remained at the brim without being able to enter, from projection of the sacral promontory. The forceps failed, and turning was performed; the head was at length disengaged by the forceps. The child's heart was beating, but it never breathed. In her ninth pregnancy, the proposition to induce labour at seven months was not carried out. The feet presented; extraction was effected; but the child, born asphyxiated, did not breathe. In the tenth labour the head was lying on the brim. Turning was effected, and the head was extracted by forceps. The child's heart pulsated, but it never breathed. In her eleventh labour, by version alone, without forceps, a still child of average size was delivered. In the twelfth and last labour the head lay again at the brim; child was alive. Turning was quickly effected immediately after rupturing the membranes. The diminution of the antero-posterior diameter, caused by the projection of the promontory, arrested the entry of the head until

after long and laborious efforts of two accoucheurs, who were successively tired out. The child was very large and still-born.

It is highly probable that in this country several of the latter children would have been delivered by craniotomy. It may be said that under the method pursued the children equally died; but it may be urged that a revolting operation was avoided, and that the children had a chance of life.

5. Dr. Mattei has introduced a new form of forceps, which, from its assumed gentle action, he calls the *Leniceps*. The instrument consists of two similar blades, having the single cranial curve, which nearly accurately fits the cranium. The curve is therefore great. The blades do not lock, but fit into a bar of wood having several notches to admit the stalks at various distances. The blades and stalks are very short. Amongst the advantages claimed are that its moderate size enables the accoucheur to use it without the knowledge of the patient; that the blades being more curved, the tissues of the mother are less liable to dragging; that the head cannot be exposed to compression; that it does not require an assistant to help in the introduction. It may be observed that all these objects, except the first, which is not to be commended, are accomplished by properly-constructed forceps, which are moreover capable of effecting what the leniceps cannot do—namely, the bringing a head through a narrow pelvic brim. In the latter case some amount of compression by the forceps is necessary, and may be safely employed.

6. Dr. Martin relates five cases in support of the proposition that inflammation of the Fallopian tubes and discharge of the purulent secretion into the abdominal cavity is a cause of puerperal peritonitis. He refers to a passage in Cruveilhier's 'Anatomie Pathologique,' in which that admirable pathologist suggested this explanation of some cases of peritonitis. It is right to reproduce this passage:

"The presence of pus in the Fallopian tube being an extremely frequent phenomenon in peritonitis, I have asked myself if it were not possible that peritonitis was in some cases the result of the passage of pus from the cavity of the tube into the cavity of the peritoneum; if capillary attraction or vital suction be exerted in the act of conception upon the spermatic fluid by the Fallopian tube, might not it be exerted as well upon the pus or any other liquid contained in the cavity of the uterus?"

It is desirable to give briefly the cases of Dr. Martin, so as to expose the evidence upon which his proposition is based.

Case I.—A primipara, aged twenty-two, delivered in the Jena Lying-in Hospital on the 8th April, 1839, after a natural labour, of a strong living child. She felt a "chill" a short time afterwards. Suddenly, on the 2nd May, pain set in in the left abdomen; on the 3rd May, this was more intense, and fever was added; on the 6th, diarrhœa and delirium; on the 7th, death. The treatment consisted of twelve leeches applied on two occasions during the first days; castor-oil, opium, camphor, and ipecacuanha.

Autopsy.—A sero-purulent effusion in the lower part of the abdominal cavity; the omentum was glued to the peritoneum in front by puriform gelatinous masses. The uterus and lower intestinal convolutions were also covered with purulent exudations. The uterus was of the size of a large fist; the *left tube* was considerably enlarged in its outer third, and filled with purulent mucus; the fimbriæ were swollen. The right tube had also swollen fimbriæ, but its canal was not enlarged. The substance of the uterus was pale, normal, the inner surface reddened and covered with purple blood; there was a muco-purulent discharge at the placental seat. (It is not stated at what part of the uterus the placenta had been seated.)

Case II.—A primipara, aged thirty-three, was delivered in the Jena Lying-in Hospital on the 5th November, 1853, of a living child, after a natural labour. On the evening of the 6th, there was tenderness in the right side of the abdomen, and fever; ten leeches were applied. On the 7th, there was an offensive discharge. On the 9th, general symptoms worse; hurried breathing with bronchial secretion, diarrhœa, headache. On the 10th, death.

Autopsy.—The uterus rose above the promontory of the sacrum; its muscular structure was anæmic, containing no pus; its cavity, especially at the placental seat at the left angle, was covered with a grey-red pulpy mass, entangling shreds of vessels and plugs of blood; the cervical canal was filled with sanguinolent purulent fluid. At the abdominal extremity of both tubes was a purulent exudation, which was continued throughout the entire mucous membrane; the *right tube* was much enlarged, the mucous membrane loosened and partly covered with a yellow, purulent secretion; the right ovary was united to the posterior wall of the uterus and the rectum; the *left tube* was affected in a lesser degree.

CASE III.—A primipara, aged twenty-five, was delivered in the hospital on the 12th July, 1854, after a tedious labour, of a living girl. In the night she felt suddenly severe pain in the abdomen, which increased; the belly became distended, hot, and acutely painful on moving; ten leeches applied; calomel and opium. Death early on the 14th.

Autopsy.—The abdominal cavity held a brownish flocculent pus. The lymphatic vessels of either side along the spermatic veins were much distended; the lymphatic glands enlarged; the larger lymphatic vessels contained pus and small purulent foci. The uterus was strongly contracted; muscular substance pale; vessels empty; the placental seat was on the anterior wall, two inches above the inner os uteri; the remaining part of the inner surface was uniformly reddened, without traces of exudation or suppuration. The *tubes*, especially the *left*, were much enlarged; the vessels of the fimbriæ injected; the mucous membrane loosened, thick, and covered at the outer end with a creamy secretion. On the *left ovary* and tube the veins were distended, and the lymphatic vessels were filled with whitish fluid.

CASE IV.—A primipara, aged twenty-five, was delivered in the Jena Hospital on the 19th November, 1857, of a living boy. In the afternoon of the following day, she had pain in the abdomen, and took ten grains of calomel, and on the 21st, ten grains more. Several stools followed the last dose. On the 22nd, the symptoms had remitted. On the 23rd, however, when seemingly quite well, she got out of bed, and being surprised, leaped suddenly back again. Shortly after this, shivering and acute pain in the abdomen came on. On the 24th, the pain was especially severe in the *right side.* Twelve leeches applied. On the 25th, the abdomen was distended; everything was worse; twelve more leeches applied. Bronchitis, tympanitis appeared, and death ensued on the 27th.

Autopsy.—A large quantity of bloody purulent exudation flowed from the abdomen when opened. The omentum, where glued to the *right iliac* fossa, was inflamed. The ovaries were covered with exudation; *both tubes* at their outer extremities much distended with purulent contents. The inner surface of uterus showed remains of endometritis.

CASE V.—A primipara, aged twenty-nine, was delivered in the Jena Hospital of a living boy—labour natural—on the 26th May, 1858. She was quite well for the first week, excepting that the lochial discharge was profuse and offensive. On the 3rd June the abdomen was painful, and she had three stools. During the 4th and 5th, the diarrhœa continued; she took an infusion of ipecacuanha with acetate of ammonia. On the 6th, the diarrhœa had ceased, and the patient got up to dress; while stooping for this purpose, she was *suddenly seized with an acute pain* in the abdomen, which increased from hour to hour; the severest pain was in *the right side.* It was concluded that this sudden pain was caused by the escape of pus from the Fallopian tube, brought about by the sudden compression of the abdomen in stooping. Ten leeches applied. Tympanitis, delirium, and collapse followed, and death on the 10th.

Autopsy.—A considerable quantity of purulent exudation was found, *especially in the right half of the abdomen*, and the principal focus was seated in the *right iliac fossa.* There was pus in the cavity of the uterus, but no pus in the vessels or muscular wall. The *right tube* was much dilated, and contained a considerable collection of pus. The left tube was quite normal.

Dr. Martin insists upon the necessity of keeping women who exhibit any symptoms of metritis perfectly quiescent, so as to favour one of the terminations of tubal inflammation, which is closure of the fimbriated extremity. Professor Virchow, in some observations upon this paper, said it was a very difficult matter to determine the starting-point of a peritonitis. Every inflammation of the abdomen, no matter how arising, had by the law of gravity a tendency to involve the pelvic organs. Thus in perforation of the stomach or processus vermiformis, inflammations of the tubes and ovaries occurred as much as in primitive disease of these organs. The clearing-up of this question did not rest with anatomy, but with clinical observation.

(The Reporter would remark that all the cases occurred in hospital, and that no history is given of the sanitary condition of the institution. If puerperal fever—especially that form arising from hospital air—be the consequence of a blood-poison, the theory here set forth of a fatal peritonitis caused by the accidental escape of a little pus from a localized inflammation of the Fallopian tube, does not appear sufficiently proved.)

THERAPEUTICAL RECORD.

On the Mode of Preparation and Preservation of Normal Hydrocyanic Acid.[*]—M. Dannecy, of Bordeaux, has adopted the plan proposed by Everett, for obtaining hydrocyanic acid, which consists in decomposing a quantity of cyanide of silver by its equivalent proportion of hydrochloric acid diluted with water. The formula which M. Dannecy employs is founded upon that of Everett, and it has the advantages of being easy and very rapid in its application, and of furnishing a perfectly pure acid with proportions which are always exactly alike.

On the Best Form to be given to certain Pharmaceutical Preparations intended for External Use.[†]—In an article in the 'Bulletin Général de Thérapeutique,' it is remarked that greasy applications to the skin do not possess in general, any great therapeutical efficacy, and it is therefore suggested that they might be advantageously replaced in many instances by saponaceous compounds. In studying the action of the latter, M. Deschamps composed a soap with iodide of potassium, and rubbed it in four times upon his epigastrium. He analysed his urine in the intervals of the frictions, and found it to contain appreciable quantities of iodine. He afterwards washed with water the part which had been rubbed with the soap, and he found in the liquid some fatty acids arising from the decomposition of the soap by the fatty acids secreted by the skin, together with iodide of potassium in solution. These washings were continued for five days successively, and every day some iodine and fatty acids were obtained. These facts prove that the skin is easily penetrated by saponaceous compounds; that the skin rejects a part of the principles which it absorbs; and that therapeutical agents may pass through the skin, diffuse themselves in the system, produce physiological effects, and be expelled by the ordinary passages. M. Deschamps has therefore proposed to employ certain saponaceous compounds, instead of liniments and pomades, in cases where the surface of the body, where these preparations are to be applied, is not denuded. These soaps contain respectively iodide of potassium, laudanum, ammonia and laudanum, extract of belladonna, sulphuret of sodium, and digitalis, combined with an alcoholic solution of soap.

Adulteration of Valerianate of Iron.[‡]—The valerianates are often adulterated, the adulteration consisting of the mixture of some salt with a certain quantity of essential oil of valerian. M. Monnerat has given some easy methods of detecting these adulterations. He found that the false valerianate had a deeper colour than the true, and that it was insoluble in alcohol and ether, and that, when treated with boiling water, it gave after cooling, a deposit of subcarbonate of iron and a considerable quantity of essential oil of valerian floating upon the surface of the liquid. Besides, the true valerianate of iron is insoluble in water, but on the contrary, is entirely soluble in alcohol. Another character of the true valerianate is its acid, disagreeable, persistent smell, which is very different from the penetrating odour of valerian presented by the false valerianates formed by the addition of the essential oil of the plant.

Anodyne Liniment in Otitis.[§]—M. Trousseau recommends the following liniment in acute otitis, namely a mixture of the alcoholic extract of belladonna in water, with glycerine. A cotton ball, soaked in the mixture, to be placed in the external auditory canal.

A Homely Substitute for Cod-liver Oil.[‖]—Mr. McSherry, of Baltimore, relates the case of a patient who was suffering from phthisis, and for whom cod-liver oil was prescribed; after some time he again came under notice very much improved in health, and it was ascertained that he had made use of *dog fat*, which remedy he had tried at the suggestion of some of his friends. On auscultation it was discovered, that there was a cavity in the lungs with its walls cicatrized. Mr. McSherry does not assert that dog-fat has any virtues superior to other animal fats, but he states that this inelegant remedy appears with certain classes in America to be both popular and successful.

Formula for a New Elixir of Pepsine.[¶]—The syrup of pepsine, recommended by M. Corvisart, not being capable of preservation for a long time, and the elixir of Garus having a disagreeable taste, M. Mailhe has invented a formula of pepsine associated with wine, alcohol, and sugar, in sufficient quantity to conceal the peculiar taste of the ferment. The proportions are six grammes (about ʒ iss.) of amylaceous pepsine, twenty-four grammes of distilled water, fifty-four grammes of white wine of Lunel, thirty grammes of white sugar, and twelve grammes of spirits of wine. This elixir has a very agreeable taste, and women and children can take it with pleasure. It is administered immediately after each meal, in the dose of a tablespoonful, containing exactly the quantity of pepsin necessary for digestion, namely, one gramme (about fifteen grains).

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[*] Bulletin Générale de Thérapeutique, May 15th, 1858.

[†] Ibid.

[‡] Ibid , May 30th, 1858.

[§] Ibid., Oct. 15th, 1858.

[‖] American Journal of the Medical Sciences, Oct. 1858.

[¶] L'Union Medicale, June 19th, 1858.

Ready Method of Recognising a Mixture of Citric and Tartaric Acids.[*]—M. Barbet recommends that a slight layer of caustic potash should be laid upon a plate of glass placed horizontally, and that then a part of the mixture of the doubtful crystals should be thrown upon it. At the end of a few seconds the crystals belonging to the tartaric acid become white, and even quite opaque, becoming covered with little microscopical crystals of bitartrate of potash, while the fragments of citric acid remain diaphanous, being partially dissolved in the alkaline liquid. The difference is so well marked, that the relative quantity of both acids may be very accurately determined.

A New Mode of Preparing Hydrate of Magnesia as an Antidote to Arsenious Acid.[†]—The hydrate of magnesia having been proved to be superior to peroxide of iron as an antidote to arsenious acid, M. Guérin has proposed a new plan of preparing the former, which consists in substituting ammonia for potash in the precipitation of the magnesia. The sulphate of magnesia is the salt employed. The hydrate of magnesia may be prepared extemporaneously by rapidly dissolving a considerable quantity of sulphate of magnesia in common water, and adding to it some caustic ammonia, until the ammoniacal smell is perceptible; then the whole is to be filtered, the hydrate of magnesia collected, and administered in a state of suspension in water. The superiority of this mode of preparing hydrate of magnesia is said to consist in the entire absence of potash in the preparation; for in the treatment of arsenic poisoning, if any portion of potash should be present, an arsenite of potash would be formed, and the risk of the patient would be increased instead of being diminished.

Ethereal Oil of Horse-Chestnut as a Local Remedy in Gout and Rheumatism.[‡]—The bark of the horse-chestnut, from its containing a bitter principle, forms a tonic medicine which has been classed among the substitutes for quinine; the roasted pulp has been recommended in atonic uterine hæmorrhages, and the ethereal oil has been indicated as a topical agent at the commencement of gouty and rheumatic attacks. This oil is obtained from the powder of the horse-chestnut by treating it with ether, which takes up the oil, and afterwards evaporating the ether. This oil is employed as a mild innunction on the inflamed part, and when there is great sensibility, the innunction is employed in a circular course round the inflamed part, so as to arrive gradually at the centre. Dr. Masson, who has made numerous experiments with innunctions of the ethereal oil of horse-chestnut as a method of arresting the local symptoms of gout, states that he has remarked an exasperation of the pain during the first half-hour following the application, but that after this period there was manifest relief.

Formula for the Combination of Nitrate of Bismuth with Copaiba and Cubebs.[§]—According to the observations of M. Caby, the nitrate of bismuth combined with balsam of copaiba and powdered cubebs, possesses the property of neutralizing the irritating effects produced habitually by these medicines on the digestive canal. The formula which is employed at the Hospital of St. Lazare is a mixture of equal parts by weight of balsam of copaiba, powdered cubebs, and nitrate of bismuth, with some essence of peppermint as a flavouring ingredient. This combination is said to be supported easily by the most delicate stomachs; there is no excitement, epigastric heat, or diarrhœa, so that the action of the medicines being entirely concentrated upon the genito-urinary passages, the desired results are more rapidly and easily obtained.

On an Inexpensive Mode of Administering the Protiodide of Iron.[‖]—The protoxide of iron has been given in the form of pills, syrup, or oil. The first two furnish good preparations, but they are expensive, and as the treatment with iodide of iron often requires to be long continued, the poorer classes of patients are sometimes obliged to abandon its use. The mode of administration recommended by a Belgian pharmaceutist is by preparing a solution of iodide of iron in such proportions that one or two drops represent five centigrammes (about one grain) of the iron salt. This is preserved in a stoppered bottle, and some iron filings are added, in order to preserve it from decomposition. When the solution is to be used, one or two drops of it are poured either upon a piece of sugar or in a spoonful of water, beer, or gruel.

On the Combination of Iodine and Sulphur by the Medium of Oil.[¶]—The combination of iodine and sulphur having very little stability, M. Vezu, a pharmaceutist of Lyons, has proposed to combine the iodine and the sulphur by dissolving them each separately, beforehand, in oil of sweet almonds. The sulphur is heated in the oil until it is dissolved, but the iodine is dissolved in the oil while cold. The resulting compound has the taste and smell of heated oil, and has a chestnut colour.

[*] Bulletin Général de Thérapeutique, Sept. 30th, 1858.

[†] Ibid., Aug. 30th, 1858.

[‡] Ibid., Sept. 15th, 1858.

[§] Ibid., Sept. 15th, 1858.

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